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# OF
# SCIENCE AND ARTS.

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### SCIENTIFIC INTELLIGENCE.

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AMERICAN

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[THIRD SERIES.]

## ART. I.—*Notices of Recent Earthquakes*; by Prof. C. G. ROCKWOOD, Jr., Bowdoin College.

1. ON January 16, 1872, an earthquake almost entirely destroyed the city of Shamaka in Russia. Over one hundred persons are reported to have been killed and a large amount of property destroyed, scarcely a building having been left standing in the city. The earthquake was felt over a large extent of the surrounding country. Shamaka is a city of 25,000 inhabitants, lying at the southern base of the Caucasus Mountains, and about 75 miles west of the Caspian Sea.

2. On February 6, at 8 o'clock A. M., a slight shock of earthquake was felt at Wenona, Mich. A letter from Ed. D. Cowles of that place, states :—"The shocks were three in number and lasted altogether about thirty seconds, the vibrations travelling from the N.N.E. They jarred buildings and were plainly observable by persons out of doors, and were characterized by that peculiar rumbling sound which is noticed in subterranean vibrations."

3. On February 8, at about 5 A. M., a slight earthquake occurred at Cairo, Ill. A letter (in my possession) from Geo. Fisher of Cairo to Clinton L. Conkling of Springfield, Ill., gives the following :—"I was in bed on the second floor of a brick dwelling house. It seemed to me that something struck the head of my bed with considerable violence from the southeast, making quite a noise and shaking the entire house. The shaking continued for several seconds with varying intensity. I suppose that fully twenty seconds elapsed before it finally ceased. Persons who were up at the time seem to think that

the vibrations were from N.W. to S.E. I think they were the other way, from S.E. to N.W. No damage was done by the shock."

4. On March 6, a despatch from Berlin, Prussia, says:—"Shocks of earthquake were felt this afternoon simultaneously in Dresden, Pirna, Schandau, Chemnitz, Bodenbach, Weimar and Rudolstadt. The movement was not violent, but was more or less perceptible at intervals for over an hour."

5. On March 26, the State of California was visited by an earthquake more severe than any that has occurred there for some years.

The main shock occurred at about $2^h$ $25^m$ A. M., and was felt throughout the length of the State, from Red Bluff on the north, to San Pedro on the south, thus extending over 6½ degrees of latitude, and from the Pacific coast inland to Virginia City, Nev.

The *time* of this shock is variously reported from $2^h$ $10^m$ at Jackson to $2^h$ $45^m$ at White Pine, Nev. The discrepancies are doubtless due partly to difference of longitude, and partly to the irregularity of watches. The most probable time seems to be that given above, which is the time reported from Visalia and Independence, where the shock was heaviest.

The *duration* of this first shock was estimated by different observers at from 2 or 3 seconds to 3 minutes. The general estimate was about one minute.

The *region shaken* was the eastern and western slopes of the Sierra Nevada, and the Sacramento, San Joaquin and Tulare valleys, extending southward even into Mexico. (A shock was reported in the City of Mexico, and in several of the Mexican States, on the same day, and presumed to be synchronous with the one in California, although the exact time was not given.) The shock was most violent, and caused the greatest damage in the Owen's river valley, Inyo Co., which is situated east of the Sierra Nevada, near to its southern extremity, and in a region covered with tokens of former volcanic action. In other places no lives were lost, and not much property destroyed.

There is great discrepancy in the reported direction of the vibrations, but in most places they appear to have been transverse to the mountain system, or in a general N.E. and S.W. direction. In Lone Pine the fallen houses seemed to have been pushed toward the N.E.

In many places the first heavy shock was followed by a series of lesser ones, closing with a stronger one at a few minutes after six A. M. And in the neighborhood of the mountains the slight shocks continued to be felt at intervals for some days or even weeks. Thus a letter from Visalia, dated April 12, says:—"Ever since the first shock of the earthquake we have had from day to day slight shocks,—at least from three to five per day,—not sufficient to do any damage, but still somewhat exciting.*

The village of Lone Pine, eighteen miles south of Independence, in the Owen's river valley, and a place of some 500 inhabitants, was greatly damaged by the earthquake, and here was almost the only loss of life. Over fifty adobe houses were shaken into ruins, and in their fall twenty-seven persons were killed and thirty-four others seriously injured. Frame houses were shaken, but not thrown down. At Independence also many buildings were prostrated and a few lives lost.

In this valley, and at some other places, the shocks were preceded and accompanied by a loud rumbling sound, which is described as being "like a train of cars or like distant artillery."

Mention was made in the first accounts of large fissures in the ground, fires seen in the mountains, etc.; but these reports do not seem to be confirmed by the later advices. The level of Owen's lake is also said to have risen four feet.

That the shock extended under the ocean is proved by the schooner *Beal*, which was becalmed in the straits off San Pedro, and was so much injured that she made the port with difficulty. It is also worthy of mention that the volcano of Colima in Mexico was reported in violent eruption and throwing out clouds of ashes on April 4, a few days after the earthquake.

The above is gathered mostly from the numerous accounts published in the newspapers of San Francisco; and for aid in collecting them, my thanks are due to C. G. Rockwood, Esq., of Newark, N. J., J. C. Smith, Esq., secretary of the Merchants' Exchange and News Association, New York, Rev. D. W. Poor, D.D., of Oakland, Cal., and John A. Keyes, postmaster at Visalia, Cal.

It is to be hoped that more full and careful scientific accounts of the physical phenomena may have been collected by some person on the spot, and that they may in due time be given to the public.

6. A slight shock was reported at Paducah, Ky., on the morning of March 26, and another at Salt Lake City, Utah, at

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* The following later news has appeared in the columns of the San Francisco Bulletin:

"*Lone Pine*, May 17, 1872.—We had such a shake to-day as we have not had since the 26th of March, when the town was reduced to ruins. There has been no cessation of the shocks, in fact, ever since. The only difference is that sometimes they are quite heavy, and again comparatively light. A remarkable fact, however, is that the shocks are no longer long, and like a sea swell, but are short, sharp, and appear to describe a circle. I was sitting in the parlor of my house to-day when the shock came, about one o'clock. It had been preceded by several lighter agitations, accompanied by the inevitable rumbling sound. Every timber in the house shook, the furniture in the room was violently disturbed, and I was actually thrown from my seat. The shock lasted some thirty seconds."

1 P. M., March 28. These may have been part of the Inyo earthquake.

7. On April 3, at 8 o'clock A. M., a severe earthquake destroyed a large part of the ancient city of Antioch in Syria. The shock lasted over 40 seconds, and the wave travelled from east to west. It was accompanied by a noise "like distant thunder or artillery." Lighter shocks continued to be felt at irregular intervals for at least a week after the first one. Very many buildings were shaken down, filling the narrow streets with the débris and burying hundreds of the inhabitants beneath the ruins. The number of killed is estimated at 1,000 to 1,600 and many more were left without shelter. The old Roman bridge of four arches is rent in several places. The villages south of the Orontes river were also much injured; but comparatively little harm was done north of the city.

8. A despatch from Copenhagen, May 14, gives the following:—"A schooner, which arrived here to-day from Iceland, reports a series of violent earthquake shocks at Hasvick on the 16th, 17th and 18th of April. Twenty houses were destroyed, and several persons were injured, but no lives were lost."

9. The recent grand eruption of Mt. Vesuvius is interesting, as being possibly connected with the phenomena recorded above. This eruption first assumed noticeable proportions on the night of April 24, 1872, when a flow of lava was added to the flames and smoke which had for months adorned the summit of the mountain. On the night of the 25th, a chasm opened in the side of the cone, from which issued a torrent of lava; the whole occurring so suddenly as to overtake and destroy a number of the spectators who were watching the eruption. The flow of lava continued two or three days, overwhelmed two villages, and buried a considerable extent of cultivated land. The eruption finally ended with a shower of stones and volcanic sand, which fell in the streets of Naples to the depth of several inches. The eruption was attended with the usual local tremblings of the earth.

Brunswick, Me., May 31, 1872.

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## ART. II.—*Contributions from the Physical Laboratory of Harvard College;* No. III. *On the Electrical Condition of Gas Flames;* by JOHN TROWBRIDGE, Assistant Professor of Physics.

PROF. H. BUFF, of the University of Giessen, has published in the Annalen der Chemie und Pharmacie, vol. lxxx, 1, and in the Phil. Mag. of Feb., 1852, an investigation of the electrical properties of flames. He reviews at first the different theories in regard to the subject; Becquerel, for instance, finds electric opposition in all directions in flames which depend upon the difference of the temperature of the metals immersed in them. Pouillet recognizes a motion of electricity only from the interior to the exterior, and hence also from the base to the summit of the flame; Hankel, however, finds a motion the reverse of this in the flames produced by the ignition of spirits, and states that it is independent of the temperature of the immersed conductor.

Prof. Buff then gives the following as the results of his investigation:

1. Gaseous bodies which have been rendered conductible by strong heating are capable of exciting other conductors, solid as well as gaseous, electrically.

2. When a thermo-electric circuit is formed of air, hydrogen or carburetted hydrogen, alcohol vapor, charcoal, or finally a metal, whether combustible or incombustible, an electric current is developed, which proceeds through the air from the hottest place of contact to the less warm place.

3. The development of electricity which has been observed in processes of combustion, and particularly in flames, is due to thermo-electric excitation, and stands in no immediate connection with the chemical process.

4. The products of combustion do not therefore, by any means, occupy the relation to the burning body which has been assumed by Pouillet; if positive electricity rises with the ascending gases, it is only in the degree in which the air exterior to the place of hottest contact is connected by a proper conductor.

The following are the results which I have obtained in testing the electrical condition of the flame of a Bunsen burner with a Sir William Thomson's quadrant electrometer. The degrees given refer to the arbitrary divisions of the scale, upon which a spot of light is reflected from the mirror of the instrument.

Upon connecting the testing plate of one pair of quadrants of the instrument with the flame, while the other pair were connected with the metallic burner and with the earth, the flame was found to be negatively electrified.

The following are some of the experiments selected from a series that were made.

Exp. 1. Flame 12 c. m. high; plate at the height of 7 c. m. A negative indication of 130°, very steady.

Exp. 2. A platinum wire, substituted for the plate, and meeting the flame 3 c. m. above the burner, gave a deflection of 30° in a negative direction.

Exp. 3. With the testing plate just above the tip of the flame, the instrument showed a positive deflection of 70 to 80 degrees.

Exp. 4. With the testing plate 5 mm. from the outer surface of the flame, on all sides, a feeble positive charge was obtained, the air in contact with the flame being apparently charged positively, the indication in no case exceeding fifty or sixty degrees on the scale of the electrometer.

Exp. 5. The metallic tip of the burner was found to be charged positively, giving an indication closely agreeing in the number of degrees with that corresponding to the negative indication of the flame. This indication was quite constant.

Exp. 6. When a glass tip was substituted for the metallic tip, no charge was found upon it. This was the case when any non-conducting body formed the tip.

Exp. 7. A glass tip having been substituted for the metallic one, a platinum wire was inserted below the orifice and carefully pushed upward until it occupied the centre of the interior cone of flame. A very feeble indication of negative electricity was the result.

While, with the Bunsen burner, the flame and the metallic tip are in decided electrical opposition; the one having a negative charge and the other a nearly equal positive charge; in spirit flames the two opposite states recombine, the wick of the lamp and the fluid contained in the vessel connecting the two charges. The flame, therefore, merely takes the potential of the atmospheric electricity at the place where it is situated.

The electrical condition of the flame of a Bunsen burner when tested by a sensitive galvanometer gives in the main the same results as those obtained by Prof. Buff from spirit flames. The quantity of electricity in the current passing from the flame to the tip is exceedingly small; whereas we have seen above that the terminal immersed in the gas flame has a tension a little exceeding that of the negative pole of a Daniell's element.

The air in the room, at the time the above experiments were first performed, was charged positively to about the tension of the positive pole of twelve Daniell's elements. The experiments were afterward repeated when the air in the room indicated a negative charge, with no difference in the results.

At the suggestion of Dr. Wolcott Gibbs, Rumford professor, I tried the above experiments with a Bunsen blast lamp, by means of which I could increase the heat and the flow of air and gas at pleasure. Slight deviations in the scale readings were obtained by this means: the flow of air appeared to affect the charge of electricity upon the metallic tip, rendering it less constant. The above experiments were in the main confirmed. The nature of the metallic plate submitted to the flame and the degree to which it was heated appeared to have a very slight influence upon the charge.

Sir William Thomson, in the proceedings of the Literary and Philosophical Society of Manchester, March, 1862, has a paper upon the electricity of the air in rooms. He finds that it is generally negative. By placing a spirit lamp upon the prime conductor of an electrical machine, he was enabled to change the tension of the air from a positive to a negative state and the reverse. He carefully separates the results obtained from the *idioelectric* effect of the flame, which, he states, in no case gave a tension equal to either pole of a Daniell's element.

During the past winter observations made in the laboratory tend to confirm these views. I have, however, found on some days the air within strongly positive. The room is in the north-west corner of the building, and there was a strong north-west wind blowing at the times this was observed. I noticed, also, while experimenting with the flame of a Bunsen burner placed near the water dropper used by Sir William Thomson in investigating the electrical state of the atmosphere, that the positive charge of the air in the neighborhood greatly decreased, and in some instances became feebly negative, by the presence of the flame.

The popular idea that great fires are followed by a change in the atmosphere inducing rain, does not seem to be unwarranted from an electrical point of view. The electricity of the air during cloudy and rainy weather is generally negative or at the most feebly positive. The flames being negative would tend to change a strong positive charge in clear weather to a negative one, and thus bring the air to the state noticed in cloudy and rainy weather.

The following are the conclusions to which the above experiments lead:

1. The flame of a Bunson burner is negative while positive electricity accumulates on the burner itself, if it is a good conductor. With orifices made of non-conductors, no charge was found upon the tip.
2. The stratum of air in contact with the outer cone of flame is slightly charged with positive electricity. The partly consumed gas of the interior cone is neutral.
3. The presence of flames tends to change the nature of the atmospheric electricity at the given place, reducing a positive tension to a feebly negative one.

## ART. III.—*On Molecular and Cosmical Physics;* by Prof. W. A. NORTON.

[Concluded.]

13. If we include in the curve of effective molecular action the external electric attraction which we have seen may arise within the sphere of what has been called the effective external repulsion, it will be seen that the curve beyond *Oc* should be raised; and that when an effective attraction supervenes, and determines a chemical union, it will pass entirely above the zero line, from *c* to a certain distance beyond *Od*. But instead of this the result may be that it passes above the zero line for a certain distance, beginning at a point, say *a′*, beyond *c*, or even *d*. That is, an effective external repulsion may be in operation over a distance *ca′*, and be succeeded by an *external attraction* for a certain distance beyond *a′*; and accordingly *a′* will be a second point of stable equilibrium. Such an external effective attraction, succeeded at less distances, down to *c*, by a repulsion, is apparently the determining cause of all phenomena that result from bodies of matter coming into contact, which do not terminate in a chemical union. As this attraction operates through a certain distance before the equilibrium is established, more or less heat is generally given out.

We recognize it (1) as the force of *adhesive attraction* between solids and liquids (which experiments have shown to be very feeble in comparison with the cohesive attraction that constitutes the general force of tenacity of the molecules of the liquid), of which capillary attraction is one mode of manifestation: (2) as the similar force by which gases are compressed on the surfaces of solids and liquids, or into their pores, with the evolution of heat: (3) as the operative cause of the diverse phenomena of *liquid* and *gaseous diffusion*. To understand how it is capable of producing such effects, we have only to reflect that at the surface of contact of two liquids, or gases, the force in question should come into operation between the dissimilar molecules posited on opposite sides of the surface, and that the result must be that the molecular repulsion exerted by the similar molecules of either liquid or gas, on one side of the surface, will urge those lying near it to the opposite side. This should happen, in the case of a gas, even though the force under consideration should not amount to an effective attraction, but should only diminish the repulsion between the dissimilar molecules. In this fact we see the reason why all gases interpenetrate each other. The diffusion should not stop until the two gases become a uniform mixture, since as long as an ideal plane can be taken within the gaseous mass, on opposite sides of which the number of molecules of the two kinds is not the same, the repulsion that takes effect across this plane will be of a less intensity than that which is directed toward it.

Since the force of electric attraction that comes into play between two dissimilar gaseous molecules, lying on opposite sides of the surface of contact, is of the same intensity for each molecule, and the molecular repulsion of each gas is the same, the velocity of diffusion of each should be inversely proportional to the square root of the density; which is the fundamental law of gaseous diffusion established by Professor Graham. In the case of two liquids the result is not precisely the same, since the molecular repulsion may be of different intensities for the two liquids, and hence the moving forces of the two dissimilar molecules may be unequal. This repulsion is generally least for the more viscid liquid. The general result, however, is, as in the case of a gas, that the fluid whose specific gravity is the least, penetrates most rapidly into the other.

It may be added that the external attraction under consideration is also the force which gives to liquids their solvent power.

14. When two substances combine in several different proportions, the force of affinity is ordinarily weaker in proportion as the number of primitive molecules ("atoms") of the one that combine with one of the other is greater. This may be ascribed to the circumstance that each new combination withdraws a portion of the electric ether from the sides of the molecules on which union has previously taken place, and occasions a collapse of the envelopes there. The result of this should generally be that the molecular attraction subsisting there is weakened, and that developed at the new point is less than that before existing. The point of saturation should be reached when the diminishing attractions have become so feeble that the heat developed by a new combination suffices to detach one of the atoms already combined; or when the decreasing force of external attraction, that comes into operation at the successive combinations, is not sufficient to predominate over the external repulsion.

There should be a tendency, in any case of a chemical union of the combining molecules, to modify the electric condition of those in their vicinity, by a propagation of the disturbance of the envelopes. In view of this tendency we may see how it is that a minute change in the proportion of one of the constituents may effect a marked change in the degree of tenacity, hardness, &c., of the compound (e. g., different qualities of steel resulting from small differences in the quantity of carbon that is combined with the iron). The phenomena of fermentation may be referred to the same cause.

15. All the mechanical properties of bodies may be ascribed to varied values of the quantities $\frac{n}{m}$, $m$, and $r^2$ (vol. iii, p. 338), and to the variations that may occur in these values under different circumstances. These quantities must depend primarily upon the mass and size of the atoms around which the ethereal atmospheres and electric envelopes are condensed. The marked difference in the properties of certain substances which have nearly the same atomic weight, indicates that atoms of the same mass may differ in size.

It is to be observed that the same substance may assume various states of aggregation of its primitive molecules, in which it exhibits different properties; under varied thermal or other circumstances of solidification, giving rise to modifications of the molecular envelopes. One general result may be noted, viz., that an aggregation of compound molecules should have less tenacity than one of primitive molecules of the same substance; since in the latter case no two molecules can be drawn asunder without exerting a force equal in intensity to their maximum effective attraction; but if the primitive molecules are arranged in groups, instead of one homogeneous mass, there must be a number of the constituents of each group separated from those of the contiguous one by a distance greater than the natural distance between two primitive molecules in equilibrium. Diamond and charcoal may be cited as examples of the two states of aggregation under consideration.

16. When the molecules of a solid, or liquid, are in a state of equilibrium, that portion of the heat-pulses which acts as a molecular repulsion is insensible heat; since all the repulsive impulses that proceed from one primitive molecule and take effect on another, are there neutralized by equal attractive impulses. Whenever, in the disturbance of the equilibrium of two contiguous molecules, heat is given out, a certain part of these insensible impulses passes into the sensible state; that is, are propagated indefinitely as unneutralized heat-pulses. At the same time a change generally occurs in the physical condition of the molecules; ordinarily their envelopes collapse.

17. Heat may originate in several different ways. (1) When molecules are forced, or in any way made to come into closer proximity, in opposition to an effective repulsion, the entire amount of the repulsive impulses that operate as a retarding force, shows itself as sensible heat. Heat originates in this manner in the act of chemical union, in which a certain amount of effective molecular attraction is expended in urging the molecules toward each other, and an equal amount of effective repulsion destroys the motion, and in so doing originates ethereal waves of heat proceeding outward from the

molecules, which convey impulses of the same intensity as those expended in arresting the molecular movements. Heat is developed in this way when bodies are compressed by pressure, or impact. The heat of friction also originates in this manner; for between the molecules of the two rubbing surfaces the force of external repulsion is in operation, and certain pairs of them are being incessantly urged nearer to each other on the line of movement. The increasing repulsion thus developed constitutes the force of friction, and the resisting work done by it develops an equivalent heat-energy. It may seem that the continued operation of the same repulsive force, after the approaching particles have passed each other, should counteract the effects resulting from their approach; but it is to be observed with regard to the effect of this circumstance, that any work thus done by the molecular heat-repulsion operating as an accelerating force will be given out again, by reason of the subsidence of the motion produced by it, and therefore whatever heat-energy is expended in such motion is afterward restored. Besides, the repulsion in operation between two surface molecules, in the act of separating after having been forced into close proximity, will in general be more inclined to the line of movement than the repulsion exerted between the same molecules during their approach. This inequality of inclination should obviously increase with the pressure.

(2.) Heat may also be developed by reason of a collapse of molecular envelopes, resulting from a change in the distance between contiguous molecules. For example, all substances for which the ratio $\frac{n}{m}$ exceeds 6·9, will, when compressed, give out heat in this manner; since the mutual attraction of their envelopes will diminish. We have seen that the same state of things occurs with india-rubber when it is stretched. In fact, it appears from the table on page 444 (vol. iii), that all bodies of matter for which the ratio $\frac{n}{m}$ is less than 6·9 are in this respect in the same condition with india-rubber. There are certain theoretical reasons for believing that all liquids belong to this class. Upon this view the heat of congelation may be ascribed to a collapse of the molecular envelopes resulting from the expansion of the mass that ensues as the compressive force at the surface of the liquid ceases to operate. Except in the case of water, the molecules of the liquid being in what has been termed the secondary condition, this collapse of the envelopes will be attended with an augmentation of their attractive actions and a consequent attraction of the mass; but this augmentation will increase the ratio $\frac{n}{m}$, and so tend to make the expansion of the envelopes that should result from the contraction of the mass less than the previous contraction. Besides, the operation of the increased attraction should tend directly to develope sensible heat-pulses for the reason given above.

(3.) The evolution of heat may also result from the action of an electric current,—either compressing the molecular envelopes, and thereby causing heat-pulses to proceed from the ethereal atmospheres, or originating waves in the interstitial luminiferous ether, the energy of which is subsequently absorbed by the molecular envelopes, to be radiated off in heat-pulses.

18. All the diverse effective forces in operation, in or upon bodies, may act as *statical forces*, or *dynamical forces.* In the first case the impulses that fall upon the central atoms of the primitive molecules are reflected off again, either in electric or ethereal waves; and no transformation of motion from ethereal to ordinary matter occurs. But when motion ensues a certain amount of ethereal wave-force is expended in the production of such motion, and is transformed into the equivalent dynamic energy of the moving mass. This motion can only be counteracted, and the body brought to rest, by the exertion, either directly or indirectly, of an amount of impulsive wave-force equal to that which originated the motion; in which event the energy that had passed into the body is restored to the ether, either luminiferous or electric—ordinarily the former in the form of heat-waves, since the repulsion that operates to bring an impinging body to rest is usually the molecular heat-repulsion taking effect at the surface, and in the interior of the body

The dynamical energy of any moving mass, of either of the three varieties of matter, represents the previous expenditure of a certain amount of ethereal wave-force, and can disappear only by being transformed into an equivalent amount of energy of one of the other two varieties of matter. Thus radiant heat is a dynamical energy that has resulted from the expenditure of a certain amount of the molecular heat-repulsion before noticed; and may be transformed into an equivalent amount of energy of ordinary matter, or of electric ether.

19. Heat is everywhere in a state of perpetual flux,—both while it retains the condition of ethereal wave-force and while in the act of transformation into an equivalent energy of either of the other two forms of matter; and each transformation ordinarily soon gives place to another, and so never ending cycles of transformation are passed through.

When the pulses of radiant heat fall upon ordinary molecules one portion of the wave-force is transmitted, another reflected, and a third absorbed. *Absorption* is of two kinds, *general* and *elective.* That portion of heat-energy which experiences general absorption, is consumed in urging the molecular envelopes farther from the central nuclei, while the other is expended in imparting vibratory movements to the atoms and layers of the envelopes, which naturally vibrate in unison with especial heat-waves. *Radiation* is the necessary correlative of absorption. For the recess of the envelopes attendant upon absorption is succeeded by an equal collapse that originates pulses of radiant heat equal in amount to those absorbed; and the special vibratory movements induced subside again by being communicated to the luminiferous ether. The collapse of an envelope is attended with vibratory movements of its individual atoms both toward and from the central nucleus, and at right angles to this line (varying in rate with the position of the atom in the envelope), which originate ethereal waves of diverse rates of undulation.

Heat is *conducted*, by good conductors, chiefly by waves of the electric ether passing over from one molecular envelope to another; and hence the same physical conditions which favor the conduction of heat should also favor the conduction of electricity. Heat may also be slowly and imperfectly conducted by successive radiations from molecule to molecule through the luminiferous ether, with attendant absorptions by the molecular envelopes; when the density of the interstitial electric ether is too feeble to admit of direct conduction.

20. What is called the interior potential energy of a body of matter, in the mechanical theory of heat, is the mechanical or equivalent thermal energy capable of being expended, or given out, in a contraction of the mass after expansion, or in the condensation of simple into compound molecules, or in the collapse of expanded molecular envelopes. When the expansion of envelopes has resulted immediately from an increased attraction between contiguous molecular envelopes, superinduced by an extraneous action (mechanical or thermal), the potential heat-energy absorbed will be equivalent to the attractive energy expended. The work done, if any, in this case by the extraneous action in opposition to molecular resistances is altogether distinct from this incidental effect. The mechanical stretching of wires, and liquefaction, with the attendant absorption of heat, may be cited as illustrative examples.

The hypothesis now in vogue that heat imparts vibratory movements to the atoms themselves of bodies, involves the assumption that the vibrations are as rapid, or approximately so, as the undulations of the heat-waves; which the comparatively slow rate of transmission of sound-vibrations, and the comparatively feeble intensities of the elastic forces called into play in their transmission, renders in the highest degree improbable.

21. Waves of light and of the actinic force originate, like those of heat, in the vibratory movements of the atoms, or layers, of the electric envelopes of molecules.

22. The diverse properties of substances with respect to the absorption and radiation of special rays of light and heat, as revealed by the *spectroscope*, may be chiefly ascribed to diversities in the range and rate of variation of the intensity of the forces acting on the atoms of the electric envelopes of molecules. The envelope of the molecule of each substance is in a special condition of equilibrium, both as a whole and in all its parts; and any displacement of it, by heat-waves for example, will originate special rates and systems of vibration throughout its mass. It will in general vibrate as a whole, or in subdivisions of greater or less extent, or in its individual atoms. Stationary waves, and nodal points, lines, or surfaces, may also supervene by the combination of individual waves passing up and down through the envelope, or to and fro around it. In fact we have all the conditions necessary for the production of the diverse systems of bright or dark lines noticed in the spectra obtained with different substances, and also the continuous spectrum formed under certain circumstances. The commingling of initial waves with the direct and reflex waves from other contiguous molecular envelopes may also play a certain part in the phenomena.

Physicists have sought for a similar theoretical result by conceiving that the molecules of the vapor or gas are compound; but the fact that each incandescent gas when sufficiently compressed gives a continuous spectrum, necessitates the supposition that the gaseous molecule is composed of an indefinitely great number of simple molecules, which cannot be admitted. This conclusion cannot be avoided if we allow that to each rate of undulation of a ray belongs a particular degree of refrangibility.

23. The principle of the "Correlation of Forces," is implied in the doctrine of convertible energy that has been briefly set forth (p. 12). It applies to the dynamic energies that have resulted from the operation of cosmical or molecular forces during certain previous intervals of time. These energies are over and above the forces attendant at any instant upon the natural statical condition or tendency of things; and hence are so many disturbances of the natural equilibrium, and as this always tends to assert itself, must continually manifest themselves in connection with material movements, and these movements must be continually undergoing transformations, wherever the moving masses (whether of either of the two ethers or of ordinary matter) come into contact with others at rest.

24. In the gaseous state of matter the only molecular force in

operation is the heat-repulsion—the mutual attractive actions of the molecular envelopes having become insensible. The *law of Mariotte* may be deduced from this theoretical principle in the following manner. Let *m* be a point in the enclosure against which the elastic pressure is exerted, and *cmd* a slightly divergent cone; all the gaseous particles lying within this cone will be the centers of heat-waves proceeding in all directions, and in the thermal equilibrium of the mass each molecule will radiate an amount of heat equal to that which it receives by absorption from surrounding molecules. There will accordingly be a uniform wave-flow of heat from one layer *ab* of the conical mass to the next toward *m*, and thus the quantity of wave-force that falls on *m* is the same as if there were an uninterrupted flow directly from one layer *ab*. Now if the density of the gas be doubled, the number of molecules, or wave-centers in any layer *ab* will be doubled, and hence the wave-force impinging on *m* will be doubled. The result is the same as if, for every such slightly divergent cone, there was a line of aerial particles moving with a certain uniform velocity and impinging upon *m*; the density of this representative line being proportional to the density of the gas. This representative idea accords with the fundamental hypothesis of the kinetic theory of gases. The known deviations from the strict law of the proportionality of the elastic pressure to the density may be ascribed to the fact that the mutual attractive action of the molecular envelopes becomes sensible when the density is greatly increased, and the distance between the molecules approximates to *Od* (fig. 4, vol. iii, p. 339). The well known experiment by Joule, which established that a gas expanding into a vacuum experiences no change of temperature, shows that the heat-energy lost in the expansion of the escaping gas is restored by the impact of the particles upon the enclosure. This should obviously be the result if, as we have supposed, gaseous phenomena are entirely due to the operation of molecular heat-repulsion, since the energy of the repulsive heat-waves expended in imparting velocity to the particles should be given out again when the motion of the particles is arrested.

5.

It is obvious from the explanation above given of the law of Mariotte that if two different gases have the same temperature they will exert the same elastic outward pressure, provided the number of their molecules is the same for equal volumes; or, in other words, at the same temperature and pressure the number of particles in equal volumes of the two gases should be the same.

The specific heat of different gases should be the same under similar circumstances, unless some portion of the sensible heat applied should be expended in doing work within the molecules, and portions so expended should be unequal. Now in the case of simple gaseous molecules heat can do such interior work only by expanding their envelopes, and there is no apparent reason for any such possible effects being different with different gases. The case is different with compound molecules, for the expansive action of the heat will in general tend to develope an increased attraction between the envelopes of the constituents of the compound molecules, the operation of which will give rise to an absorption of a certain amount of heat. Such effects may be quite different for different compound gases; and so their specific heats may be quite unequal. The specific heats of compound gases, we should at the same time expect, would be greater than that of a simple gas.

## ART. IV.—*On the Datolite from Bergen Hill, New Jersey;* by EDWARD S. DANA. With Plate I.

THE Bergen Hill tunnel is famous for the abundance, beauty, and variety of the minerals which it brought to light. Datolite, pectolite, calcite, analcite, apophyllite, natrolite, stilbite, and others were obtained there during its excavation in a degree of perfection rarely equaled by the productions of any other locality. The crystallizations of datolite are especially remarkable; some of the surfaces covered with the brilliant crystals being eighteen to twenty-four inches in length.*

The crystals are in general not over a third of an inch across, though they sometimes have a diameter of one inch. Those of a single specimen have always entire uniformity of habit. The datolite is associated on different specimens with most of the other species found at the same locality, but it was not found possible to obtain any facts which would throw light upon the influence of the associated minerals on the crystalline form.

Among the varied forms, four different types may be distinguished.

*a*. Figure 1 represents a very common and characteristic form. The crystals here are very thin, wedge-like, and are attached to the mass of rock approximately by the extremity of the clinodiagonal, though varying from that of the diagonal terminating between 2 and 2 on one side, to that between $-4$ and $-4$ on the other. From the position and shape of these crystals, they offer an unusual number of surfaces for the reflection of the light, and hence give the specimen a brilliant sparkling aspect,

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* Many specimens of this kind occur in the collection of Mr. Haines of Elizabeth, N. J.

which serves to distinguish them at a glance from all other forms. It is always simple, no further modifications than those figured having been observed.

*b.* Another and more interesting type is shown in figure 2. The crystals here are approximately equal in the three dimensions. The figure shows this form in its simplest condition. Other planes occur, viz: $-2\text{-}i$; also (as in fig. 3, which is a portion of a crystal with this habit) $2\text{-}i$, $4\text{-}2$, $\frac{4}{3}$, $\frac{4}{5}$, $\frac{2}{3}$, $\frac{4}{3}\text{-}\grave{\imath}$, $-8\text{-}\grave{2}$; these modifications do not however change at all the general habit of the crystals.

*c.* Figure 14 represents a rare and quite unique form. The planes $-2\text{-}i$ and $2$ are here most prominent, and when the $I$ becomes merely a line and $i\text{-}i$ a minute triangle, as is sometimes the case, it has the aspect of a rhombohedron. The planes $i\text{-}2$ and $\frac{9}{4}\text{-}\grave{9}$ are noticeable features of the figure, and are spoken of more particularly in another place. In addition to the planes figured, there occur on crystals of the same specimen, $1$ very small, and $i\text{-}\grave{\imath}$, $i\text{-}\grave{2}$, $2\text{-}i$ as mere lines.

*d.* Another type of form, though not so distinct, is exhibited in figure 13. Under this type we seldom find the crystals of two specimens exactly similar, there being a great variation in the relative sizes and in the number of the planes. A very complex form of this type is represented in fig. 8. The special points to be noted in crystals of this habit are: the plane $-2\text{-}i$, which is often very large and almost invariably etched and therefore without polish; and also the presence of the series $-4\text{-}2$, $-4\text{-}4$, $-4\text{-}i$ and $-6\text{-}\frac{3}{2}$, $-6\text{-}3$, $-6\text{-}i$ (fig. 4). The planes of the series last mentioned are very common, the two series frequently occurring together; and one of them at least is almost always present with the $-2\text{-}i$. This peculiar range of planes above $i\text{-}i$ does not appear to have been met with on crystals from foreign localities.

Under these four distinct types and their modifications, all the crystals of Bergen Hill datolite observed by the writer (on over two hundred specimens) can be included.

The following is a list of all the planes observed; in it, those marked with an asterisk (*) have not been observed before: $O$ (top); vertical prisms, $i\text{-}i$, $i\text{-}4$,* $i\text{-}2$,* $I$, $i\text{-}\frac{3}{2}$, $i\text{-}\grave{2}$, $i\text{-}\grave{3}$,* $i\text{-}\grave{\imath}$; orthodomes, $-1\text{-}i$, $-\frac{4}{3}\text{-}i$, $-2\text{-}i$, $-4\text{-}i$, $-6\text{-}i$, $-8\text{-}i$; $2\text{-}i$; clinodomes, $\frac{1}{2}\text{-}\grave{\imath}$,* $1\text{-}\grave{\imath}$, $\frac{4}{3}\text{-}\grave{\imath}$, $2\text{-}\grave{\imath}$, $4\text{-}\grave{\imath}$; hemi-octahedral, $-4$; $4$, $2$, $\frac{4}{3}$, $1$, $\frac{4}{5}$, $\frac{2}{3}$;* $-4\text{-}2$, $-4\text{-}4$,* $-6\text{-}\frac{3}{2}$,* $-6\text{-}3$; $4\text{-}2$;* $-6\text{-}\grave{\frac{3}{2}}$* (?), $12\text{-}\grave{\frac{3}{2}}$; $-4\text{-}\grave{2}$, $-8\text{-}\grave{2}$, $\frac{8}{3}\text{-}\grave{2}$, $2\text{-}\grave{2}$; $-4\text{-}\grave{3}$,* $\frac{5}{2}\text{-}\grave{3}$,* $\frac{3}{2}\text{-}\grave{3}$;* $\frac{9}{4}\text{-}\grave{9}$,* $\frac{5}{2}\text{-}\grave{5}$,* $\frac{8}{7}\text{-}\grave{4}$* (?), $\frac{8}{9}\text{-}\grave{\frac{4}{3}}$* (?).†

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† The plane $6\text{-}i$, found by Hessenberg (Min. Not., IV, where it is designated $\frac{2}{3}$P∞) on a crystal from Bergen Hill, I have not observed except on crystals from Roaring Brook, Conn.

Among the new planes, the following are determined by the zones in which they occur, as will be seen in the figures: −6-$\frac{3}{2}$, −4-4 (fig. 4), 4-2 (fig. 3), *i*-2 (fig. 14), −4-3̀ (fig. 8,) $\frac{3}{2}$-3̀ (fig. 12),* $\frac{1}{2}$-ì (fig. 10, between 1 and 1-ì). *O* ⋏ $\frac{1}{2}$-ì = 171° 1′ (measured, 171° 7′). The plane *i*-2 consists of an oscillatory combination of *i*-*i* and *I*.

Other new vertical prisms are *i*-4 (fig. 11), and *i*-3̀ (fig. 8); *i*-4 ⋏ *i*-*i* = 170° 58′ (measured = 171° 11′); *i*-3̀ ⋏ *i*-*i* = 117° 39′ (measured = 117° 51′). $\frac{2}{3}$ (fig. 3) is a new octahedral plane; *O* ⋏ $\frac{2}{3}$ = 158° 36′ (measured = 158° 50′). $\frac{5}{2}$-3̀ has always the shape and position shown in figs. 9 and 12; *i*-*i* ⋏ $\frac{5}{2}$-3̀ = 108° 4′ (measured = 107°–108°); *O* ⋏ $\frac{5}{2}$-3̀ = 138° 14′ (measured = 138°). −6-$\frac{3}{2}$ (fig. 9) is on the edge between *I* and −4-2̀, consequently $m=\frac{2n}{n-1}$; the case did not admit of determination by measurement; but *n* is obviously less than 2 and greater than 1, and it is hence very probably equal to $\frac{3}{2}$, which puts it in the same zone with 12-$\grave{\frac{3}{2}}$.

Between $\frac{4}{3}$ and 2-ì a rough plane was observed in one case. Here $m=\frac{4n}{2n+1}$; the plane gives an angle of 165° to 168° upon 2-ì, and consequently *n* cannot be less than 3.

A remarkable series of planes, usually convex (figs. 16, 17), in the same zone with −2-*i*, 2-ì and *I* (opposite −2-*i*) having $m=\frac{2n}{n-1}$, often takes the place of the clinodome 2-ì, which when present is very narrow. The common form of this +*m*-$\grave{n}$ plane (*G*) is convex; starting from −2-*i*, where it makes on *i*-*i* an angle of about 95°, it curves around toward *I*, changing in the value of *m* and *n* constantly till the intersections with 4-ì and 1 become parallel and it makes an angle on *i*-*i* of 101° to 102° 30′. This is represented in fig. 17, which is a front view of the plane. In fig. 14 we have a plane of this zone quite flat though unpolished, and giving on measurement the angle on *i*-*i* of 94° 30′–96°; its symbol is consequently $\frac{9}{4}$-9̀ (required 95° 42′). Another plane of this zone, also sometimes occurring alone, as shown in fig. 16, a direct view of it, makes parallel intersections with 4-ì and 1, and is hence $\frac{5}{2}$-5̀; *i*-*i* ⋏ $\frac{5}{2}$-5̀ = 100° 57′. On the edge between $\frac{5}{2}$-5̀ and −2-*i* in the same crystal, a plane appears ($\nu$, fig. 16) which corresponds to the other part of *G*.

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Des Cloizeaux gives the planes $\gamma$, $\eta$, $\chi$, $\omega$, $\vartheta$, misapprehending the form which he takes from Dana's Mineralogy; placed in the proper position $\gamma$ would become $d\frac{1}{2}$, $\chi$, $q$, etc. $\gamma$ (−2-2̀), however, he figures also from a crystal of Haytorite.

[*] In figures 12 and 15 the crystal is represented inverted, which was necessary in order to show well the new planes.

The clinodome $\frac{4}{3}$-ì is also occasionally wholly or in part replaced by a convex $+m\text{-}\grave{n}$ plane ($\tau$), in the zone that includes 4, $\frac{4}{3}$-ì and $\frac{4}{5}$, which plane therefore has $m=\frac{4n}{2+3n}$. In fig. 6 the plane ($\tau$) occurs, though by the projection its shape is not satisfactorily shown. Measurement gives for $\tau \wedge i\text{-}i$=91° 30′ to 92° 30′ near its intersection with $-2\text{-}i$; but for the larger part of the plane $\tau \wedge i\text{-}i$=97°–98°, and near its intersection with $\frac{4}{5}$, $\tau \wedge i\text{-}i$= 107° 20′–108°. The last angle corresponds to the plane $\frac{8}{9}\text{-}\grave{\frac{4}{3}}$, whose calculated inclination on $i\text{-}i$ is 107° 36′; while the main part of the plane has for $n$ probably 4, giving $\frac{8}{7}\text{-}\grave{4}$, which inclines toward $i\text{-}i$ at the angle 97° 31′. There may be included in $\tau$ the plane $\frac{6}{5}\text{-}\grave{6}$, for which the same angle is 95° 13′. The plane $1\text{-}\grave{2}$ belongs to the same zone; but its angle with $i$-ì is 103° 36′, and evidence of its presence was not found, while that of $\frac{8}{9}\text{-}\grave{\frac{4}{3}}$ was sustained by the following observation.

Between 1 and $-2\text{-}i$ a convex plane ($\eta$ fig. 15) occurs, giving $i\text{-}i \wedge \eta$=105°–107°; from the zone we have $m=\frac{2n}{3n-1}$. This plane is in a zone between 1 and the position of $\frac{2}{3}$-ì, while $\frac{8}{9}\text{-}\grave{\frac{4}{3}}$ is in that between $\frac{4}{5}$ and $\frac{4}{3}$-ì. The two zones here cross, and if $\eta$ is the plane common to them, it is $\frac{8}{9}\text{-}\grave{\frac{4}{3}}$. That it is so is rendered almost certain by its angle of inclination on $i\text{-}i$, and its similarity to $\tau$ in having a convex surface.

The convex planes $G$ and $\tau$ replacing the clinodomes 2-ì and $\frac{4}{3}$-ì, sometimes occur together. In fig. 6, the intersections of $O$ with the two planes $\tau$ converge backward, a necessary consequence of the oblique position of $\tau$.

The series of planes above $i\text{-}i$, $-4\text{-}i$ and $-6\text{-}i$ and the adjoining planes, are in many cases curved, so that sometimes all intersections between them disappear; this is true of $-6\text{-}i$ and $-8\text{-}i$ in fig. 6. These series of curved planes are a remarkable feature of the datolite from this locality.

In regard to the special character of different planes, and their frequency of occurrence, I make a few additional remarks.

The clinodiagonal hemi-octahedral planes and vertical prisms, almost without exception, are destitute of any polish, and often quite rough; this is true also of $-2\text{-}i$ and the other orthodomes with $-6\text{-}3$ $-6\text{-}\frac{3}{2}$ $-4\text{-}4$, $4\text{-}2$, and 4. The remaining planes, with occasional exceptions, are well polished, though the presence of wavy lines on the surface in most cases prevents very accurate measurements. $O$, when of sufficient size to be well observed, is uniformly striated in two different directions, parallel to its intersections with the octahedral planes of the $m$ series. In the crystals from which fig. 3 was drawn, it consisted of an oscillatory combination of *O* and $\frac{2}{3}$, introducing so much irregularity as to make $\frac{4}{3}$-ì at times slightly triangular in shape.

The planes *I*, *i-i*, 4-ì, −4, and 2 are never absent; −2-í, 2-ì, *O*, 1 are very common; $\frac{4}{3}$-ì, −4-í, −6-í, −4-2, −4-4, −6-$\frac{3}{2}$, −6-3, a little less common; ì-$\frac{3}{2}$, ì-2̀, −8-2̀, 4, (the last generally associated with the clinodiagonal planes), were observed on perhaps one-tenth of the specimens examined; −1-í, −$\frac{4}{3}$-í, $\frac{4}{3}$, $\frac{4}{5}$, 2-í, 12-$\frac{3}{2}$, $\frac{9}{4}$-9̀, $\frac{8}{9}$-$\frac{4}{3}$, on one-twentieth; −8-í, ì-2, 4-2, $\frac{2}{3}$, $\frac{1}{2}$-ì, ì-3̀, $\frac{8}{3}$-2̀, $\frac{5}{2}$-3̀, $\frac{5}{2}$-5̀ are rare; and ì-4, 2-2̀, −6-$\frac{3}{2}$, −4-3̀, $\frac{3}{2}$-3̀, are very rare.

The following table of all the observed planes, with the lettering employed by different authors, is added for the sake of convenience of reference. Miller's letters are taken from Brooke and Miller's Mineralogy (1852); Schröder, from Pogg. Ann., xciv, 1855; Dauber, from Pogg., Ann., ciii, 1858. The planes in this list that have not been found by the author on the Bergen Hill crystals, have their symbols in parentheses.

| Dana. | Mohs. | Miller. | Schrœder. | Dauber. | Des Cl. |
|---|---|---|---|---|---|
| *O* | *s* | *a* | *s* | *a* | $h^1$ |
| *ii* | *b* | *c* | *b* | *c* | *p* |
| *i*4 (new) | | | | | |
| *i*2 (new) | | | | | |
| (*i*$\frac{3}{2}$) | *β* Gregg | | | | $e^{\frac{3}{2}}$ |
| *I* | *d* | *d* | *d* | *d′* | $e^1$ |
| ì$\frac{3}{2}$ | *r* | *r* | | *r′* | $e^{\frac{2}{3}}$ |
| ì2 | *o* | *o* | | *o′* | $e^{\frac{1}{2}}$ |
| ì3 (new) | | | | | |
| *i*ì | *u* | *b* | *o* | *b′* | *g′* |
| (−$\frac{2}{3}$*i*) | | | | | $o^{\frac{1}{6}}$ |
| −1*i* | | *u* | *γ* | *u* | $o^{\frac{1}{4}}$ |
| −$\frac{4}{3}$*i* | | *v* | | | $o^{\frac{1}{3}}$ |
| −2*i* | *a* | *x* | *a* | *x* | $o^{\frac{1}{2}}$ |
| (−3*i*) | | *f* | | | $o^{\frac{3}{4}}$ |
| −4*i* | | *φ* | | | *o′* |
| −6*i* | | *s* | *y* | | $o^{\frac{3}{2}}$ |
| −8*i* | | | *z* | *ψ* | $o^2$ |
| (8*i*) | | | | | $a^2$ |
| (6*i*) | | | | | $\frac{2}{3}$P∝Hess'b. |
| 2*i* | | | | *γ″* | $a^{\frac{1}{2}}$ |
| $\frac{1}{2}$ì (new) | | | | | |
| 1ì | | *σ* | | *σ* | $h^{\frac{5}{3}}$ |
| $\frac{4}{3}$ì | | *t* | *t* | *t* | $h^2$ |
| 2ì | *g* | *g* | *g* | *g* | $h^3$ |
| ($\frac{8}{3}$ì) | | | | | $h^5$ |
| 4ì | *f* | *m* | *f* | *m* | *m* |
| (8ì) | | | | | $g^3$ |
| −4 | P | *n* | P | *n* | $d^{\frac{1}{2}}$ |
| (−8) | | | | *ξ* | *δ* |
| 4 | *n* | | | *β′* | $h^{\frac{1}{2}}$ |
| 2 | *e* | *e* | *e* | *e′* | *ε* |
| $\frac{4}{3}$ | *l* | | *l* | *φ′* | *d* |
| 1 | *m* | *l* | | *l′* | *μ* |
| $\frac{4}{5}$ | | *κ* | | *κ′* | *κ* |
| $\frac{2}{3}$ (new) | | | | | |

| Dana. | Mohs. | Miller. | Schrœder. | Dauber. | Des Cl. |
|---|---|---|---|---|---|
| $-42$ | | | $q$ | | |
| $-44$ (new) | | | | | |
| $42$ (new) | | | | | |
| $-6\frac{3}{2}$ (new) | | | | | |
| $-63$ | | $\rho$ | | $\rho$ | $q$ |
| $(-3\cdot3)$ | | | | | |
| $(-3\frac{3}{2})$ | | $\omega$ | | | $x$ |
| $(-5\frac{5}{3})$ | | | | $\chi$ | $\zeta$ |
| $(24\cdot3)$ | | | | | $z$ |
| $(-2\grave{2})$ | | | | | $\gamma$ |
| $(-\frac{8}{3}\grave{2})$ | | $i$ | $a$ | $i$ ($h$ Gregg) | $u$ |
| $-4\grave{2}$ | | $z$ | | $z$ | $d\frac{1}{4}$ |
| $-8\grave{2}$ | $q$ | $q$ | $\beta$ | $q$ | $\beta$ |
| $\frac{8}{3}\grave{2}$ | $i$ | | | | |
| $2\grave{2}$ | $h$ | $h$ | $a$ | $h$ | $a$ |
| $-4\grave{3}$ (new) | | | | | |
| $\frac{3}{2}\grave{3}$ (new) | | | | | |
| $\frac{5}{2}\grave{3}$ (new) | | | | | |
| $(4\grave{4})$ | | $y$ | | | $d\frac{1}{8}$ |
| $-\grave{6}\frac{3}{2}$(?) (new) | | | | | |
| $12\grave{\tfrac{3}{2}}$ | $p$ | $p$ | | | $\pi$ |
| $\frac{5}{2}\grave{5}$ (new) | | | | | |
| $\frac{9}{4}\grave{9}$ (new) | | | | | |
| $\grave{\tfrac{8}{7}}4$(?) (new) | | | | | |
| $\grave{\tfrac{8}{9}}\frac{4}{3}$(?) (new) | | | | | |

In the figures the axes have the positions and the relative values adopted by Professor Dana, and the system of symbols employed is also the same. It is to be noticed that Lèvy adopted this position of the axes in his work on the Heuland Cabinet (1837), while other authors have taken *i-i* as *O* and made either 2-ì or 4-ì the fundamental prism. This position has the considerable advantage of giving the planes in vertical zones, and so making the form more symmetrical, and the mathematics more simple. It might be an improvement to double the length of the vertical axis; the theoretical form would then approach more closely to the dimensions commonly occurring in the crystals.

It is worthy of note that the planes of the fundamental *plus* octahedrons are represented by the terms in the series $\frac{4}{1}$ $\frac{4}{2}$ $\frac{4}{3}$ $\frac{4}{4}$ $\frac{4}{5}$ $\frac{4}{6}$ $\frac{4}{8}$; and excluding 8-ì and $\frac{8}{3}$-ì, two clinodomes mentioned by Des Cloizeaux, the clinodomes are all of the same series, though wanting thus far the members $\frac{4}{5}$ and $\frac{4}{6}$.

In the preparation of this article I have had the free use of the specimens of datolite in the cabinets of Yale College, Prof. G. J. Brush, Rev. E. Seymour of New York, and Mr. Benjamin

Haines of New Jersey. The cabinet of Mr. Haines contains many hundred specimens, and I am greatly indebted to his kindness for the privilege of examining them at my leisure. To Mr. Seymour and Professor Brush I would also express my grateful acknowledgments. The complex form represented in figure 8 is from a crystal in the cabinet of Mr. Seymour.

New Haven, Ct., May, 1872.

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## ART. V.—*On certain Lower Silurian rocks in St. Lawrence county, N. Y., which are probably older than the Potsdam Sandstone;* by T. B. BROOKS.

A SURVEY of the Caledonia and Keene iron mines at Keene Station, St. Lawrence county, New York, made by me in the spring of 1870, developed the following series of sedimentary conformable rocks, some of which are apparently older than the Potsdam.

In descending order the series is as follows:—1st, A fine grained, somewhat friable light gray, sometimes reddish, sandstone, which toward the bottom of the bed is often a quartz conglomerate. It is lighter colored and less firm, but otherwise resembles the sandstone quarried at Potsdam. The maximum thickness observed was, say 40 feet, but the line separating this rock from No. 2 was not always well-defined, and the surface was lowered from erosion.

This rock is named by Dr. Emmons Potsdam sandstone, page 93, Part IV, Geology of N. Y., where the Caledonia Mine is described under the name of the "Parish ore bed."

2nd, Next below this sandstone is the iron ore formation; made up of red hematites, both specular and earthy, together with irregular lenticular masses of a brownish and very compact sandstone or quartzite, and a magnesian rock resembling No. 3 of this series. Associated with the ore are the carbonates of lime and iron and other minerals: carbonaceous matter is shown by the analyses. This formation varied greatly in thickness in different localities, from a few feet to at least 40. The mines which are now extensively worked are in this formation.

3rd, Under the ore, and forming the foot wall of the mines, is a soft rock, generally schistoze or slaty, but sometimes massive in structure, of a green to grayish-green color, weathering lead gray and becoming porous where exposed in outcrops. It is apparently a magnesian rock, containing considerable graphite and iron pyrites, designated by Dr. Emmons as serpentine, and with the overlying ore was regarded by him as eruptive. This schist, like the ore, varies greatly in thickness, the maximum observed being at least 90 feet.

4th, Below the schist is a bed of granular, crystalline limestone, white to light gray in color; often friable near the surface and weathering to a dark color. It holds numerous crystals of bronze colored mica and, still more abundantly, graphite in thin scales. The thickness of this formation is not less than 250 feet.

5th, Is a sandstone similar in character to No. 1, described above. The thickness is uncertain, but one outcrop is exposed not less than 15 feet. Dr. Emmons does not mention this rock, and I do not think he observed it, or he certainly would have mentioned it in connection with his igneous theory for the origin of limestones, inasmuch as it separates two deposits of his "primitive limestone."

6th, Is a granular crystalline limestone closely resembling No. 4 before described, but differing in containing in places irregular beds or veins of granite, composed of a white feldspar and quartz. A mineral resembling tremolite was observed in this formation. This association of limestone and granite is fully described by Dr. Emmons (pages 24 and 338 and 340), and seems to have afforded him the best arguments for his peculiar views regarding the origin of the limestone. The thickness of this bed could not be even approximately determined; it is certainly thicker than the limestone already described.

As my survey was purely economic, having reference to explorations for iron ore, not much attention was paid to the thickness of the rocks below the ore. I consider that the series described has a minimum thickness of 700 feet, and probably much greater. Underlying this whole series and bounding it on the southeast side is a well characterized gneiss, which is in all probability non-conformable; but the actual contact was not seen. This gneiss is a part of the great azoic area of northern New York, and is colored Laurentian on the geological map of Canada. It is lithologically a totally different rock from the granite described above in No. 6 as associated with the marble, although Dr. Emmons seems to give them the same origin and age. No limestone was seen in it,—the feldspar was reddish, and it always contained mica and often hornblende.

This series of sandstones, limestones, and ferruginous and magnesian schists was not found complete, so as to display unmistakably the sequence of the beds except on one section, i. e., through the west corner of the Caledonia mine lot:—and there the 2nd and 3rd members were of less than usual thickness. The Laurentian gneiss was not seen on this section. At the Kearny mine the members of the series from 1 to 4 inclusive are well shown. At the Caledonia mine the 1st, 2nd and 3rd are well developed, as is also the case southeast of the Keene. At this latter point the magnesian schist No. 3 is seen in such close proximity to the gneiss as to render it probable that beds 4 to 6 are wanting. This could be explained by supposing great irregularity in the bottom of the sea in which they were deposited.

The whole series are folded, presenting several anticlinal and synclinal axes which run rudely parallel with the edge of the Laurentian area, i. e., northeast and southwest. On one section a half-mile long across the Caledonia and Kearney mines, no less than six such axes were observed. Prof. Dana describes the rocks of the Potsdam epoch in New York as having usually "a gentle dip or as nearly horizontal." This description would not embrace the rocks at this locality. In one place an outcrop of magnesian schist (No. 3) has a dip of 80°, indicating a very sharp fold. Aside from this, the inclinations observed varied from 0° to 40°. The upper sandstone has been eroded from considerable part of the area about the mines, exposing the lower rocks and affording a good opportunity for stratigraphical study. The surface is quite hilly, the highest point observed being 120 feet above the valley at its base.

As has been remarked, Dr. Emmons describes formations Nos. 1, 2, and 3 in Geology of New York, Part IV, page 93, under the respective names of "Potsdam Sandstone," "Specular Iron Ore" and "Serpentine." He regards the last two as well as the limestone which underlies them as eruptive, and does not seem to have observed the sandstone which divides the two limestone beds. The planes of bedding were occasionally obscure in the magnesian rock and sandstones, often so in the iron ore and marbles; but taken as a whole they cannot be regarded as other than sedimentary rocks showing more or less evidence of metamorphic action. Except as noted above, I do not find them described by the New York geologists.

Regarding their age, if Dr. Emmons is right in calling the uppermost sandstone No. 1, "Potsdam," as he has repeatedly done, and if he begins the Potsdam epoch at this locality with the bottom of this sandstone, which is unquestionably his intention, then the rock beds 2 to 6 inclusive are older than this so-called base of the Paleozoic column. This view would possibly make them the equivalents of the Taconic system of Dr. Emmons, and it is strengthened by the similarity in general lithological character. number and order of the beds between this series and that system as described in Geology of New York, Part IV, pages 138 to 144. It is hardly to be supposed, however, that Dr. Emmons would have passed over so prominent a suggestion of his favorite system without recognizing it.

There is no doubt but that his peculiar views regarding the origin of limestones stood somewhat in the way of his seeing all the facts that this interesting locality exhibits. His section of the Parish ore bed, page 93, Part IV, does not represent the facts now to be observed. I found only magnesian schist where he has marked gneiss. It should be remarked, however, that there has been a large amount of work done of late years, revealing many additional facts.

Dr. Emmons gives 60 to 300 feet as the minimum and maximum thicknesses of the Potsdam sandstone in northern New York, and it has been described as diminishing in some instances to 20 or 30 feet. The prevailing rock observed in this region seems to have been a laminated sandstone, frequently having a conglomerate as its lower member. Nor have any rocks of different lithological character been ascribed to this period in the region in question; although partly calcareous layers and even beds of true limestone have been observed in the upper rocks of this period in the northwest. The rocks I have described, therefore, have apparently too great a thickness and show too wide a variation in lithological character to be regarded as the equivalents of the Potsdam. Some forms, obtained from a calcareous layer the base of the upper sandstone, which I thought might be organic, were pronounced by Dr. Newberry to be concretions. The abundance of graphite was the only evidence of organic life observed.

The Potsdam quarries are only 35 miles northeasterly from the Rossie mines, and the country between was examined by Dr. Emmons. He may have traced the sandstone through stratigraphically. My own reconnaissance of this country leads me to believe that this could easily be accomplished. Should it be done, a point of considerable interest which would be incidentally settled is this:—Does the Potsdam sandstone in this region become, in places, decidedly gneissic in character? My own hasty observations at the Sterling and Tate ore beds leads me to believe that it does: if so, the Tate bed described by Dr. Emmons, pages 95 and 346, as being overlaid by gneiss, may be found to be the equivalent of the Rossie beds, which I believe to be the case. The ores are certainly very similar. This Tate bed confirmed Dr. Emmons, and with good reason, in his view, that the iron ores were of "primitive age," lying either in or immediately on top of the great gneissic series.

As bearing on this subject, I would mention that the iron ores of the Maramec district in Crawford and Phelps Co., Mo., bear a close resemblance to those of Rossie, and work equally well in the furnace. The Missouri ore contains considerable yellow ochre, which is less abundant at Rossie; but the specular and earthy red hematites are nearly identical and unlike any other iron ore I have seen. A sandstone associated with the Missouri ores is very similar to the Potsdam in lithological character, and the series is, I believe, regarded by the Missouri geologists as of Lower Silurian age. Iron ores are described as occurring in the Potsdam period of Canada, but I do not know of any ore in the United States which the Geological Reports assign to that period.

## ART. VI.—*On a simple Apparatus for the Production of Ozone with Electricity of high tension;* by Prof. ARTHUR W. WRIGHT.

EXPERIMENT has shown that in the production of ozone by electricity the maximum amount of oxygen is ozonized by the silent or glow discharge, and most of the forms of apparatus by which this is effected are contrivances by which oxygen is made to flow slowly through a space traversed by such a discharge. In v. Babo's apparatus, as well as in those of Siemens and Houzeau, the metallic conductors are separated by glass and a stratum of air. By inductive action of the charged metallic surfaces the intervening air becomes charged with electricity oppositely upon its two sides, and simultaneously with the discharge of the metallic terminals, through the wire of the coil, a discharge takes place through the air, not in the form of sparks, but diffusely, producing a glow of purplish light, visible only in the dark.

These apparatus succeed best with electricity of comparatively low tension. In using the Holtz's electro-machine with them the discharge is apt to occur chiefly in the form of sparks through the air, or it may even traverse and perforate the glass, and the form of the apparatus must be varied to give the best results.

When the poles of the machine itself are separated to a sufficient distance the electricity passes between them either in the form of a diffuse brush, spanning the whole interval, or with a very minute brush upon the negative pole, and a glow upon the positive, the intermediate space not being visibly luminous. This is the so-called dark or silent discharge, exhibiting the phenomena of the electric shadow when suitable objects are interposed, as described in a former paper.* When this occurs the strong odor shows that a considerable amount of the atmospheric oxygen is converted into ozone.

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* This Journal, II, xlix, p. 381, and III, i, p. 437.

If this discharge is made to take place in an enclosed space through which air or oxygen can be driven, the ozonizing effect of the electricity is heightened and can be utilized. The apparatus which I have employed, and which has afforded very satisfactory results, consists of a straight glass tube about 20 centimeters long and having an internal diameter of 2·5 centimeters, the two ends being stopped with corks covered on the inner side with a thin coating of cement to protect them from the action of the ozone. Through the axis of each cork is inserted a glass tube of about 5 millimeters caliber, and 7 centimeters in length, having a branch tube inserted perpendicularly at the middle and long enough to permit a rubber tube to be slipped upon it. The outer ends of the tubes themselves are closely stopped with corks, through which are passed straight, thick copper wires carrying suitable terminals at their inner ends, and bent into a ring at the others. They are fitted so as to make tight joints, but to allow of motion in order to vary the distance between their inner ends. One of these wires carries a small ball; the other terminates in a disk with rounded edge, set perpendicularly to the axis of the tube, and so large as to leave an annular space of some two or three millimeters breadth around it. The gas is admitted through one of the branch tubes and escapes from the other, after having passed through the whole length of the tube.

In using the apparatus the wires must be connected with the poles of the machine in such a manner that the disk becomes the negative terminal, as this arrangement gives the greatest degree of expansion and diffuseness to the current. On turning the machine, and adjusting the ball and disk to a proper distance, a nebulous aigrette surrounds the latter, quite filling the interval between it and the wall of the tube, while the part of the tube between the disk and ball is crowded with innumerable hazy streams converging upon the positive pole, or simply causing the latter to be covered with a faint glow. A current of air or oxygen sent into the tube must pass through this, and ozone is very rapidly produced, and in great quantity. The condensers are of course not used with the machine, when this apparatus is employed.

There appears to be an advantage in causing the oxygen to pass from the negative toward the positive within the tube, for the gas through which the discharge passes is transported in the contrary direction, as may be readily seen on bringing a candle flame between the poles of the machine, or causing a thin column of smoke to rise through the polar interval. The flame and the smoke are deflected, and stream off toward the negative pole. If the gas should be admitted in the direction mentioned, there would be a tendency to obstruct its flow somewhat, and thus keep it longer under the influence of the electricity.

Some experiments which were made with the apparatus will give an idea of its efficiency. One hundred cubic centimeters of water were placed in an upright tube or test-glass, and into it were put 20 drops of strong indigo solution, causing it to assume a deep blue tint. Air was driven through the ozonizing tube, under a pressure of about three inches of water, and on issuing from it conveyed by a tube into the solution. When the electro-machine was put in operation, being turned with sufficient speed to give nearly its maximum effect, the solution completely lost its blue color in less than four minutes. Blue litmus solution under similar circumstances became pale pink, but required a considerably longer time for the change.

When Schönbein's test solution is employed the deep blue color is immediately produced, but the solution is too thick to work well if the starch has been heated considerably, or for a long time, in making it. A better proportion is to take one part of potassic iodide by weight, ten parts of starch, and five thousand parts of water. This forms a milky solution, sufficiently mobile to mix well when the ozonized air bubbles through it. When 100 cubic centimeters of this solution were used, and air passed through the apparatus as before, the blue color appeared at once on application of electricity, and in 30 seconds it was deeply colored.

With dry oxygen the effects were much more rapid and remarkable. 100 cubic centimeters of the solution were used, as before. The instant the machine was put in action the liquid about the end of the delivery tube became deep blue, and in from ten to fifteen seconds the whole had acquired a uniform and intense blue color.

The summer moisture having interfered somewhat with the effective working of the electro-machine, there has been no opportunity to determine the percentage of ozone produced in this manner, but it appears to be very large. When dry oxygen is passed through the tube very slowly, the issuing gas when inhaled produces a painful burning sensation in the lungs, and causes violent coughing, which persists for a considerable time.

When oxygen is used it is found that the electrodes must be separated to a much greater distance than is necessary for air, otherwise sparks pass and destroy a large proportion of the ozone already formed. With air the direct spark in the apparatus could not be made to pass over an interval of more than 7 centimeters, but in oxygen they did not cease until the poles were separated about 11·5 centimeters. When the tube was filled with air and the poles were 7 or 8 centimeters apart the discharge was of the silent kind, but on admitting oxygen it immediately took the form of direct sparks.

The quantity of the solution used in these experiments was much greater than would be needed in order to exhibit the characteristic reactions of ozone to an audience of moderate size. One-half or one-third of the amount would be quite sufficient, and the time required for the reaction would be proportionally shorter. The great quantity of the ozone, as well as the ease and rapidity with which it is produced, render the apparatus especially serviceable for use in the lecture-room.

## ART. VII.—*On the Action of Ozone upon Vulcanized Caoutchouc;* by Prof. ARTHUR W. WRIGHT.

IN using the Holtz's electro-machine, in the summer season, it is often very difficult to make it retain any considerable charge, or even to keep up its action for more than a few minutes. The ebonite insulators are found to have lost in a large degree their insulating power, and to have become conductors to such an extent that considerable sparks may be drawn from them at points several inches distant from the metal parts supported by them, thus dissipating the greater portion of the charge. This is the usual condition of things when the machine, after much use, has stood for some weeks in the warmer portion of the year unused. The surface of the ebonite becomes hygroscopic, condensing upon itself a large amount of moisture, the accumulated liquid being sometimes so abundant as to trickle down in drops.

Having noticed on one occasion that this liquid had an acid taste, I was led to examine it more closely, and the ordinary tests very speedily showed it to be sulphuric acid. Its presence was a sufficient explanation of the defective insulation. Similar deposits of moisture were found upon the ebonite jackets of two induction coils some time after they had been used.

As nothing containing sulphur had been used about the apparatus, the acid was evidently derived from the ebonite itself. The first thought was that the material had been heated in the process of vulcanization sufficiently to oxidize the sulphur; but as the sulphurous oxide, if thus formed, would be dissipated by the heat, this could hardly be regarded as the source of the sulphuric acid, especially as the latter did not appear until after the apparatus had been used. It is well known that vulcanized caoutchouc is affected by ozone, and that the ordinary rubber tubes through which it is passed are attacked and quickly perforated by it. It seemed most probable then that the acid was the result of the action of the ozone upon the insulators, and experiments were made which entirely confirmed this supposition.

To the exit-tube of the ozonizing apparatus described in the previous paper was attached one end of a vulcanized rubber tube a few inches long, the other end being slipped upon the glass tube of a small wash-bottle containing some thirty or forty cubic centimeters of water. Air was slowly driven through the apparatus, and, having been strongly ozonized by the action of the electricity, bubbled up through the water. This was continued for an hour and a half. At the end of this time common air was passed through the apparatus to displace the ozone left in it, the tubes were removed, and the bottle closed with a glass stopper. On opening the bottle some time afterward, there was an unmistakable odor of sulphurous oxide, and the water reddened blue litmus paper very quickly and strongly. A strip of litmus paper, hung in the bottle so as not to touch the water, was completely reddened in a short time, and this happened even after several days had elapsed from the time of the experiment. The water tested with chloride of barium gave a considerable crystalline precipitate, leaving no doubt of the presence of sulphuric acid.

A small slip was cut from a thin plate of ebonite, cleaned and dried, and placed in a small bottle into which ozone was driven as before. In a short time it was bedewed with moisture having an acid taste, and exhibiting the same properties as that found upon the insulators of the machine.

In order to determine whether the sulphur itself could be directly oxidized by ozone, a quantity of fine flowers of sulphur was gently rubbed into a loose lock of dry cotton, so as to diffuse it as much as possible. The cotton was placed in a dry wash-bottle, and connected by means of a glass tube with a second wash-bottle containing a little water, all the connecting tubes being of glass. Ozone was passed through the bottles for an hour and a half, but at the end of this time not the slightest evidence of any action upon the sulphur could be detected. This was what might have been expected, for as the air often contains a small percentage of ozone, sulphur exposed to it would undergo slow alteration, with loss of weight, and it does not appear that anything of the kind has ever been observed.

It is evident that while the ebonite is undergoing decomposition by the ozone, the oxygen combines with the issuing sulphur to form sulphurous oxide, which with the atmospheric moisture produces sulphurous acid, this in turn being converted into sulphuric acid by the further action of the ozone. The absorption of moisture from the atmosphere by the sulphuric acid produces the dew-like deposits observed.

The deleterious effect upon the insulators can be remedied by neutralizing the acid with some substance which will not form a hygroscopic compound or essentially lessen the insulating power of the ebonite. I have used oxide and carbonate of magnesium with very good effect. A little of either of these substances in fine powder is sprinkled upon a soft cloth or piece of chamois leather and rubbed over the insulators. The excess is removed with a wet cloth, and the surface, after drying, cleaned and polished by rubbing with a soft woolen cloth very slightly moistened with carbonic di-sulphide. As the ebonite is attacked by the latter substance, care should be observed, in employing it, to use only so much as is needed to facilitate the polishing process without injuring the surface. The ebonite may be somewhat discolored by these operations, but the color can be restored by rubbing with a little oil, or will return of itself after a time.

Probably a better method may be found, but this gives very good results. On one occasion, early last autumn, when the electro-machine had not been used for some months, the sparks obtained on charging it and using small condensers were only about one quarter of an inch in length, and the action of the apparatus was very feeble. The insulators were quite damp with the accumulated moisture. When this had been removed by the process described, sparks eight or nine inches in length were obtained at once, and the machine worked with nearly its usual energy.

## ART. VIII.—*On the Oceanic Coral Island Subsidence;* by JAMES D. DANA.*

Coral islands have been shown to be literally monuments erected over departed lands; and, through the evidence from such records, it is discovered that the Pacific has its deep-water mountain chains, or lines of volcanic summits, not merely hundreds, but thousands of miles in length. Some of the ranges of high islands are proved by such records to have an under-water prolongation, longer than that above water: the line of the Hawaiian Islands, for example, which has a length of only four hundred miles from Hawaii to Kauai, and five hundred and thirty to Bird Island, the western rocky islet of the group, but stretches on westward, as the coral registers show,

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* From the closing chapter of the writer's work on Corals and Coral Islands, (pp. 364–372), recently published by Dodd & Mead, New York.

even to a distance of two thousand miles from Hawaii, or as far as from New York to Salt Lake City; and how much farther is unknown, as the line of coral islands here passes the boundary of the coral reef seas, or the region where coral records are possible.

Other ranges of submerged summits are shown to extend through the whole central Pacific, even where not a rocky peak remains above the surface; for all the coral islands from the eastern Paumotus to Wakes Island, near long. 170° E. and lat. 19° N., north of the Ralick and Radack (or Marshall) groups, are in linear ranges: and they have, along with the equally linear ranges of high islands just south, a nearly uniform trend, curving into northwest and north-northwest at the western extremity. The coral islands consequently cap the summits of linear ranges of elevations, and all these linear ranges together constitute a grand chain of heights, the whole over five thousand miles in length. Thus, the coral islands are records of the earth's submarine orography, as well as of slow changes of level in the ocean's bottom.

This coral island subsidence is an example of one of the great secular movements of the earth's crust. The axis of the subsiding area* has a length of more than six thousand miles —equal to one-quarter of the circumference of the globe; and the breadth, reckoning only from the Sandwich Islands to the Friendly Group (or to Tongatabu) is over twenty-five hundred miles, thus equalling the width of the North American continent. A movement of such extent, involving so large a part of the earth's crust, could not have been a local change of level, but one in which the whole sphere was concerned as a unit; for all parts, whether participating or not, must have in some way been in sympathy with it.

This subsidence was in progress, in all probability, during the Glacial era, the thickness of the reefs proving that in their origin they run back through a very long age, if not also into the Tertiary. It was a downward movement for the tropical Pacific, and perhaps for the warmer latitudes of all the oceanic areas, while the more northern continental lands, or at least those of North America, were making their *upward* movement, preparatory to or during that era of ice.

The subsidence connected with the origin of coral islands and barrier reefs in the Pacific has been shown† to have amounted to several thousands of feet, perhaps full ten thousand. And, it may be here repeated that, although this sounds large, the change of level is not greater than the *elevation* which

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* The position of this area is stated on page 328 of the volume on Corals and Coral Islands.

† Ibid, p. 329.

the Rocky Mountains, Andes, Alps and Himalayas have each experienced since the close of the Cretaceous era, or the early Tertiary; and perhaps it does not exceed the upward bulging in the Glacial era of part of northern North America.

The author has presented reasons for believing* that in this Glacial era the watershed of Canada, between the River St. Lawrence and Hudson's Bay, was raised at least 5,500 feet above its present level (1,500 feet); and that this plateau thus elevated was the origin of the great glacier which moved southeastward over New England. This region is the summit of the eastern arm of the great V-shaped Azoic area of the continent, the earliest elevated land of North America; and it is not improbable that the other arm of the V, reaching from Lake Superior and Huron, northwestward, to the Arctic, was raised at the same time to a higher elevation, and was the source of glacial movements over the more central portions of the continent:—we cannot say *western* portions also, since, in the *first* place, the facts, according to Prof. J. D. Whitney, do not sustain the statement; and, in the *second*, the great mountain ranges of the west would have been a barrier to all influences from any central continental elevation, and, besides, the slopes of these ranges, even if the Pacific border were higher to the north than now, would have in general determined the course of the western glacial movements.

The idea that the two arms of the great Azoic V were raised together, is not without some support. For the courses of the two were the courses of great continental uplifts or movements, again and again, through the successive subsequent ages; and the present outline of the continent is but the final expression of the great fact; moreover, the elevations parallel to the western arm of the V have been much the greatest. Even the exceptional courses, such as the nearly north and south trend of the Green Mountains, were marked out first in the Azoic, the Azoic peninsula of northern New York with the line of the Adirondacks being an exhibition of it. And all this uniformity of movement, from the laying of the first stone in the developing continent to the last, has been shown by the author to be directly connected with the fact that the continent has always been bordered by the same two great oceanic depressions, the Atlantic and the larger Pacific, the same in trend of axis as now, the North Atlantic having a northeast and southwest trend, parallel with one arm of the Azoic, and the Pacific a northwest and southeast parallel with the other arm of the Azoic. It is therefore reasonable that, late in geological history, during the Glacial era, after the great mountain chains of

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* This Journal, III, ii, 1871.

the continent had been made and raised to their full height, and the surface crust thickened over all the continent, except that of the Azoic nucleus, by successive beds to a thickness of thousands of feet, even thirty-five thousand by the close of the Paleozoic along the Appalachians, and probably much beyond this on the Pacific border; and when these thick sediments had in many regions been stiffened by crystallization or metamorphism; I say it is reasonable that, finally, changes of level, through the working of the old system of forces, should again have affected most the old nucleal Azoic area of the continent, where there had been no thickening except what had taken place internally; and that, if one arm of the V, that along the Canadian watershed, were raised at this time—as the facts prove—the other, northwestern in trend, should also have been raised, and to a greater extent. This is at least probable enough to become a question for special examination over the region.

These northern continental upward movements which introduced the Glacial era, carrying the Arctic far toward the tropics, may have been a balance to the *downward* oceanic movements that resulted in the formation of the Pacific atolls. While the crust was arching upward over the former (not rising into mountains, but simply arching upward), it may have been bending downward over the vast central area of the great ocean.

The changes which took place, cotemporaneously, in the Atlantic tropics, are very imperfectly recorded. The Bahamas show by their form and position that they cover a submerged land of large area stretching over six hundred miles from northwest to southeast. The long line of reefs and the Florida Keys, trending far away from the land of southern Florida, are evidence that this Florida region participated in the downward movement, though to a less extent than the Bahamas. Again, the islands of the West Indies diminish in size to the eastward, being quite small in the long line that looks out upon the blank ocean, just as if the subsidence increased in that direction. Finally, the Atlantic beyond is water only, as if it had been made a blank by the sinking of its lands.

Thus the size of the islands, as well as the existence of coral banks, and also the blankness of the ocean's surface, all appear to bear evidence to a great subsidence.

The peninsula of Florida, Cuba, and the Bahamas look, as they lie together, as if all were once part of a greater Florida or southeastern prolongation of the continent. The northwestern and southwestern trends, characterizing the great features of the American continent, run through the whole like a warp and woof structure, binding them together in one system; the former trend, the northwest, existing in Florida and the Bahamas, and the main line of Cuba; and the latter course, the west-southwest, in cross lines of islands in the Bahamas (one at the north extremity, another in the line of Nassau, and others to the southeast), in the high lands of northwestern and southeastern Cuba, and in the Florida line of reefs, and even further, in a submerged ridge between Florida and Cuba. This combination of the two continental trends shows that the lands are one in system, if they were never one in continuous dry land.

We cannot here infer that there was a *regular* increase of subsidence from Florida eastward, or that Florida and Cuba participated in it equally with the intermediate or adjoining seas; for the facts in the Pacific have shown that the subsiding oceanic area had its nearly parallel bands of greater and less subsidence, that areas of greatest sinking alternated with others of less, as explained on page 328; and that the groups of high islands are along the bands of least sinking. So in the Atlantic, the subsidence was probably much greater between Florida and Cuba than in the peninsula of Florida itself; and greater along the Caribbean Sea parallel with Cuba, as well as along the Bahama reefs, than in Cuba.

The position of the lonely Bermuda atoll confirms these deductions. Its solitary state is reason for suspecting that great changes have taken place about it; for it is not natural for islands to be alone. The tongue of warm water due to the Gulf Stream, in which the Bermudas lie, is narrow, and an island a hundred miles or more distant to the northeast-by-east, or in the line of its trend, if experiencing the same subsidence that made the Bermuda land an atoll, would have disappeared without a coral monument to bear record to its former existence. Twenty miles to the southwest-by-west from the Bermudas, there are two submerged banks, twenty to forty-seven fathoms under water, showing that the Bermudas are not completely alone, and demonstrating that they cover a summit in a range of heights; and it may have been a long range.

In the Indian ocean, again, there is evidence that the coral-island subsidence was one that affected the oceanic area more than the adjoining borders of the continent, and most, the central parts of the ocean. For, in the first place, the Archipelago of the Maldives narrows and deepens to the southward. Further, the large Chagos Group, lying to the south of the Maldives, as remarked upon by Darwin, contains but very little dry land in any of its extensive reefs, while some of them, including the Great Chagos Bank, are sunken atolls. Again, still other large reefs nearly bare, lie to the southwest of the Chagos Group; while Keeling's is another outlying atoll southwest of southern Sumatra and far out toward mid-ocean.

The probability is, therefore, that both the central Atlantic and Indian Oceans were regions of this subsidence, like the central Pacific, and that the absence of islands over a large part of their interiors may be a consequence of it. A rate of sinking exceeding five feet in a thousand years (if my estimate from the growth of corals is right) would have buried islands and reefs together in the ocean; while, with a slower rate, the reefs might have kept themselves at the water's surface. So small may have been the difference of rate in the great movement that covered the Pacific with coral islands, but left the Indian Ocean a region of comparatively barren waters, with some "half-drowned" atolls, and the central Atlantic almost wholly a blank.

While thus seeming to prove that all the great oceans have their buried lands, we are far from establishing that these lands were oceanic continents. For as the author has elsewhere shown, the profoundest facts in the earth's history prove that the oceans have always been oceans. These lands in all probability were, for the most part, volcanic islands or summits of volcanic ranges, for of this nature are all the islands over the interior of either ocean that are not of coral origin.

The course of argument leads us to the belief that a very large number of islands, more than has been supposed, lie buried in the ocean. Coral islands give us the location of many of these lands; but still we know little of the extent to which the earth's ranges of heights, or at least of volcanic peaks, have disappeared through oceanic subsidence. Recent dredgings and soundings have proved that the bottom of the oceanic basin has little of the diversity of mountain chains and valleys that prevail over the continents; and, through this observation (and also by the discovery that some ancient types of animal life, supposed to have been long extinct, are perpetuated there), they have afforded new demonstration of the proposition, above stated, that *the oceans have always been oceans.* But while the facts do not imply the existence deep in the ocean of many granitic mountain chains, they do teach that there are long ranges, or lines, of volcanic ridges and peaks, and some of these may be among the discoveries of future dredging expeditions. A range of deep-sea cones, or sunken volcanic islands, would be as interesting a discovery as a deep-sea sponge or coral, even if it should refuse, excepting perhaps a mere fragment, to come to the surface in the dredge.

We may also accept, with some confidence, the conclusion that atolls and barrier reefs originated in the same great balance-like movement of the earth's crust that gave elevation and cold, in the Glacial era, to high-latitude lands. If so, the tropics and the colder latitudes were performing their several works simultaneously in preparation for the coming era; and it is a gain to us in our contemplations, that we hence may balance the beauty and repose of the tropics, through all the progressing changes, against the prolonged scenes of glacial desolation that prevailed over large portions of the continents.

## ART. IX.—*On a precise Method of tracing the Progress and of determining the Boundary of a Wave of Conducted Heat;* by ALFRED M. MAYER, Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, N. J.

IN 1870 Meusel experimented on the formation of double iodides, and on the remarkable changes of color produced in these bodies by heat.* He prepared a double iodide of copper and mercury, by adding to a warm solution of mercuric iodide in potassium iodide, copper sulphate and then sulphurous acid; the resulting precipitate is of a brilliant carmine red and (in that experimented on by me) turns to a deep chocolate brown on heating to about 70° C. In order forcibly to exhibit this change of color, Boettger moistened the iodide with weak gum water, and painted it on paper; on heating the latter, the change of color is produced, and on cooling, the iodide regains its former brilliancy.

Dr. G. F. Barker had the kindness to present me with a card so prepared, and on experimenting with it I soon perceived the valuable means it afforded of tracing the progress and of determining the boundary of a wave of conducted heat. To Dr. Barker I am also indebted for the iodide used in the experiments I here present.

The first use I made of this substance was to track the heat conducted by bars and plates of metal,† and the sharpness of the boundary of the colors instigated me to test the value of this mode of experiment, by applying it to a determination of the elliptical contour of the isothermal of conduction, in the principal section of a quartz crystal.

Sénarmont, in his beautiful researches on this subject (Ann. de Ch. et de Ph., 3e S., t. xxi, xxii), has carefully determined the ratio of the axes of this elliptical figure, by coating a thin longitudinal section of the crystal with wax, and leading through it a silver wire, by means of which heat was brought

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* Ber. Berl. Chem. Ges., iii, 123, 1870. Bul. Soc. Ch., II, xiii, 220, 1870. J. Pr. Ch., II, ii, 136, Aug., 1870.

† The iodide is decomposed by contact with certain metals; these should be coated with a film of collodion, or electrotyped with copper before applying the iodide.

to the center of the plate, whence it was conducted outward, and its progress and isothermal contour determined by the melting of the wax. The following are Sénarmont's experiments on a plate $27^{mm}$ square, whose sides were parallel and perpendicular to the principal axis of the crystal:

| Exp. | Major Axis. | Minor Axis. | Ratios. |
|---|---|---|---|
| 1 | 12·50 | 9·75 | 1·28 |
| 2 | 11·60 | 8·50 | 1·35 |
| 3 | 10·00 | 7·50 | 1·33 |
| 4 | 12·00 | 9·00 | 1·33 |
| 5 | 13·75 | 10·00 | 1·37 |
| 6 | 18·00 | 14·00 | 1·29 |
| 7 | 15·00 | 12·00 | 1·25 |
| 8 | 9·75 | 7·50 | 1·30 |
| | | | 1·31 Mean Ratio. |

Sénarmont, in the above experiments, used every precaution to attain accurate results. He screened the plate from draughts of air and from radiations; kept the plate horizontal and frequently rotated it around its heated wire. After the ellipse had become constant in its form, he allowed the plate to cool, and then measured the axes of the ellipse by means of a micrometer.

In the experiments which follow, I used a quartz plate $27^{mm}$ long, $22^{mm}$ wide, and whose thickness was $1{\cdot}2^{mm}$. Its center of figure was pierced by a hole $1{\cdot}25^{mm}$ in diameter, through which passed the vertical conical end of a silver wire. The iodide was made into a paint with weak gum water, and in experiments 1, 2, 3 and 4 was applied to the surface of the plate by a camel's hair pencil. In experiments 5, 6, 7 and 8, the better plan was adopted of flowing the iodide over the plate, and allowing the water spontaneously to evaporate. Thus we obtain a smooth, evenly distributed coating, giving a sharp outline to the elliptical figure of the conducted heat. The plate was screened from radiations of the flame which heated the wire, but was not shielded from currents of air, nor was unequal radiation of the iodide specially prevented. The method of measurement was as follows: after the ellipse was well formed, and of permanent dimension, the extremities of its longer and of its shorter axes were marked by scratching through the iodide with a very slender steel point; the plate was then removed, and the lengths of the axes determined by means of dividers and a scale divided into half millimeters.

| Exp. | Major Axis. | Minor Axis. | Ratios. |
|---|---|---|---|
| 1 | 12·5 | 9·25 | 1·35 |
| 2 | 14·0 | 10·5 | 1·33 |
| 3 | 17·75 | 13·5 | 1·31 |
| 4 | 18·25. | 14·0 | 1·30 |
| 5 | 12·75 | 9·5 | 1·34 |
| 6 | 12·8 | 9·5 | 1·34 |
| 7 | 12·8 | 9·5 | 1·34 |
| 8 | 16·4 | 11·8 | 1·38 |
| | | | 1·33 Mean Ratio. |

An opinion on the relative values of the two modes of experimenting can only be formed from a discussion of the two series of observations by the method of least squares. It is true that the series are not as extended as one would wish for the application of this process, yet its results are equally fair for both. We thus have found that the—

| | | | | |
|---|---|---|---|---|
| Probable error of a single determination of ratios | in | S.'s | series is | ·0267 |
| " " " | " | M.'s | " | ·0170 |
| " in the mean ratio | " | S.'s | " | ·0094 |
| " " | " | M.'s | " | ·0060 |

From these figures we infer that Sénarmont's ratio is barely true to a hundredth, while my result can be relied on to that figure, and if my measures had been made with a micrometer microscope, on a plate protected from unequal radiation, and shielded from currents of air, I would have obtained a ratio reliable to the third decimal place.

To the higher ratio of my determination I attach no importance; I attribute it to the peculiarity of this particular crystal, for several measures on this plate, with a waxed surface, gave even a higher ratio than when the iodide was used. It hence appears that to obtain the correct ratio for a given crystal, the mean ratio derived from several plates should be adopted.

The remarkable change of color which heat produces in this iodide led me to hope that this molecular change would be accompanied by a simultaneous variation in its radiating power. To solve this problem, I made the following experiments, at different temperatures, below and above the degree at which its color changes. One side of a hot-water cube was coated with lamp-black, and another side with a thick paste of iodide and gum-water; after the latter had nearly dried, I sifted iodide over it, and caused this to adhere by rubbing it gently with my finger. The cube was now filled with water, in which was supported a thermometer. The water was raised to the following temperatures, and frequently agitated, so as to ensure a uniform heating of the cube. The deflections produced in the galvanometer needles by the lamp-black, and by the iodide, were then obtained for each fixed temperature. Each deflection given below is the mean of three experiments.

| Temp. | Lamp-black. | Iodide. | Ratio of Deflections. | Changes in Color. |
|---|---|---|---|---|
| 60° | 18·75 | 13·75 | 1 : ·70 | |
| 65° | 22·25 | 17· | 1 : ·71 | Cherry red, and turning in spots to chocolate color. |
| 68° | 22·75 | 16·25 | 1 : ·71 | Dark red, with spots of chocolate color. |
| 70° | 24·0 | 16·87 | 1 : ·70 | Whole surface of a deep brown. |
| 72° | 25·0 | 17·62 | 1 : ·70 | Deep purplish brown. |
| 75° | 26·25 | 18·62 | 1 : ·70 | " " |
| 100° | 45·0 | 30·5 | 1 : ·67 | " " |

The last experiment, in which the temperature of the surface was 100°, gave deflections so far exceeding those produced before that I sought to render them comparable by removing the hot water cube to a greater distance from the thermo-battery, when I obtained the following ratio:

| Temp. | Lamp-black. | Iodide. | Ratio. |
|---|---|---|---|
| 100° | 20° | 13·41° | 1 : ·67 |

The result was the same ratio as formerly obtained.

These experiments seem to show that the molecular change in the iodide, which causes it to act so differently in reflecting light, does not appear to have any action on its power of radiating the rays of heat of low intensity. I intend, however, to return to this investigation, provided with an apparatus giving the differential actions of two cubes, and having a carefully calibrated galvanometer, and with this arrangement to test the reflecting as well as the radiating power of this and other iodides.

Several applications of this iodide for showing elevations of temperature will naturally present themselves; for example, Foucault's experiment of the heating of a copper disc, when rotating in the magnetic field, can be exhibited to a large audience by painting the disc with this iodide; on the disc attaining 70° C., the brilliant scarlet will change to a deep brown, to regain its former brilliant hue on cooling.

A more useful application may be made of this, or of several other more appropriate metallic compounds, by painting them on the *pillow-blocks*, and other parts of machines which are liable to injurious heating from friction. Thus the machinist can, from the colors of these paints, ascertain the temperature of these sometimes inaccessible parts of moving machines.

May 20, 1872.

## ART. X.—*Remarks on the late Criticisms of Prof. Dana;* by T. STERRY HUNT, LL.D., F.R.S.

IN this Journal for February last (p. 86) Prof. Dana has criticized certain points in my address "On the Geognosy of the Appalachians and the Origin of Crystalline Rocks," given in August, 1871, at Indianapolis, before the American Association for the Advancement of Science. I am charged by him with rejecting, for many mineral silicates, the view that they are pseudomorphs; that is to say, crystals chemically altered without loss of external form. I have denied that crystals of serpentine having the shape of chrysolite, pyroxene, dolomite, etc., and crystals of pinite having the shapes of nepheline or scapolite, are results of a chemical change of these species; notwithstanding this view is now held by most mineralogists on the grounds of similarities of geometrical form and the existence of what are regarded as intermediate stages in the process of transmutation; and I have maintained another and a very different view, which, in my opinion, is more rational. Until we can watch the transmutation of one of these species into another, the argument from supposed intermediate forms is worth no more in the mineral than in the organic world; the reasoning of the transmutationists, in the one case and the other, resting upon somewhat similar considerations. In either case we may say, with Prof. Warrington Smyth, that in these intermediate forms "lie the materials for a history;" while we venture, with him, to express a doubt whether, from a series of specimens supposed to show a transition from chrysolite to serpentine, or from hornblende to chlorite, "we are obliged to conclude that there has been, *historically speaking*, an actual transition from the one to the other." [See his anniversary address, as president of the Geological Society of London, in 1867.]

Prof. Dana says that Scheerer is the only one who shares my peculiar views on this question. I have, however, asserted in my address that Delesse has maintained the views of Scheerer and myself, as opposed to the popular doctrine of epigenesis, and shall endeavor to make good my assertion. In his essay on Pseudomorphs, published in 1859 [Ann. des Mines, V, xvi, 317–392], Delesse begins his argument by remarking that since, in some cases, a mineral is found to be surrounded by another clearly resulting from its alteration (as for example anhydrite by gypsum), certain mineralogists have supposed that wherever one mineral encloses another there has been epigenesis or pseudomorphous alteration. Such, he says, may sometimes be the case, but it is easy to see that it is not so habitually. A crystallized mineral species frequently includes a large and even a predominating portion of another, and the combination is then considered by many as an example of partial pseudomorphous alteration. In such instances, remarks Delesse, the question arises whether we have to do with the results of envelopment, or of chemical alteration; to resolve which it becomes necessary to study carefully the problem of envelopment. He then proceeds to show that the enveloped substance is, in some cases, crystalline (and arranged either symmetrically or asymmetrically with regard to the enveloping mass), and in other cases amorphous, and enclosed like the sand-grains which predominate in the calcite crystals of Fontainebleau. The difficulty in deciding whether we have to do with envelopment or with epigenesis increases when the enveloped mineral becomes so abundant as to obscure the enveloping species, or when it becomes mixed with it in so intimate a manner as to seem one with the latter, (*se fondre insensiblement avec lui*). The proportions of the enveloped and the enveloping mineral, we are told, may so far vary that the one or the other is no longer recognizable. "As the forces which determine crystallization have a great energy, the enveloping mineral is sometimes found in so small a quantity as to be entirely masked by the enveloping species." "When minerals have crystallized simultaneously, they have been able to become associated with each other, and to envelop each other, in all proportions" [loc. cit., pages 338, 339, 341, 353].

Our author then proceeds to tell us that having carefully studied, in numerous specimens, the supposed mica-pseudomorphs of iolite, andalusite, cyanite, pyroxene, hornblende, etc., he regards them, as in all cases, examples of envelopment, and expresses the opinion that we must omit from our lists a great number of the so-called pseudomorphous minerals, especially among the silicates. The final result of the process of envelopment is, according to Delesse, this—to give rise to mixed mineral aggregates, owing their external forms to the crystallizing energy of one of the constituents, which may be present in so small a quantity as to be completely obscured by the other matter present. From this condition of things, result crystalline forms which, though totally different in their origin from the products of chemical alteration or substitution, are emphatically pseudomorphs.

From this process of mechanical and more or less heterogeneous envelopment, Delesse next proceeds to consider the crystallizing together of isomorphous or homœomorphous species, in relation to the generally received notion of epigenic pseudomorphism. He declares that "isomorphism explains very well facts which are often attributed to pseudomorphism," and that many "minerals which are still considered pseudomorphs are in reality examples of isomorphism" [pages 364, 365]. Referring to the well-known investigations of Mitscherlich upon the crystallizing together, in all proportions, of isomorphous species, and of the symmetrical crystallization of one salt around a nucleus of another isomorphous with it, Delesse suggests that the different forms and varieties of hornblendic and pyroxenic minerals afford many examples of the kind. He then adds, "If, as Scheerer has remarked, water plays in silicates the part of a base, anhydrous silicates may crystallize at the same time with hydrated silicates, and moreover be isomorphous with them." In this way, he suggests, we may explain by isomorphism, or homœomorphism, the association with pyroxene of the hydrous species, schiller-spar, as well as that "of various anhydrous and hydrated minerals" [pages 357, 358].

In further illustration of the words just quoted from Delesse, we may cite from Scheerer, as examples of what he called polymeric isomorphism, the association (in the same crystals) of iolite and aspasiolite, and of chrysolite and serpentine. If these, and similar species, crystallize together because they are isomorphous, it is evident that they may each crystallize separately; and thus the crystals of serpentine with the form of chrysolite, and those of aspasiolite and other so-called hydrous iolites, may be regarded as examples not of epigenesis, but of isomorphism.

We have thus endeavored to set forth, chiefly in his own words, the views enunciated in 1859 by Delesse, according to whom the phenomena of so-called pseudomorphism among mineral silicates are to be explained, for the most part, not by chemical alterations of pre-existing species but by envelopment and by isomorphism. That the above are really his views, and are moreover regarded by himself as contrary to those of the school which I oppose, Delesse does not permit us to doubt; for after having set them forth as his own, (*après avoir exposé notre manière de voir*), he says, "We hasten to add that these facts may also be explained in a manner altogether different (*peuvent aussi s'interpreter d'une manière toute différente*); and some savants of Germany, notably G. Rose, Haidinger, Blum, G. Bischof and Rammelsberg have sought their explanation in pseudomorphism. Their example has been followed by most mineralogists, etc." [pages 358, 359].

That the "pseudomorphism" of the authors just named is chemical alteration or epigenesis, it is not necessary to remind the reader; who will now be able to judge whether it is Prof. Dana or myself who has misrepresented or misunderstood Delesse. Let us, however, add that the long and somewhat diffuse memoir of the latter, from which we have quoted, is wanting in unity of plan and purpose; and that parts of it, if we may hazard a conjecture, seem to have been written while he still inclined to the views of the opposite school. From the table of pseudomorphs which he has given, and from many passages in the text, it might be inferred that he then held the notions of Rose, Haidinger, etc., which he elsewhere, in the same paper, speaks of as being entirely different from his own. The views of Delesse, about this time, underwent a great change, which has a historic importance in connection with those which I advocate. When, in 1857 and 1858, he published the first and second parts of his admirable series of studies on metamorphism, Delesse held, in common with nearly every geologist of the time, to the eruptive origin of serpentine and the related magnesian rocks. Serpentine was then classed by him with other "trappean rocks;" and he elsewhere asserted that "granitic and trappean rocks" undergo in certain cases a change, near their contact with the enclosing rock, by which they lose silica, alumina and alkalies, and acquire magnesia and water, being thus changed into a magnesian silicate; which may take the form of saponite, serpentine, talc or chlorite. [Ann. des Mines, V, xii, 509; xiii, 393, 415]. It would be difficult to state more distinctly the view, which he then held, of the origin of these magnesian rocks and minerals by the chemical alteration of plutonic (granitic and trappean) rocks. This was in 1858, and in 1859 appeared the memoir on pseudomorphs, already noticed, in which, in place of the theory of epigenic pseudomorphism or chemical alteration of various mineral silicates, taught by the German school, he brought forward, in explanation of the facts upon which this was based, another theory, which was only an extension of that already maintained by Scheerer and myself.

It was not until 1861 that Delesse published the last part of his studies on metamorphism, which appeared in the Memoirs of the Academy of Sciences of France (vol. xvii), and in it we find that, consistently with the new views adopted by him in 1859, the old doctrine of the epigenic origin of serpentine and the related magnesian rocks from the alteration of plutonic rocks, is abandoned. In its stead, it is here suggested by Delesse that all these magnesian rocks result from the crystallization of the sepiolites or so-called magnesian clays, which are frequent in many sedimentary deposits. These, according to him, by a molecular re-arrangement of their elements, may give rise to serpentine, talc, chlorite, and their various associated and related minerals. The rocks thus generated are still declared to pass insensibly into plutonic rocks, but instead of maintaining, as in 1858, that they are derived from the latter, Delesse, in 1861, asserts, on the contrary, that "the plutonic rocks are formed from the metamorphic rocks, and represent the maximum of intensity, or extreme limit of metamorphism."

This recognition of the notion that the great masses of serpentine, with their constantly associated hornblendic, talcose and chlorite rocks, have been directly formed from the molecular re-arrangement or *diagenesis* of aqueous magnesian sediments, and not from the chemical alteration, or *epigenesis* of erupted plutonic masses, marks a complete revolution in our views of the history of the crystalline rocks. The new doctrine did not, however, originate with Delesse, but was previously put forward by myself in a paper "On Some Points of Chemical Geology," read before the Geological Society of London in January, 1859, appearing in abstract in the *Philosophical Magazine* for February, and published at length in the *Geological Journal* for November, in the same year. I there maintained that serpentines were "undoubtedly indigenous rocks, resulting from the alteration of silico-magnesian sediments;" and moreover asserted that the final result of heat, aided by water, on such rocks, would be their softening, and, in certain cases, their extravasation as plutonic rocks; which were regarded "as, in all cases, altered and displaced sediments." When this paper was written, in 1858, I still supposed that the reactions between the elements in beds of siliceous magnesian carbonates (which, I had shown, may give rise to certain magnesian silicates in immediate proximity to eruptive rocks) might serve to explain the origin of great areas of serpentine and related crystalline magnesian silicates; but my studies of the silicates deposited during the evaporation of natural waters, and of the magnesian sediments of the Paris basin, soon led me to seek the origin of these rocks in the alteration of previously formed uncrystalline magnesian silicates. This view was set forth by me in this Journal for March, 1860 [II, xxix, 284] and more fully in the *Canadian Naturalist* for June, 1860 [also in this Journal, xxxii, 286], where it was pointed out that steatite, chlorite and serpentine were probably derived from sediments similar to the magnesian silicates found among the tertiary beds in the vicinity of Paris, the so-called magnesian clays.

We have seen that these various novel views, put forth by me in 1859 and 1860, though totally different from those taught by Delesse in 1858, were integrally adopted by him in 1861. These dates are circumstantially given in my address of last year, and yet Prof. Dana, in his review of it, charges me with "following nearly Delesse" as to the origin of serpentine. He also asserts that I "make Delesse the author of the theory of envelopment," when I have there declared that the view of Delesse—"that the so-called cases of pseudomorphism, on which the theory of metamorphism by alteration has been built, are, for the most part, examples of association and envelopment, and the result of a contemporaneous and original crystallization,—is identical with the view suggested by Scheerer in 1846, and generalized by myself, when, in 1853, I sought to explain the phenomena in question by the association and crystallizing together of homologous and isomorphous species." To Delesse therefore belongs the merit not of having suggested the notion of envelopment in this connection, but of having pointed out the bearing of the envelopment of heteromorphous and amorphous species on the question before us.

Prof. Dana moreover asserts that while Scheerer is the only one who maintains similar views to myself, I, in common with all other chemists, reject the chemical speculations which lie at the base of his views. On the contrary, unlike most chemists, who have failed to see the great principle which underlies Scheerer's doctrine of polymeric isomorphism, I have maintained [this Journal, II, xv, 230 ; xvi, 218] that it enters into a general law, in accordance with which bodies whose formulas differ by $nM_2O_2$ or $nH_2O_2$, may (like those differing by $nH_2C_2$), have relations of homology, and moreover be isomorphous. The existence of these same relations was further maintained and exemplified in a paper on Atomic Volumes, read by me before the French Academy of Sciences and published in the *Comptes Rendus* of July 9, 1855. This view, which I have never repudiated, is reiterated in my address last year, and declared to include the polymeric isomorphism of Scheerer.

Prof. Dana next says that in asserting that "the doctrine of pseudomorphism by alteration, as taught by G. Rose, Haidinger, Blum, Volger, Rammelsberg, Dana, Bischof and many others, leads them * * * to maintain the possibility of converting almost any silicate into any other," I have, in his language, "grossly misrepresented the views of at least Rose, Haidinger, Blum, Rammelsberg and Dana," and that I "complete the caricature" by this sentence, to be found in my address: "In this way we are led from gneiss or granite to limestone, from limestone to dolomite, and from dolomite to serpentine; or more directly from granite, granulite or diorite to serpentine at once, without passing through the intermediate stages of limestone and dolomite;"—"part of which transformations," says Prof. Dana, "I, for one, had never conceived; and Rose, Haidinger, Rammelsberg, and probably Blum, and the 'many others,' would repudiate them as strongly as myself." The '*many others*,' as he rightly remarks, are "other writers on pseudomorphism," among whom it would be unjust not to name their progenitor, Breithaupt, von Rath and Müller, at the same time with Volger and Bischof. According to Prof. Dana, I "add to the misrepresentation by means of the strange conclusion that because such writers hold that crystals may undergo certain alterations in composition, therefore they believe that rocks of the same constitution may undergo the same changes." This "*strange conclusion*" I have always supposed to be Prof. Dana's own. No one has perhaps asserted it so clearly or so broadly as himself, and I shall therefore quote his own words in my justification. As early as 1845, in an article entitled "Observations on Pseudomorphism," [this Journal I, xlviii, 92] he wrote: "The same process which has altered a few crystals to quartz has distributed silica to fossils without number, scattered through rocks of all ages. The same causes that have originated the steatitic scapolites, occasionally picked out of the rocks, have given magnesia to whole rock-formations, and altered, throughout, their physical and chemical characters. If it be true that the crystals of serpentine are pseudomorphous crystals, altered from chrysolite, it is also true, as Breithaupt has suggested, that the beds of serpentine containing them are likewise altered; though often covering square leagues in extent, and common in most primary formations. The beds of steatite, the still more extensive talcose formations, contain everywhere evidence of the same agents." Again, in 1854, in his *Mineralogy*, 4th edition, [page 226], Prof. Dana, after a complete list of pseudomorphs, compiled from the writers of the school in question, says: "These examples of pseudomorphism should be understood as cases not simply of alteration of crystals, but in many instances of changes in beds of rock. Thus all serpentine, whether in mountain-masses, or the simple crystal, has been formed through *a process of pseudomorphism, or in more general language, of metamorphism;* the same is true of other magnesian rocks, as steatitic, talcose or chloritic slates. *Thus the subject of metamorphism, as it bears on all crystalline rocks, and of pseudomorphism, are but branches of one system of phenomena.*" If there could be any doubt as to the meaning of the words which we have italicized, in quoting them from Prof. Dana, it is removed by his language in 1858. Then, as now, adversely criticising my views on this question, he refers to the statements, above cited, made in 1845 and 1854, as expressions of his doctrine, mentioning especially the first one, in which, he says, "*metamorphism is spoken of as pseudomorphism on a broad scale.*" [This Journal, II, xxv, 445]. I confess that I do not understand Prof. Dana, when in his last criticism of me, fourteen years after the one just quoted, he reproaches me with having charged him with holding the doctrine that "*regional metamorphism is pseudomorphism on a grand scale;*" and declares that he makes no such remark, neither expresses the sentiment in his Mineralogy of 1854.

With these citations before us, and remembering the views Scheerer, and the later ones of Delesse, together with the language of the latter in his essay on Pseudomorphs, let us notice the words of Naumann, addressed to Delesse in 1861, in allusion to the essay in question. "Permit me to express to you my satisfaction for the ideas enunciated in your memoir on Pseudomorphs, ideas which my friend Scheerer will doubtless share with myself" (*idées que mon ami M. Scheerer partagera sans doute comme moi-même*). Then follows the language which I have quoted in my address, in which he combats the error of those who hold that gniesses, amphibolites, and other crystalline rocks are "the results of metamorphic epigenesis, and not original rocks," and adds, "It is precisely *because pseudomorphism has so often been confounded with metamorphism* that this error has found acceptance." [Bull. Soc. Geol. de Fr., II, xviii, 678]. The reader must now judge whose opinions it is that are here denounced as erroneous, and whether Naumann was on the side of Prof. Dana, or, with Delesse, on the side of Scheerer and myself. I insist the more strongly on this matter, because Prof. Dana not only declares that Delesse and Naumann have always avowed the doctrines of the transmutationist school, and do not in any way whatever countenance my views, but implies that I have dealt unfairly with these authorities. In regard to another point raised by Prof. Dana, I may remark that their explicit declarations are not to be set aside, because traces of the doctrine of epigenic pseudomorphism still hold a place in the last edition of Naumann's *Mineralogy*, or in the *Revue de Géologie*, of which Delesse is one of the editors.

Prof. Dana says "If there was any occasion for a notice of my opinions, a critic of 1871 should have referred to the formal expression of them in my *Manual of Geology*, first published in 1863. The reader will there find the *diagenesis* of Gümbel, which Mr. Hunt takes occasion to commend, * * * with but a brief allusion to pseudomorphism." The doctrine of diagenesis, it is hardly necessary to say, I have never attributed to Gümbel, nor does he claim it. It is the old doctrine of Hutton, Playfair and Boué, is taught by Bischof [Chemical Geology, III, 318, 325, 342], and pervades my papers of 1859 and 1860, already referred to. But while it has been generally admitted that what, in my address, I have called the first class of crystalline rocks (consisting chiefly of quartz and aluminous silicates) might result from the molecular re-arrangement of the elements of clay and sand-rocks, I maintained in those papers that what I have called the crystalline rocks of the second class (in which protoxide silicates predominate) have been generated, by a similar process, from deposits of chemically-formed silicates. This view being adopted by Delesse and by Gümbel to explain the origin of the various magnesian silicated rocks, hitherto generally regarded as the product of epigenesis, the latter has proposed to designate the process as diagenesis; a term which I adopt, as one well fitted to denote the generation of all kinds of crystalline rocks through a molecular re-arrangement of sedimentary deposits, of whatever origin. Prof. Dana, in common with most other geologists, admits in his *Manual* the production by diagenesis of the rocks of the first class, but in the case of serpentine and steatite declares them to have been formed by epigenic pseudomorphism or chemical alteration of pyroxenic and other crystalline rocks; *the origin of which is left entirely unexplained.* It is true that his allusions to pseudomorphism in this volume are confined to very brief notices, on pages 704 and 710; a fact which is the more noticeable, when we recall that the author had formerly expressed the belief "that pseudormorphism will soon constitute one of the most important chapters in geological treatises." [This Journal, I, xlviii, 66.] That Prof. Dana has receded from the extreme views on this subject which he maintained from 1845 to 1858, and which I have constantly opposed, seems probable; but until he formally rejects them, the student of geology will not unnaturally suppose that he still gives the sanction of his authority to the doctrine which he once taught, without any qualification, but now repudiates, that "*metamorphism is pseudomorphism on a broad scale.*"

Prof. Dana having clearly defined the proposition that the chemical alterations which are recognized in individual crystals are to be conceived as extending to rock-masses; and having moreover asserted that the principle of the identity of metamorphism and pseudomorphism "bears on all crystalline rocks," is logically committed to all the deductions as to the changes of rocks which the transmutationist school has drawn from the supposed alteration of minerals. By reference to the table of pseudomorphs in the fourth edition of Dana's Mineralogy, it will be seen that each one of the metamorphoses of rocks mentioned in the above extract from my address is based upon an asserted epigenic change or conversion of the constituent species. I shall however show, in addition, that in each case the application of the principle to rock-masses has been recognized by one or more of the authorities already named, and that the so-called caricature has been drawn by their own hands. It would be easy, did space permit, to extend greatly this list of supposed transmutations. The various associations of rocks and minerals in nature, when interpreted according to the canons of this school, seem in fact, as remarked by Prof. Warrington Smyth, in his address already quoted, "to offer a premium to the ingenious for inventing an almost infinite series of possible combinations and permutations." Before proceeding further it is to be noted that no distinction can, in many cases, be established between the results of alteration (or partial replacement) and substitution (or complete replacement); since successive alterations may give the same product as direct substitution. Thus, for example, quartz might be directly replaced by calcite, or else first altered to a silicate of lime, which, in its turn, might be changed to carbonate. The alteration of quartz to a silicate of magnesia, and that of both pyroxene and pectolite to calcite, is maintained by the writers of the present school.

Metamorphosis of granite or gneiss to limestone. Calcite, we are told, is pseudomorphous of quartz, of feldspar, of pyroxene, and of garnet, besides other species: it moreover replaces both orthoclase and albite "by some process of solution and substitution." [Dana's Mineralogy, 5th edition, 361.] Since quartz, orthoclase and albite can be replaced by calcite, the transmutation of granite or gneiss into limestone presents no difficulty. I cannot, at present, give the reference to the statement of Volger that some gneissoid limestones owe their origin to such a process.

Metamorphosis of limestone to dolomite. This change is maintained by von Buch, Haidinger and many others. I am blamed for mentioning in connection with this school the name of Haidinger, who, Prof. Dana says, "never wrote upon the subject of the alteration of rocks." It has, however, never before been questioned that Haidinger was the first, if not to suggest, to clearly set forth the theory of the supposed conversion of limestone into dolomite by the action of magnesian solutions, aided by heat and pressure; a theory which I have elsewhere refuted. [Bischof, Chem. Geology, iii, 155, 158; Zirkel, Petrographie, i, 246; Liebig and Kopp, Jahresbericht, 1847–48, 1289, and this Journal, II, xxviii, 376].

Metamorphosis of dolomite to serpentine. This change is maintained by G. Rose [Bischof, Chem. Geol., ii, 423], and by Dana [this Journal, III, iii, 89].

Metamorphosis of granite, granulite, and eclogite directly into serpentine, chlorite and talc. These transmutations are maintained by Müller, and adopted by Bischof. [Chem. Geol., ii, 424, 434.]

Metamorphosis of limestone to granite or gneiss. This is taught by Blum and Volger. [Ibid., ii, 186; iii, 431.]

Having thus given the authorities for the examples cited in my address, I may notice some further illustrations of the doctrine from the pages of Bischof's work already quoted. Metamorphosis of diorite, hornblende-rock and labradorite to serpentine; G. Rose, Breithaupt, von Rath [ii, 417, 418]. Diorite and hornblende-slate to talc-slate and chlorite-slate; G. Rose [iii, 312]. Mica-slate to talc-slate, and steatite and mica to serpentine, steatite and talc; Blum, C. Gmelin [ii, 405, 468]. Quartz-rock to steatite; Blum [ii, 468].

With regard to New England rocks, Prof. Dana asserts that "there are gneisses, mica-schists, and chloritic and talcoid schists in the Taconic series." I have, however, shown in my address that Emmons, the author of the Taconic system, expressly excluded therefrom the crystalline rocks, which he included in an older primary system; excepting, however, certain micaceous and talcose beds, which he declared to be recomposed rocks, made up from the ruins of the primary schists, and distinguished from these by the absence of the characteristic crystalline minerals which belong to the Green Mountain primary schists.

Again, Prof. Dana states that I make the crystalline schists of the White Mountains a newer series than the Green Mountain rocks. A careful perusal of my address will show that I nowhere assert that the rocks of the third series, on my line of section, are younger than the second series. Such a view of their relations has, however, been maintained for the last generation by the Messrs. Rogers, Logan, and many others, all of whom assigned the crystalline schists of the White Mountains to a higher geological horizon than the Green Mountains. In support of this view of their relative antiquity, I have, it is true, brought together observations from South Carolina, Pennsylvania, Michigan, Ontario, and Maine, all of which point to the same conclusion; and I might now add similar evidence from New Brunswick and from Nova Scotia. My "chronological arrangement" of New England crystalline rocks, as it is called by Prof. Dana, so far as it is my own, is limited to my affirmation that they are all of pre-Cambrian age; in proof of which it need only be mentioned that the crystalline schists of both the types in question are, in southern New Brunswick, directly overlaid by uncrystalline shales, sandstones and conglomerates, made up in part of the ruins of these, and holding a Cambrian (Menevian) fauna.

As regards the mica-schists with staurolite, cyanite, andalusite and garnet, I have in my address pointed out the fact that they appear to belong to a great series of rocks, very constant in character, which have a continuous outcrop from the Hudson river to the St. John, a distance of 500 miles, and, in the latter region are clearly pre-Cambrian. I have moreover brought together the evidence of observers in other parts of North America, in Great Britain, in continental Europe, and in Australia, showing that similar crystalline schists, holding these same minerals, always occupy, in these regions, a similar geological horizon. Prof. Dana hereupon inquires whether any one has yet *proved* that these mineral characters are restricted to rocks of a certain geological period. I answer, that in opposition to these facts, it has not yet been proved that they belong to any later geological period than the one already indicated; and that it is only by bringing together observations, as I have done, that we can ever hope to determine the geological value of these mineral fossils. In no other way did William Smith prove, in Great Britain, the value of organic fossils, and thus lay the foundations of paleontological geology.

Montreal, April, 1872.

### ART. XI.—*On the Meteors of April 30th–May 1st;* by Professor DANIEL KIRKWOOD.

PROFESSOR SCHIAPARELLI, in his list of meteoric showers whose radiant points are derived from observations made in Italy within the last few years, describes one as occurring on April 30th and May 1st, the apparent position of whose radiant is in the Northern Crown, R. A. 237°, N. P. D. 55°. The same shower has also been recognized by Robert P. Greg, F.R.S., of Manchester, England. This meteor-stream, it is now proposed to show, is probably derived from one much more conspicuous in ancient times.

In Quetelet's *Physique du Globe*, pp. 290–297, we find meteoric displays of the following dates. In each case the corresponding day for 1870 is also given,* in order to exhibit the close agreement of the epochs.

1. A. D. 401, April 9th; corresponding to April 29th, for 1870.
2. " 538, " 6th; " April 25th, "
3. " 839, " 17th; " May 1st, "
4. " 927, " 17th; " April 30th, "
5. " 934, " 18th; " May 1st, "
6. " 1009, " 16th; " April 28th, "

The epochs of 927 and 934 suggest as probable the short period of 7 years. It is found accordingly that the entire interval of 608 years—from 401 to 1009—is equal to 89 mean periods of 6·8315 years each. With this approximate value the six dates are all represented as follows:

From A. D. 401 to A. D. 538, 20 periods of 6·85 years.
" 538 to 839, 44 " 6·84 "
" 839 to 927, 13 " 6·77 "
" 927 to 934, 1 " 7·00 "
" 934 to 1009, 11 " 6·82 "

This period corresponds approximately to those of several comets whose aphelion distances are somewhat greater than the

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* Making proper allowance for the precession of the equinoxes.

mean distance of Jupiter. So long as the cluster occupied but a small arc of the orbit, the displays would evidently be separated by considerable intervals. The two *consecutive* showers in the tenth century indicate, however, an extensive diffusion of the cluster at that epoch; so that the *preceding* part passed the node April 30th, 927, and the *following* part May 1st, 934. The comparative paucity of meteors in modern times may be particularly explained by the fact that the ring has been subject to frequent perturbations by Jupiter.

It is not impossible that this meteor-stream was connected in its origin with the comet which passed its perihelion about the 29th of April, B. C. 136.

## ART. XII.—*On the Tertiary Basin of the Marañon;* by CH. FRED. HARTT, A.M.

ON the 12th of December, 1867, Prof. James Orton of Vassar College, on his journey down the Marañon, or Peruvian Amazonas,[*] spent a few hours at Pebas, a little village on the Ambayacú a mile from the left bank of the Marañon, and some 50–60 miles below the mouth of the Napo. Here he collected several species of fossil shells, but strangely neglected to observe the mode of their occurrence. In announcing his discovery in the Geological Magazine,[†] he says that "the shells are all found in the colored plastic clays which stretch unbroken from the foot of the Andes to the Atlantic." In the American Journal of Conchology,[‡] he speaks of them as occurring "in that peculiar formation of fine laminated colored clays which is spread over the entire valley of the great river, and which Prof. Agassiz had

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[*] As this river is called by some writers the Amazon, Rio Amazon, Amazons or Rio Amazons, I have preferred to use the Portuguese form. If one uses either Amazon or Amazons there is no propriety in prefixing the Portuguese word Rio. The name of the river is Rio das Amazonas, the river of the Amazons; but in Brazil it is commonly spoken of as *o Amazonas* or the Amazons. I have simply followed the rule of not attempting to translate South American names. The name Marañon, about which there has long been so much discussion, is evidently the same as the Tupí word *paraná*, which means *a river*; Maranhâo or Maranham is the Portuguese form for Marañon. M and p are interchangeable consonants, as may be seen in a great number of words in modern Lingua geral, as *morandú* or *porandú* to question, *puraçêi* or *muraçêi* to dance, etc. The terminal vowel of *paraná* is often more or less nasal; hence the Brazilian geographical names *Paranán,, Juparanda.* That Marañon or Maranhâo (Maranyáw$_g$) are derived from paraná (or maraná ?) is all the more probable as this is the name given by the Indians of the Amazonas, and not only by those who speak modern north Tupí or Lingua geral, but also the Omagua, Cocáma and other languages. Ask an Indian who does not speak Spanish or Portuguese the name of the Amazonas, and he answers *Paraná*. But to ears unaccustomed to the language it may sound like Marañon.

[†] Henry Woodward. The Tertiary shells of the Amazons Valley, from Ann. and Mag. Nat. Hist. for Jan. and Feb., 1871, p. 6.

[‡] *Conrad*, Desc. of new fossil shells of the Rio Amazon, published in advance, Oct. 10, 1870.

pronounced drift;" while in his "Andes and Amazon" he simply says that the fossils occurred in a fossiliferous bed intercalated between the variegated clays so peculiar to the Amazon,"* and that "interstratified with the clay deposits are seams of a highly bituminous lignite.†" Prof. Orton therefore leaves it to be inferred that the Pebas beds are traceable down the whole length of the Amazonas. Mr. Henry Woodward, in the paper just quoted in a foot note, says that the Pebas clays are "evidently Bed II. of Prof. Agassiz's section.‡" Prof. Orton submitted his fossils to Mr. Gabb, who described and figured them§ under the names of *Neritina pupa*, *Turbonilla minuscula*, *Mesalia Ortoni*, *Tellina Amazoniensis*, *Pachydon obliqua* and *P. tenua*. In Mr. Gabb's opinion these remains indicated a fauna of Tertiary age. On the strength of this opinion Prof. Orton ventured to attack Prof. Agassiz's theory of the glacial origin of the valley of the Amazonas, laying stress on the fact that the shells occur well preserved, in place, and "showing" no indication of a "grinding glacier."

Under the instructions of Prof. Orton, Mr. Hauxwell, an intelligent naturalist, resident some 30 years on the Amazonas, made larger and more complete collections of these shells and found the fossiliferous beds elsewhere on the Marañon, especially at Cochaquinas on the southern side of the river. These collections were placed in the hands of Mr. Conrad, who described them, distinguishing ten species of gasteropods and six of lamellibranchs, referring all the latter to the genus *Pachydon* (*Anisothyris* Conrad). More recently Mr. Hauxwell sent large collections to England. Those in the possession of Mr. Janson of London were examined by Mr. Henry Woodward of the British Museum, and form the subject of the paper already twice referred to. Mr. Woodward makes several changes of nomenclature, and describes two new species. The list of the Pebas fossils now stands as follows:

GASTEROPODA.

*Isœa* Conrad (freshwater ?)
  *I. Ortoni* Conrad=*Mesalia Ortoni* Gabb.
  *I. lintea* Conrad.
*Liris* Conrad (freshwater ?)
  *L. laqueata* Conrad.
*Ebora* (freshwater or marine ?)
  *E. crassilabra* Conrad.
*Nesis* (sub-genus of *Ebora*).
  *N. bella* Conrad.
*Hemisinus* Swainson (freshwater !)
  *H. sulcatus* Conrad.
*Dyris* Conrad.
  *D. gracilis* Conrad.
*Neritina* Lamarck (fresh and brackish water).
  *N. pupa* Gabb (=*N. Ortoni* Conrad ?)
*Bulimus Scopoli* (land).
  *B. linteus* Conrad.
*Turbonilla* Risso.
  *T. minuscula* Gabb.
*Odostomia ?* Woodward (brackish water).

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* Andes and Amazon, 282.

† Op. cit., 283.

‡ Bull. de la Soc. Géol. de France, 2$_d$ Série, T. xxv, p. 685. Hartt, Geol. and Phys. Geog. of Brazil, p. 487.

§ Amer. Jour. Conch., vol. iv.

LAMELLIBRANCHIATA.

- *Anodon* Cuvier (fresh water).
  - *Anodon Batesii* Woodward.
- *Anisothyris* Conrad (=*Pachydon* Gabb) (brackish water).
  - *A. tenuis* (=*Pachydon tenuis* Gabb, *P. tenuis* Conrad, *Anisothyris Hauxwelli* Woodward);[*] of this species Mr. Woodward distinguishes two varieties: *α*, *distorta*, *β*, *crassa*.
  - *A. ovata* Conrad.
  - *A. carinata* Conrad.
  - *A. obliqua* Conrad.
  - *A. erecta* Conrad.
  - *A. cuneata* Conrad.
- *Tellina*.
  - *T. Amazoniensis* Gabb.

In the summer of 1871 I met Mr. J. B. Steere, a graduate of Michigan University, who was traveling on the Amazonas, making natural history collections. We spent more than a month together, and I took him over my old ground at Ereré and Monte Alegre. As he was about to visit the upper Amazonas, I gave him instructions to examine the Pebas locality, make a geological section, showing the character and arrangement of the beds, and collect carefully the fossils. Under date of Jan. 26th of this year, he has written me an account of his visit to the locality in question, and has sent some interesting notes which give us for the first time a clear idea of its geological structure, and of the conditions under which the fossils are found.

Mr. Steere says that a short distance below Tabatinga,[†] which, it will we remembered, is just on the boundary line separating Brazil from Peru, he saw "horizontal beds of blue clay, with veins of clayey coal dividing them. These veins of coal seem to vary much in thickness and appearance in a distance of a few hundred yards." This series of deposits he claims to be the same as that affording fossils at Pebas. My good friend Dr. Pimentel, Major in the Brazilian army and one of the engineers on a late Brazilio-Peruvian Boundary Commission, stationed at Tabatinga, had previously found, just above this town, a heavy bed of lignite, specimens of which he sent me.

Mr. Steere first saw signs of fossils in clay beds just above Loreto, a little place on the left bank of the Marañon some 30 miles above Tabatinga; but owing to the shortness of the stop of the steamer, he was unable to examine the locality with care. He describes the country below Pebas as low and less than a hundred feet above river level, i. e., during the dry season. The fossiliferous clay beds lie near the level of the river, but they are covered by 20–30 feet of red clay which he compares to the superficial clays so common on the lower Amazonas. Pebas, as already stated, is situated on the left bank of the Rio Am-

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[*] I sympathize with the wish to show honor to so deserving a gentleman as Mr. Hauxwell, but the change of the specific name from *tenuis*, however inappropriate the term may be, to *Hauxwelli*, is unwise and inadmissible.

[†] Tabatinga is the name given to the white feldspathic clay common all over Brazil. *Tauá*, Tupí, *taba* Portuguese form, is a yellowish clay; *tinga* means white.

bayacú, a mile above its confluence with the Marañon. Two miles below the mouth of the Ambayacú is Old Pebas. Both sites are on the high *tierra firme.** The right bank of the Amazonas opposite the Ambayacú is recent and low, but farther down the *tierra firme* appears, and Pichana is situated upon it.

The bank on which Pebas stands, Mr. Steere says, is about 100 feet high, that is during the dry season. In front of the village the lower strata are hidden from view by a slope of rubbish, but the upper are quite well exposed. The following section was made by Mr. Steere in a ravine near the road leading up the bank. The order is ascending.

I. The lowest bed seen is a blue clay of which a thickness of fifteen feet is uncovered. In the middle is a band three feet in thickness containing shells.

II. A well defined seam of lignite, six inches in thickness. For a few inches above and below this, the clay is filled with vegetable remains.

III. A bed of blue clay, thirteen feet in thickness, with an occasional shell too badly preserved to be removed.

IV. Blue clay, five feet thick, and full of fossils.

V. A bed, ten feet in thickness, of red and white clay, and sand, without fossils. This forms the surface deposit.

Not far from the ravine where the first section was made, Mr. Steere made another as follows:

I. 2 or 3 ft. of clay full of fossils.
II. 10 ft. blue clay.
III. 3 ft. blue clay filled with fossils.
IV. 5 ft. dirty coal.
V. 5 or 6 ft. of red and white clay.

In a ravine in the forest near the village, he made still another section, "finding in descending order" (I quote his own words) "five or six feet of red and white clay; a vein of dirty coal (two feet); blue clay without fossils, ten feet; another narrow vein of coal; eight or ten feet of blue clay without fossils; more coal; beds of clay without fossils; more coal veins."†

Mr. Steere visited Pichana, where he found much the same structure as at Pebas. At Old Pebas the same beds are seen containing beds of lignite, but they appear to be more denuded than at New Pebas.

At Iquitos Mr. Steere found similar beds that appeared to be the continuation of those of Pebas, but afforded no fossils.

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* Land not laid under water during the annual flood.

† Lieut. Herndon visited Pebas in 1851. He speaks of the ravines back of the town in which a black slate rock crops out, and says that he brought from the old town to the new "specimens of black clay slate, that crops out in narrow veins on the bank, and made a fire with it, which burned all night, with a strong bituminous smell."—Exploration of Valley of the Amazonas, Pt. I, pp. 219–220.

Mr. Steere has made very extensive collections of the fossils of the Pebas locality and vicinity, and they will probably afford some new species. When these collections with their accompanying lithological specimens shall have been studied, we shall have more details relative to the character of the beds and probably some facts bearing on the vertical distribution of the species. Mr. Steere simply says that the bivalves are more numerous in the lower and the univalves in the upper beds.

In examining the above sections, we find the surface bed always composed of variegated clays, with more or less sand, which, according to Mr. Steere were deposited on the much denuded surface of the lower fossiliferous beds, the last being clearly Tertiary. We have then at Pebas two well marked formations to deal with. It seems to me a little doubtful whether Prof. Agassiz could have seen the blue clays in the neighborhood of Tabatinga, for he makes no mention of the lignites which occur in them, and it also seems to me doubtful whether the greyish laminated clays with leaves, at Tonantins, which correspond with the iron stone with similar leaves, found by me on the Tapajos, can belong to the same formation. It is therefore by no means clear that Prof. Agassiz included any part of the Pebas series in his theoretical section of the Amazonian valley. I see no reason for referring the Pebas Tertiary to Bed II. of Prof. Agassiz's section. In sooth, that section is simply theoretical and cannot stand. There is no resemblance whatever between the blue Tertiary clays of Pebas and the mottled or variegated clays of the vicinity of Pará. The surface clays of Pebas, however, appear to resemble them; but resemblance in lithological structure, color, etc., is not identity, for one may find "variegated clays" on the Amazonas containing Devonian or Carboniferous fossils.

The Pebas fossils do not therefore occur in the superficial variegated clays, but in an older and distinct underlying formation quite unlike the ordinary more recent variegated clays of the Amazonas. The fossils, therefore, aid us only in determining the age of the lower series, leaving the question of the age of the superficial clays undecided. They certainly afford no proof that these latter clays are not of glacial origin. As to the condition under which these superficial clays were deposited, we simply have so far no evidence whatever, although the probabilities are that they are of quite modern origin; but whether fluviatile or estuarine, is a question which cannot be settled till we know more about their distribution. I dare not compare them with the superficial clays of the Lower Amazonas, for my experience with these deposits has satisfied me, that, how similar soever these beds may be in different localities, they may vary in age and greatly in the conditions under which they were deposited.

The Pebas shells do not shed one ray of light on the great question of the glaciation of the Amazonian valley. I have, however, shown that the supposed facts on which Prof. Agassiz founded his theory, viz: the assumed identity of structure of the Serras of Ereré and Parú (Almeyrim); the occurrence of erratics of diorite at Ereré, etc., were no facts at all. Ereré is a monoclinal ridge of sandstone which no geologist would ever think of calling drift, and the supposed drift clays at its base contain lower Devonian trilobites and are traversed by trap dykes; the supposed erratics of diorite are *boulders of decomposition;* the Serras of Parú* are composed of horizontal beds of soft rocks undoubtedly more modern than the Serra of Ereré and offering not the first evidence of glacial origin; the gigantic moraine which Prof. Agassiz thought to have extended across the mouth of the Amazonas does not exist. Moreover I have failed in finding, during many months of careful search, anything like drift in the province of Pará; and therefore, having no evidence whatever of the former existence of glaciers in the Amazonas, the question of the glacial origin of the valley need not be raised.

While I do not believe in the glaciation of the Amazonas, I still adhere to the belief that glaciers have existed in the central and southern portions of the Brazilian plateau. Prof. O. H. St. John, who, as one of the geologists of the Thayer expedition, made a journey through the interior of Brazil from Rio de Janeiro to Maranhâo, assures me that he has found not only the superficial deposits, but also the topography characteristic of a glaciated country in Minas Geraes, while these phenomena are not visible in Piauhy and Maranhâo.

Though the Pebas shells throw no light on the question of the glaciation of the Amazonian valley, they aid in establishing the fact that the Upper Amazonas or Marañon, from Iquitos to Tabatinga, a distance of some 240–250 miles, flows through a Tertiary basin, the channel of the river being deeply cut through beds of this age. The width of this basin is undetermined, as is also the exact age of the beds, for the nature of the fauna is such that it is impossible to say to which division of the Tertiary they are to be referred. The fauna indicates an estuary formation. That at the time of the deposition of Pebas beds there was water communication between the basins of the Amazonas and the Orinoco is scarcely probable.

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* I visited the Serra of Parauáquára in 1871.

# SCIENTIFIC INTELLIGENCE.

## I. CHEMISTRY AND PHYSICS.

1. *On the light emitted by the vapor of iodine.*—SALET has found that the vapor of iodine may be heated to redness like a solid or liquid. It then emits the less refrangible luminous rays which furnish a continuous spectrum. The experiment is easily made by heating the iodine in a tube of Bohemian glass. A small crystal of iodine is placed in a tube of thick glass, which is then heated strongly at some distance from the crystal. When the glass is red for a considerable part of its length, it is to be allowed to cool until it is no longer visible in the dark; the iodine is then to be rapidly volatilized. The colored vapor reaching the heated part of the tube then glows with a distinct red light. This experiment shows that the iodine becomes luminous at a lower temperature than glass. Another method of exhibiting the incandescence of the vapor of iodine is the following: A spiral of fine platinum wire is sealed in the interior of a tube of glass eight millimeters in diameter. Pure iodine is then introduced into the tube, which after expulsion of the air is sealed. If the iodine be then volatilized and the wire ignited by a battery, the spiral appears surrounded by a flame of a very rich red color, which yields the well-known interrupted spectrum.—*Comptes Rendus*, Tome lxxiv, p. 1249. W. G.

2. *On the absorption spectra of the vapors of selenium and of certain other bodies.*—GERNEZ has found that the vapors of selenium and a number of other colored vapors give distinctly marked absorption bands. When selenium is heated in a porcelain tube closed at its extremities by plates of glass, we observe a progressive absorption of all the rays of the spectrum, beginning with the most refrangible and proceeding toward the red, but without any traces of dark bands. But if we continue to raise the temperature, the tint of the vapor becomes more clear and the different regions of the spectrum reappear, crossed in the blue and the violet by bundles of dark bands. The result cannot be ascribed to the formation of selenious acid, since the same phenomena are observed when the selenium is heated in an atmosphere of dry carbonic acid. Selenious chloride, which is a brown limpid liquid, gives a vapor the spectrum of which is crossed by rays beginning at the limit of the green and the blue, and extending to the extremity of the violet. Bromide of selenium produces systems of almost equidistant rays, but in a different part of the spectrum not specified by the author. Tellurium when volatilized in an atmosphere of dry carbonic acid gas emits at a very high temperature a golden yellow vapor, which yields a very brilliant absorption spectrum much more extended toward the red than the spectra of sulphur and selenium, and composed of systems of fine rays extending from the yellow into the violet. Tellurous chloride gives yellow vapors, which act very strongly on light. The absorption spectrum is particularly developed in the orange and the green. Tellurous

bromide gives a violet vapor, the most remarkable absorption bands of which are in the red and the yellow. Proto-bromide of iodine gives off at ordinary temperatures vapors which in thin layers have a copper-red color. The absorption spectrum of this substance presents very fine rays in the red, yellow and orange; and differs from the spectrum obtained by passing light through successive layers of iodine and bromine vapors. Alizarin when carefully heated gives vapors which exhibit systems of sensibly equidistant rays in about the middle of the spectrum.—*Comptes Rendus*, Tome lxxiv, p. 1190. W. G.

3. *On the absorption spectra of the vapors of sulphur, selenious acid and hypochlorous acid.*—The same writer has observed the existence of dark lines in the spectra of many other colored vapors. The source of light employed was the Drummond light so-called. Sulphur heated in a porcelain tube closed at the extremities with plates of glass gave at first vapors which absorbed the most refrangible rays of the spectrum, leaving finally a red band extending a little beyond C. On raising the temperature higher this band spreads out; the other rays of the spectrum then reappear, the violet and blue being crossed by bundles of dark rays. The phenomenon is therefore the same as that observed in the case of selenium. Selenious acid at the instant of vaporization gives well-marked dark lines, especially in the violet and blue. The author found the absorption spectrum of hypochlorous acid identical with that of hypochloric and chlorous acids, only in the case of hypochlorous acid the layer of gas must be much longer in order that the phenomenon may be distinctly visible. Aqueous solutions of all these gases give the most salient lines of the gases themselves. —*Comptes Rendus*, Tome lxxiv, p. 803. W. G.

4. *On fluoride of silver.*—In continuing his researches on the compounds of fluorine, Mr. G. GORE has arrived at the following results: Iodine acting upon argentic fluoride with the aid of heat produces argentic iodide and fluoride of iodine. Fluoride of iodine is a volatile, colorless liquid, which does not corrode mercury or red-hot platinum, but which corrodes glass at 60° F., and crystals of silicon at a red heat, as well as platinum in contact with argentic fluoride in a state of fusion. It fumes strongly in the air, and is decomposed by water into fluohydric and iodic acids,

$$IF_5+3H_2\Theta=5HF+HI\Theta_3.$$

By heating argentic fluoride to redness in a current of dried, cool gas, it was wholly reduced to metallic silver. Fluohydric acid and carbonic tetro-fluoride being evolved, vitrified boric acid violently decomposed the fluoride when fused, emitting copious white acid fumes, but did not act on an aqueous solution of the salt at 60° F. Crystals of silicon placed upon argentic fluoride when fused became at once red hot, undergoing rapid combustion, and evolving fluoride of silicon. A lump of fused silicon also slowly decomposed an aqueous solution of argentic fluoride, setting free metallic silver in crystals. Crystals of silicon behaved in the same manner, but much more rapidly, and evolved abundance of gas if the solution contained free fluohydric acid; on adding nitric acid to this mixture, bubbles of spontaneously inflammable silicide of hydrogen were evolved and ignited. Pure and dry silicon added to argentic fluoride at a temperature of low redness evolved much heat with violent action, and set free metallic silver. The fluoride when fused is rapidly decomposed by sulphur with evolution of heat, fluoride of sulphur and argentic sulphide being formed. Fluoride of sulphur is a heavy, colorless vapor, not condensing at $0^\circ$ C. and $760^{\mathrm{mm}}$. It corrodes glass, fumes strongly in the air, and has a very powerful dusty odor. Sulphur rapidly decomposed an aqueous solution of argentic fluoride. When the vapor of carbonic disulphide is passed over argentic fluoride at a red heat, argentic sulphide and carbonic tetra-fluoride are formed, the reaction being represented by the equation:

$$4AgF + CS_2 = 2Ag_2S + CF_4.$$

The tetra-fluoride is a fuming acid vapor.—*L. and E. Phil. Mag.*, *May*, 1872. W. G.

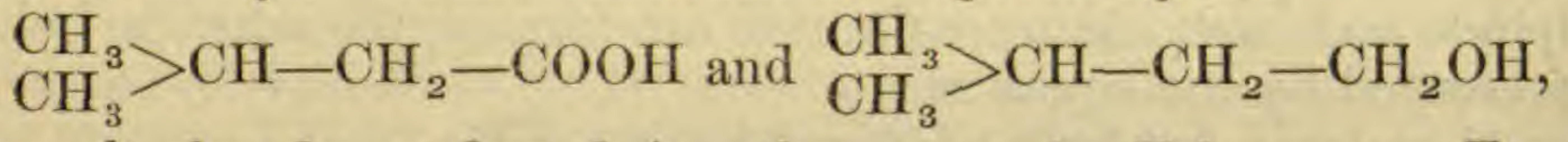
5. *On a method of fixing the Constitution of Acids and Alcohols by the oxidation of their Ketones.*—In his researches upon the oxidation of ketones, POPOFF observed a uniformity of results which led him to suggest this oxidation as a means of determining the rational constitution of acids and alcohols. Taking the four varieties of ketone expressed by the following general formulas:—

$$\begin{array}{llcl}
(1) & CH_3 - (CH_2)_n & - CO - & R \\
(2) = & CH - (CH_2)_n & - CO - & R \\
(3) & = CH & - CO - & R \\
(4) & \equiv C & - CO - & R
\end{array}$$

—in which R represents one of the alcohol-radicals united directly to the carbonyl, generally methyl, phenyl, or ethyl—Popoff finds that by oxidation, the carbonyl remains combined with the radical R, while the other alcohol radical is oxidized. If this other radical be that of a normal alcohol—as is the case in the first of the above formulas—then a normal acid results; if it be an iso-alcohol radical,—as in the second formula—an iso-acid results; a secondary-alcohol radical,—as in the third formula—gives an acetone; while a tertiary-alcohol radical—as in the last formula—splits up by oxidation. Now, since the ketones can be prepared from the fatty acids,—either by distillation of a suitable mixture of their salts or treating the acid chloride with the zinc-compound of the desired alcohol-radical—and since these ketones when submitted to oxidation, yield their carbonyl combined with the alcohol-radical R, while the other alcohol-radical—which is furnished by the acid—is oxidized after the manner just given, it is clear that this method may be used to determine the constitution of the alcohol-radical which is contained in any acid. Moreover, since the fatty acids may be obtained by oxidizing the corresponding alcohols, the constitution of these latter may also be determined by this method.

To test the method, amyl alcohol—of boiling point 130° to 131·5°, and whose power of rotation in a tube 25 cm. long was −2·4°,—

was oxidized, and the valeric acid produced—which boiled from 174° to 176°, and in which $\alpha = +4{\cdot}4°$,—was converted into the calcium salt, and distilled with an equivalent quantity of calcium benzoate. On rectifying the product, it boiled at 225° to 226°, and afforded on analysis numbers agreeing with those required by butylphenyl ketone. To fix the constitution of the butyl it contained, the ketone was oxidized. The products consisted of benzoic acid and iso-butyric acid, with traces of acetic acid; thus proving that this ketone, and also, therefore, the valeric acid and the amyl alcohol from which it was derived, contain iso-butyl and not butyl. Their constitutions respectively are—



$$\begin{matrix}CH_3\\CH_3\end{matrix}\!\!>CH{-}CH_2{-}COOH \text{ and } \begin{matrix}CH_3\\CH_3\end{matrix}\!\!>CH{-}CH_2{-}CH_2OH,$$

a result already confirmed in other ways by Erlenmeyer, Frankland and Duppa, and Butlerow.—*Ber. Berl. chem. Ges.*, v, 38, Feb., 1872. G. F. B.

6. *On Phenol-colors and their Relation to Natural Coloring Matters.*—Baeyer has continued his researches on gallein and fluorescein,* and other coloring matters derived from the phenols. He finds that the phthalic acid does not act solely by abstracting water, as he at first supposed; but that it enters itself into the composition of the new molecule. He finds, moreover, that generally, whenever the phenols are heated with polybasic organic acids, either alone or in presence of glycerin or sulphuric acid, water is abstracted, and a series of compounds, not ethers, is formed. Some of these are indifferent bodies, while others are soluble in potassium hydrate with an intense color, and on reduction become colorless. These latter, many of them, give, when heated with sulphuric acid, new colored compounds, which differ from them in the fact that, when reduced, the products are also colored. Baeyer proposes to terminate the name of the former class of bodies, which are soluble in potassium hydrate with color, with the syllable *-ein*, while to the colorless bodies obtained from these by reduction he gives names terminating in *-in*. The indifferent bodies he calls anhydrides of the former. Of the almost unlimited number of bodies thus possible, the author describes:—

(1.) Phenol colors. When 10 parts of phenol, 5 parts phthalic oxide, and 4 parts concentrated sulphuric acid are heated to 120° to 130° for several hours, a red mass is obtained, yielding a yellowish-white powder on treatment with boiling benzol. When dissolved in potassium hydrate, and precipitated with hydrochloric acid, a granular precipitate is obtained, having the composition $C_{20}H_{14}O_4$. It is the phthalein of phenol has probably the rational constitution $C_6H_4(CO\,.\,C_6H_4OH)_2$, and is isomeric with the phthalic ether of phenol. When heated, in solution in potassium hydrate, with zinc-dust, the magnificently fuchsine-colored solution is decolorized, and hydrochloric acid precipitates therefrom the white granular phthalin of phenol, $C_{20}H_{16}O_4$. Mellitic and pyromellitic acids act similarly upon phenol; but the most inter-

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* This Journal, III, ii, 203.

esting action is that of oxalic acid, which has long been known, and the product of which is rosolic acid. The aurin, lately isolated from rosolic acid by Dale and Schorlemmer, Baeyer supposes to be $C_{25}H_{20}O_8$ and to result from the oxidation of leucoaurin $C_{25}H_{20}O_4$, which is thus produced:

$$CO_2+(C_6H_6O)_4=C_{25}H_{20}O_4+(H_2O)_2.$$

(2.) $\alpha$ Naphthol colors. $\alpha$ Naphthol, heated with phthalic oxide, yields light yellow crystals of the anhydride of the phthalein of naphthol, $C_{28}H_{16}O_3$, insoluble in potassium hydrate. Heated with sulphuric acid, it gives a beautiful red body, $C_{28}H_{18}O_8$. Oxalic, mellitic and pyromellitic acids act similarly on $\alpha$ naphthol.

(3.) Resorcin colors. Resorcin heated with phthalic oxide, gives the phthalein of resorcin, or fluorescein, which, precipitated from its potash solution by hydrochloric acid, is $C_{20}H_{14}O_6$, but recrystallized from alcohol is $C_{20}H_{12}O_5$. Reduced by zinc-dust, the corresponding phthalin is obtained. Heated with sulphuric acid, a red body is formed, which is turned blue by alkalies, and which yields a second red body on reduction. It closely resembles the coloring matter of litmus. Succinic oxide gives with resorcin, the succinein of resorcin; and oxalic acid, the carbonein, or euxanthon, $C_{13}H_8O_4$.

(4.) Pyrogallol colors. Pyrogallol (pyrogallic acid) by the action of phthalic oxide, gives gallein, $C_{20}H_{12}O_7$, the phthalein of pyrogallol. Reduced, it gives gallin $C_{20}H_{18}O_7$. Heated with sulphuric acid, it forms cœrulein, $C_{20}H_{10}O_7$, and this on reduction gives cœrulin. Oxalic acid and succinic oxide, as well as oil of bitter-almonds, acetone, etc., also afford colored compounds when heated with pyrogallol.

Hydroquinone gives with phthalic oxide a red phthalein, soluble in potash with a violet color, and dyeing, like brazil-wood, with iron and alumina mordants. Pyrocatechin, thus treated, gives a phthalein, soluble in potash with a transient blue color, analogous apparently to the coloring matter of logwood. Phloroglucin gives with phthalic oxide, a yellow phthalein. These new coloring matters contain (*a*) the phenol residue, and (*b*) the connecting or linking residue, which latter is supplied by the acid. Since the acid employed has but slight influence on the color, the former must be the color-forming constituent. Baeyer is of the opinion that many natural colors, especially those of the dye-woods, have this constitution; and hence that their synthesis may be looked for, when the nature of the linking body can be made out. This is a somewhat difficult matter, as in these colors the linking compound belongs to the sugar group or to the family of vegetable acids. Thus hæmatein—the coloring matter of logwood—which yields pyrogallol on fusion with potash, is a derivative either of that, of hydroquinone or of pyrocatechin. If its formula be $C_{16}H_{12}O_6$, the 4-carbon link present suggests that its synthesis may be effected by heating the above bodies with malic or succinic acids or with a derivative of crotonic acid.—*Ber. Berl. chem. Ges.*, iv, 658, July, 1871. G. F. B.

7. *A New Erecting Prism;* by **Joseph Zentmayer**.—Mr. Joseph Zentmayer exhibited and described a single prism, which erects the image completely, and in such a way that the incident and emerging rays are parallel, which, as far as we know, was never accomplished before. In connection with the microscope, as it was shown, it interfered very little with the definition, and, although the light is twice refracted and reflected, the loss of light is much less than one would expect. With the microscope, the prism is placed right above the objective, and the instrument may be used in any inclined position. A pair of such prisms might be used also for an erecting binocular microscope, of which the two bodies have the same inclination to the stage.

Fig. 1 shows the front and profile of the prism. The projection of the front is a square, that of the profile an isosceles triangle.

FIG.1.

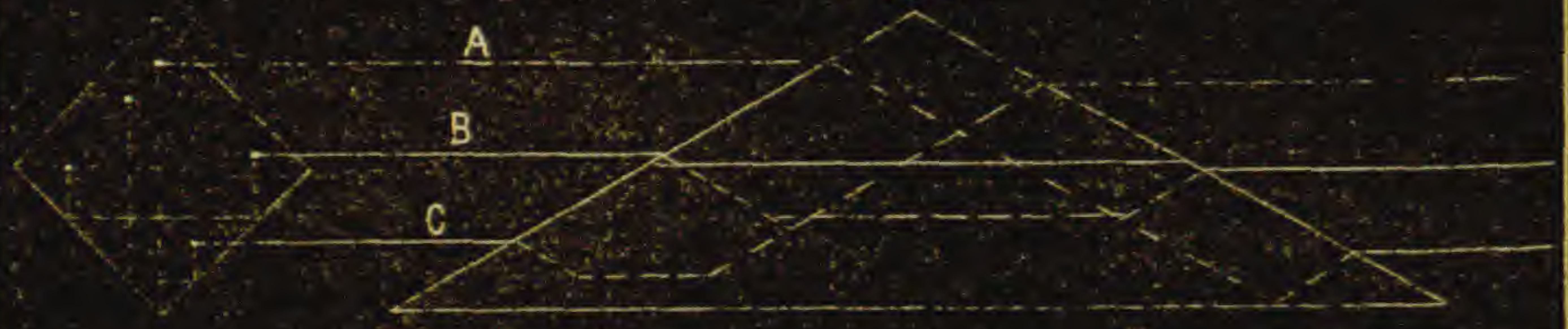
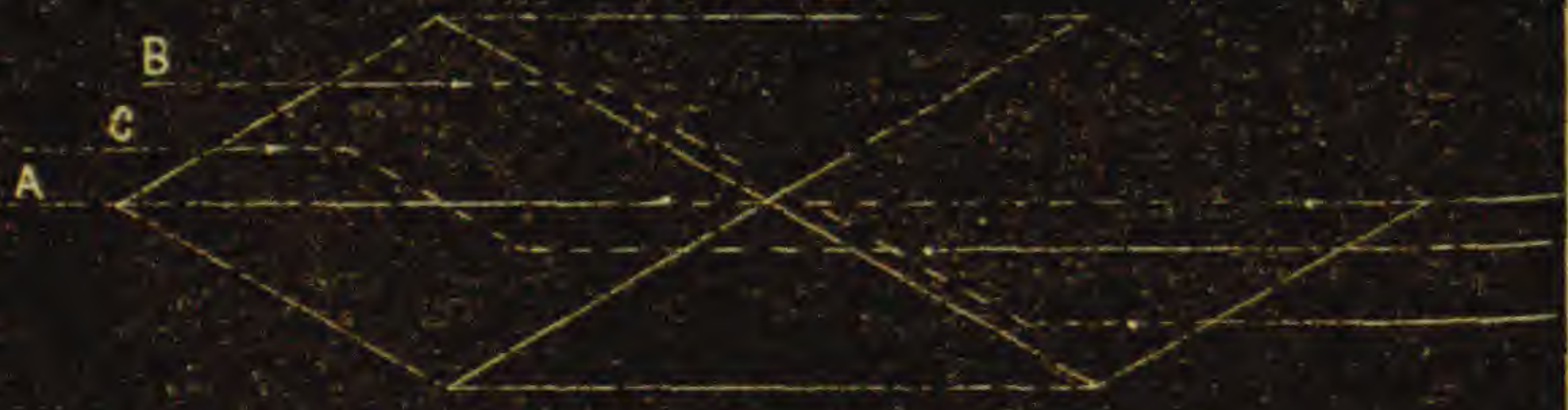


FIG. 2.



The angles at the base of the triangle are 27° 19′ for crown glass of a refracting index of 1·53, in order to obtain the greatest aperture combined with the smallest prism.

Fig. 2 is a view from above. The rays of A, B, and C of figs. 1 and 2 are the identical ones, their dotted parts are the projections of the rays inside of the glass, and their course may be read-

FIG.3.

ily followed in the profile, fig. 1, where the upper ray, A, emerges as the lower one, and the lower ray, C, as the upper one.

As the ray A enters the perpendicular line above the lower edge, it will not be reflected out of its plane, while the rays B and C, entering the left side of the prism, reach the inclined faces, from which they are reflected to the opposite lower one, and are changed in their course to the right, from here again reflected, to emerge at the corresponding opposite point.

Fig. 3 is a perspective representation of the prism.—*Journal Franklin Institute.*

## II. Geology and Natural History.

1. *On the Eozoon;* by Dr. Dawson.—Dr. Dawson published a reply to the first of the extended memoirs of Messrs. King and Rowney in volume 1 of the second series of the Proceedings of the Irish Academy. That our readers may have before them what is said by the original describer of the Eozoon, in opposition to the conclusions of the Irish investigators, we cite the following from his article.

In opposition to these facts, and to the careful deductions drawn from them, the authors of the paper under consideration maintain that the structures are mineral and crystalline. I believe that in the present state of science such an attempt to return to the doctrine of "plastic-force" as a mode of accounting for fossils would not be tolerated for a moment, were it not for the great antiquity and highly crystalline condition of the rocks in which the structures are found, which naturally create a prejudice against the idea of their being fossiliferous. That the authors themselves feel this is apparent from the slight manner in which they state the leading facts above given, and from their evident anxiety to restrict the question to the mode of occurrence of serpentine in limestone, and to ignore the specimens of Eozoon preserved under different mineral conditions.

With reference to the general form of Eozoon and its structure on the large scale, I would call attention to two admissions of the authors of the paper, which appear to me to be fatal to their case: First, they admit, at page 533 [Proceedings, vol. x], their "inability to explain satisfactorily" the alternating layers of carbonate of lime and other minerals in the typical specimens of Canadian Eozoon. They make a feeble attempt to establish an analogy between this and certain concentric concretionary layers; but the cases are clearly not parallel, and the laminæ of the Canadian Eozoon present connecting plates and columns not explicable on any concretionary hypothesis. If, however, they are unable to explain the lamellar structure alone, as it appeared to Logan in 1859, is it not rash to attempt to explain it away now, when certain minute internal structures, corresponding to what might have been expected on the hypothesis of its organic origin, are added to it? If I affirm that a certain mass is the trunk of a fossil tree, and another asserts that it is a concretion, but professes to be unable to account for its form and its rings of growth, surely his case becomes very weak after I have made a slice of it, and have shown that it retains the structure of wood.

Next, they appear to admit that if specimens occur wholly composed of carbonate of lime their theory will fall to the ground. Now such specimens do exist. They treat the Tudor specimen with scepticism as probably "strings of segregated calcite." Since the account of that specimen was published, additional fragments have been collected, so that new slices have been prepared. I have examined these with care, and am prepared to affirm that the chambers in these specimens are filled with a dark-colored limestone not more crystalline than is usual in the Silurian rocks, and that the chamber-walls are composed of carbonate of lime, with the canals filled with the same material, except where the limestone filling the chambers has penetrated into parts of the larger ones. I should add that the stratigraphical researches of Mr. Vennor, of the Canadian Survey, have rendered it probable that the beds containing these fossils, though unconformably underlying the Lower Silurian, overlie the Lower Laurentian of the locality, and are, therefore, probably Upper Laurentian, or perhaps Huronian, so that the Tudor specimens may approach in age to Gümbel's Eozoon Bavaicum.*

Further, the authors of the paper have no right to object to our regarding the laminated specimens as "typical" Eozoon. If the question were as to *typical ophite*, the case would be different; but the question actually is as to certain well-defined forms which we regard as fossils, and allege to have organic structure on the small scale, as well as lamination on the large scale. We profess to account for the acervuline forms by the irregular growth at the surface of the organisms, and by the breaking of them into fragments confusedly intermingled in great thickness of limestone, just as fragments of corals occur in Paleozoic limestone; but we are under no obligation to accept irregular or disintegrated specimens as typical; and, when objectors reason from these fragments, we have a right to point to the more perfect examples. It would be easy to explain the loose cells of *Tetradium*, which characterize the Birds-eye limestone of the Lower Silurian of America, as crystalline structures; but a comparison with the unbroken masses of the same coral shows their true nature. I have for some time made the minute structure of Paleozoic limestones a special study, and have described some of them in the Trenton formation of Canada. I propose, shortly, to publish additional examples, showing fragments of various kinds of fossils preserved in these limestones, and recognizable only by the infiltration of their pores and other minute structures. I shall also be able to show that in many cases the crystallization of the carbonate of lime and the infiltration of

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* Dr. Hunt, in a recent communication to this Journal for July, 1870, p. 85, is supposed to regard them as belonging to a great series of strata not hitherto clearly recognised, lying at the base of the Primordial, but distinct from and newer than the Upper Laurentian and the Huronian.

other substances have not interfered with the perfection of the most minute of these structures.

The fact that the chambers are usually filled with silicates is strangely regarded by the authors as an argument against the organic nature of Eozoon. One would think that the extreme frequency of siliceous fillings of the cavities of fossils, and even of siliceous replacement of their tissues, should have prevented the use of such an argument, without taking into account the opposite conclusions to be drawn from the various kinds of silicates found in the specimens, and from the modern filling of Foraminifera by hydrous silicates, as shown by Ehrenberg, Mantell, Carpenter, Bailey, and Pourtales.* Further, I have elsewhere shown that the loganite is proved by its texture to have been a fragmental substance, or at least filled with loose *débris*; that the Tudor specimens have the cavities filled with a sedimentary limestone, and that several fragmental specimens from Madoc are actually wholly calcareous. It is to be observed, however, that the wholly calcareous specimens present great difficulties to an observer; and I have no doubt that they are usually overlooked by collectors in consequence of their not being developed by weathering, or showing any obvious structure in fresh fractures.

With regard to the canal system, the authors persist in confusing the casts of it which occur in serpentine with "metaxite" concretions, and in likening them to dendritic crystallizations of silver, &c., and coralloidal forms of carbonate of lime. In answer to this, I think it quite sufficient to say that I fail to perceive the resemblance as other than very imperfect, imitative. I may add that the case is one of the occurrence of a canal structure in forms which on other grounds appear to be organic, while the concretionary forms referred to are produced under diverse conditions, none of them similar to those of which evidence appears in the specimens of Eozoon. With the singular theory of pseudomorphism, by means of which the authors now supplement their previous objections, I leave Dr. Hunt to deal.

With regard to the proper wall and its minute tubulation, the essential error of the authors consists in confounding it with fibrous and acicular crystals, and in maintaining that because the tubuli are sometimes apparently confused and confluent they must be inorganic. With regard to the first of these positions. I may repeat what I have stated in former papers—that the true cell-wall presents minute cylindrical processes traversing carbonate of lime, and usually nearly parallel to each other, and often slightly bulbose at the extremity. Fibrous serpentine, on the other hand, appears as angular crystals, closely packed together, while the numerous spicular crystals of siliceous minerals which often appear in metamorphic limestones, and may be developed by decalcification, appear as sharp angular needles usually radiating from centers or irregular disposed. Plate 44, fig. 10 (Ophite from Skye, King and Rowney's Paper, "Proc. R. I. A.," vol. x), is an eminent example of this; and whatever the nature of the crystals may be,

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* "Quarterly Journal Geol. Society," 1864.

they have no appearance in the plate of being tubuli of Eozoon. I have very often shown microscopists and geologists the cell-wall along with veins of chrysotile and coatings of acicular crystals occuring in the same or similar limestones, and they have never failed at once to recognize the difference, especially under high powers.

I do not deny that the tubulation is often imperfectly preserved, and that in such cases the casts of the tubuli may appear to be glued together by concretions of mineral matter, or to be broken or imperfect. But this occurs in all fossils, and is familiar to any microscopist examining them. How difficult is it in many cases to detect the minute structure of Nummulites and other fossil Foraminifera? How often does a specimen of fossil wood present in one part distorted and confused fibers or mere crystals, with the remains of the wood forming phragmata between them, when in other parts it may show the most minute structures in perfect preservation? But who would use the disintegrated portions to invalidate the evidence of the parts better preserved? Yet this is precisely the argument of Professors King and Rowney, and which they have not hesitated in using in the case of a fossil so old as Eozoon, and so often compressed, crushed, and partly destroyed by mineralization.

I have in the above remarks confined myself to what I regard as absolutely essential by way of explanation and defence of the organic nature of Eozoon. It would be unprofitable to enter into the multitude of subordinate points raised by the authors, and their theory of mineral pseudomorphism as discussed by my friend Dr. Hunt; but I must say here that this theory ought, in my opinion, to afford to any chemist a strong presumption against the validity of their objections, especially since it confessedly does not account for all the facts, while requiring a most complicated series of unproved and improbable suppositions.

The last point which I shall mention is the taunt that so little further progress has been made in the investigation of Eozoon. With reference to this, I beg leave to doubt whether a process of confounding the actual structure of Eozoon with all manner of dendritic and crystalline forms, in the way followed by the authors, would constitute progress. But in so far as careful comparison with all specimens which have been recently found is concerned, some progress has been made; and I trust that it will soon be possible to bring forward not merely additional specimens illustrative of the structure of Eozoon, but fresh evidence of its wide geographical range, and also links of connexion with fossils of the Paleozoic rocks. The discovery recently made in Massachusetts, and alluded to by Messrs. Rowney and King, is itself not without importance. In the meantime I am content to accept the investigations of Messrs. King and Rowney as nearly exhaustive of the natural history of those imitative forms which may be confounded with Eozoon, and therefore as in a certain way useful in the further prosecution of the subject. As already stated, I am at this moment engaged in following out, as opportunity offers, two lines of investigation bearing on the following points:—(1) the study of the Lower Silurian and Primordial successors of Eozoon, and (2) that of the tubulation and other structures similar to those of Eozoon preserved in the Paleozoic rocks.

2. *Discovery of a Large Bone Cave in Bavaria.*—During the cutting of the railway from Nuremberg to Regensburg by the Bavarian Eastern Railway Co., it was necessary to cut directly through a piece of mountain chain in Schelmengraben near Regensburg. It was owing to this that this bone cave was discovered, and its miscellaneous contents were able to be examined and arranged. Since the railway cut right across the middle of the cave, it allowed it to be very thoroughly examined, and under the most favourable circumstances and in daylight, as has been the case in very few other instances. The railway company have given every facility in their power that the cave should be thoroughly examined, and under the direction of Profs. Fraas and Zittel, a gang of men were actively employed for many days, and the objects so obtained were carefully preserved. From the local German papers the following particulars have been obtained, which, allowing for a little local coloring and exaggeration, show the find to have been a most important one, and one that may well come under the notice of the International Congress of Archæology and Anthropology at their meeting this year, where the whole question of bone caves and their contents is to form a prominent subject for discussion.

The cave in question was originally, when first discovered about two years ago, 28 metres (about 91 ft.) long, and was simply a fissure in the Jura limestone, which had been enlarged by running water. Its opening was visible half way up the mountain side, partly hidden in dense woods. It stretched from north to south, with a slight turn toward the west of about 15°. The new line of railway cut deeply into the hill side, and during the course of this year has already cut away one half of the cave; but unfortunately the contents were employed on the line. On this account, only the part not touched was able to be excavated and examined, and this was 11 metres (36 ft.) long, 2 metres (6½ ft.) wide, and in the middle 3 metres (9½ ft.) deep. Wood ashes and pieces of coal, together with pieces of pottery, had accumulated to about the height of three feet, in the midst of which were sharp splinters of flint, and a thick mass of broken and split bones, and the shattered skulls and jaw bones of a heterogeneous mass of animals of all kinds. In the lowest layer no trace of men, either by their remains or by their handiwork, could be found; all the remains consisted of bones of animals, chiefly the cave bear, hyena, and lion. These cave-dwelling animals appear to have been the first and earliest possessors of the cave. But soon after this, men must have discovered the cave and inhabited it, for, from this layer up to the newest layer of all, the presence of man is clearly shown, and the remains of their feasts and of their daily life are mingled with those of the previously-named animals. The most numerous remains consist of flints, of which many thousand were found; but these do not appear to have been used as implements, but come rather under the category of flint-flakes, the chippings from knives, saws, lances, &c. The most perfect one found is three inches long and half-an-inch wide, and is toothed like a saw, and was probably used as such to saw off the ends of the deer's horns, of which quantities were found.

In order to judge of the age in which men began to inhabit this cave, we must examine the remains of the bones and skeletons of the animals which they hunted, and whose flesh was eaten in the cave. The most conspicuous among these is the cave bear, and although it might at first sight appear very difficult to recognize in the broken and burnt bits of bone that they really do belong to the cave bear, nevertheless, careful comparison with specimens in museums has proved that this is the case. Every care seems to have been made to utilize to the utmost all parts of this animal, which was apparently the most important game in the surrounding forests, and which no doubt required much labor and time to capture. At the same time, together with the bones of the cave bear are found bones of the elephant and of the rhinoceros, but not many in comparison. These remains, however, show conclusively, by the way in which they have been spilt up and broken, that man hunted these animals at the time he first appears on the scene. Remains of horses, oxen, cats, and wolves were also met with, and in proof that the early inhabitants were not unmindful of fish, there are the bones and scales of large pike and carp. The smaller bones of mice and frogs do not appear to owe their origin so much to man as to the owls, which seem to have held possession of the cave as well.

Great interest attaches to the fragments of pottery which were found in the cave, and which rival the flint flakes in quantity. It appears to have been all hand made, but although rough, shows considerable beauty of shape and form. It is possible to put together from the fragments one or two more or less complete vessels, which, however, show great diversity as to size, &c., some being between 10 and 20 centimetres in diameter. The material of which they are made appears to be clay mixed with sand, but few, if any, seem to have been regularly burnt. Much of the pottery is ornamented with lines or rows of dots, which run in zigzag lines over the wider parts. The internal smoothness would appear to be due to the river mussel, Unio, obtained from the river Naab, which flows close by, and of which many well rubbed and polished specimens were found in the cave. A block of granite with one side rubbed smooth by long usage, and appearing quite polished, can hardly be anything else than a well-worn millstone, and this is rendered more probable by two holes having been bored into the upper side as if for the purpose of affixing a handle. The presence of this millstone would indicate the cultivation of land in the immediate neighborhood, which is confirmed by the finding of several spindles made of clay.

The different objects found in this cave are of great interest, as they apparently run counter to the somewhat hard and fast lines which have been drawn as to different well marked periods in the early history of man.—*Nature*, May 30.

3. *Pseudomorphs of Serpentine with the form of Staurolite;* T. D. RAND (Proc. Acad. Nat. Sci. Philad., 1871).—At the line between Philadelphia and Montgomery counties, the well-known steatite bed, beginning on the west side of Chestnut Hill, about three miles distant, crosses the Schuykill and continues in a nearly southwest by south direction (exactly S. 54 W.), beyond that river about two miles and a half, where it crosses the valley of Mill Creek, and ends, or sinks beneath the surface. Perhaps the most conspicuous and remarkable rock of this belt is a steatite, containing a black serpentine. This rock in many places projects above the surface of the ground in immense masses, particularly at Mill Creek, seeming to have resisted erosion and decomposition to a remarkable degree. It lies on the northwest side of the steatite proper. The whole aspect of this curious formation suggests a rock originally containing crystals of some mineral, but metamorphosed almost beyond recognition. To this mineral I believe no clue has heretofore been obtained. Nearly all these black masses, which vary in size from a half-inch or less to several inches, are irregular in form, and adhere so closely to the matrix that sections only can be obtained, which, however, rarely show any angles, or other than a nodular form, so that the rock has by some been considered a conglomerate. Even in weathering, the two, except in one place hereafter mentioned, seem to weather so much alike that no clue to the form can thus be had.

About two years ago, however, I found, near the soapstone quarry, on the northeast bank of the Schuykill, one of these serpentine masses presenting a stellated form of six rays, or of one large crystal crossed by two smaller at angles of about 60 and 120 on the section plane, suggesting staurolite. A few days ago, while with Prof. E. J. Houston examining this rock on the southwest side of Mill Creek, a piece was found containing a broken crystal 4½ inches × 1½ × 1½, presenting two well-defined sides, and upon a cross-fracture these sides were found to continue to another forming with one of the sides an angle of about 75° on the section, which approximated a plane at right angles to the faces. The steatite in which it was imbedded, and the serpentine itself, contained ferruginous dolomite or breunnerite.

On the northeast side of Mill Creek, a portion of the rock in place was found very much weathered on the surface, the steatite being cavernous and decomposed, and very soft and brittle, owing, probably, to a large admixture of ferruginous dolomite, but the serpentine gone entirely, save a little pulverulent oxide of iron; the cavities were nearly all lenticular in shape, but too regular to be other than matrices of crystals, while in two cases distinct cruciform cavities with angles of about 60° were observed. The portions of rock containing these were cut out, and in one of them lead was poured, and a cast obtained, which, while irregular and rough, was a fac-simile in metal of the common cruciform twins of staurolite. Portions of the same rock, which had not altered, were found containing the serpentine in distinct crystals, irregular in outline, but twinned at angles of about 60°.

4. *Hisingerite, from the Gap Mine, Lancaster County, Pa.;* T. D. RAND.—Black amorphous; lustre between resinous and vitreous; streak, brown. Fracture conchoidal, brittle. H.=2½–3. G.= 2.11.

Analysis, omitting 1.13 per cent. gangue:—

| | | |
|---|---|---|
| Water at 212° | 14.30 | |
| at redness | 9.89 | 24.19 |
| Silica | | 35.40 |
| FeO | | 12.53 |
| $Fe_2O_2$ | | 27.46 |
| | | 99.58 |

In a cutting through decomposed mica schists, on the new line of the Philadelphia, Wilmington and Baltimore Railroad, about half a mile southwest of Gray's Ferry, there is a white efflorescence, alkaline to the taste. It consists chiefly of sulphate of soda, an unlooked-for mineral in such location.—*Proc. Acad. Nat. Sci. Philad.*, 1871.

5. *Descriptions of new species of Fossils from the vicinity of Louisville, Ky., and the Falls of the Ohio;* by JAMES HALL and R. P. WHITFIELD. 7 pp. 8vo. Published May, 1872, in advance of the Report on the State Museum. Contains descriptions of species of Orthis, Spirifera, Pentamerus, Aviculopecten, Yoldia?, Nucula, Cypricardinia, Polyphemopsis, and of the new genus Ptychodesma, based on a modioloid shell.

6. *Mineralogical investigations of vom Rath.*—The 144th volume of Poggendorff's Annalen contains a continuation of the valuable mineralogical and crystallographic researches of vom Rath, treating of the composition of the lime-soda feldspars; composition of orthoclase; the ersbyite of Pargas; sahlite in the Pennine Alps; wollastonite of Mt. Somma; allophane of Dehrn in Nassau.

7. *Proceedings and Transactions of the Nova Scotian Institute of Natural Science of Halifax, Nova Scotia.*—Part I. of vol. iii, 94 pp. 8vo. (5*s.*), has recently been issued. It contains several papers on the geology, natural history, and meteorology of Nova Scotia.

8. *Marc Micheli; On some Recent Researches in Vegetable Physiology.* An article in the *Archives des Sciences* of the *Bibliothèque Universelle* of Geneva, in October last, reproduced in English in the Ann. and Mag. of Natural History, London, for February and March last.—Micheli is the translator into the French of Sachs' volume upon Vegetable Physiology, and we trust he will translate the other volumes of the series to which this belongs. The researches which are first considered in this inter-

esting article, relating to the movements of chlorophyl grains in the cells of leaves under the light, have been noticed in this Journal. The discovery that the green of leaves is less intense under direct sunshine than under diffuse daylight is attributed to Sachs. The discovery of the movement of the grains toward the light, that they group themselves during the day upon the more illuminated horizontal walls—and this only under the action of the more refrangible solar rays—and retreat at night to the perpendicular walls, belongs to Famintzin. Next Borodin (as we have already recorded), explained Sachs' observation by ascertaining that direct sunlight, when too ardent, caused the same retreating of the green grains that darkness does. Then Rose remarked that the movement of the green grains involved that of the whole protoplasmic mass; and Frank (in Bot. Zeit.) finds that the result of prolonged unilateral illumination is to accumulate the grains in the more strongly illuminated side of the cell,—that, like zoosposes, they incline to seek the light. In these, as in all periodical movements, currents of protoplasm, and in heliotropic curvatures, the most refrangible rays only are efficient. As to the action of light in assimilation, the decomposition of carbonic acid, and the formation of chlorophyl, the net results of all recent researches, those of Famintzin, Krauss, Prillenx, Boronetsky (Bot. Zeit., 1871, No. 13), and Pfeffer, go to confirm the now well-settled view that these phenomena are dependent solely upon luminous intensity. Dr. Pfeffer, by very complete lines of investigation (which we have no room to detail), made out that if

| | | | |
|---|---|---|---|
| White light | decomposes | 100 | parts of carbonic acid, |
| Red and orange | " | 32.1 | " |
| Yellow | " | 46.1 | " |
| Green | " | 15.0 | " |
| Blue, indigo, and violet | " | 7.6 | " |

The curve of assimilation, nearly parallel to the curve of luminous intensity, culminates between the Fraunhofer lines D and E.

As to phenomena which result from the absence of light, Krauss has studied the difference between stems and leaves when subject to etiolation; the limbs of leaves undergoing complete arrest of development in darkness, while the internodes of stems elongate far beyond normal dimensions. The blade of a leaf, it appears, completes its growth after coming into the light solely from the materials which it assimilates (into starch or its equivalent); starch stored up in the older tissues is of no use to it. In darkness none of this is produced, and so its growth is arrested. The exaggerated length of internodes is due to very different causes, and is related to the phenomena of tension, which always intervene in stems between the medulla, or active part, on the one hand, and the ligneous and cortical cells, or passive parts, on the other.

"From an anatomical point of view the etiolated internodes are distinguished by presenting all the characters of very young internodes just issuing from the bud; the thickening of the walls of the ligneous and cortical cells, which characterizes adult stems is here wholly absent. This thickening, indeed, is related by bonds which are not yet very exactly understood to the presence of leaves on the internode. In darkness the leaves not being developed, the cells retain the primitive thinness of their membranes.

"This being understood, the elongation of the etiolated stems is easily explained, thanks to the intervention of two factors. In normal stems the medulla has always a tendency to elongate; it is the peripheral layers that arrest it; in young stems these are subjected to a tension strong enough to cause them to shorten considerably when they are isolated. But in proportion as their walls become thickened the resistance becomes more effective, and we see this in the fact that their contraction, when they are separated from the rest, becomes less and less. In darkness their walls do not thicken, and nothing is opposed to the elongation of the medullary cells. This is the first factor.

"With regard to the pith itself, M. Krauss has already shown, in a former work (Botan. Zeit., 1867, Nos. 17, 18), that it has the property of elongation solely by the interposition of aqueous molecules between the cellulose molecules. This interposition may take place in the etiolated as in the normal plant; the pith is, therefore, the only part of the plant which continues to *grow* actively in the dark. This growth is precisely the second factor of the elongation of the internodes; and by combining it with the absence of resistance in the peripheral layers, we can understand that considerable results may be produced."

The recent observations which relate to the action of cold upon plants, notably Gœppert's paper, have been already referred to in our pages, but Micheli's abstract of Schrœder's researches upon the "Spring Period of the Maple" we will reproduce:—

"The author has paid attention to all the successive phases presented by the development of the vegetation, from the ascent of the sap to the moment when the expanded leaves begin to decompose carbonic acid. This is one of those complete and conscientious works which, even when they do not contain any very novel results, are, nevertheless, very useful to read and consult; but it is difficult to give a clear notion of them in a few words. A glance at the course pursued by M. Schrœder will show the great number of facts which group themselves within a framework such as he has adopted.

"The first part is entirely devoted to the study of the sap, its ascent and its composition. The maple, under the latitude of Breslau, "*weeps*" for about a month; the sap rises gradually to a certain level, whence it descends again by degrees, in proportion as the development advances. Holes pierced in the trunk, at different heights, enabled this sap to be collected daily; and very numerous analyses keep us informed of the smallest variation in its composition. It always contains sugar, a transitory product of the transformation of the starch accumulated in the tissues during the preceding summer, and destined to become re-transformed when it reaches the buds. The proportion, faithfully represented by a great number of curves, is but slight, at the first awakening of vegetation ; it increases gradually up to a certain maximum, in proportion as the vital phenomena acquire more intensity ; and, finally, it diminishes when the young organs, approaching the term of their development, are on the verge of sufficing for themselves. These facts are, therefore, perfectly in accordance with such a theory of growth as has been established by the researches of modern observers. The albumen and the mineral salts are successively studied from the same point of view ; and their dissemination in the sap, at different heights at the same moment, and at different periods, is exactly governed by the different phases of development. The second part is devoted to the microscopic examination of the bud. The different substances which are called upon to assist in the development of the young leaf are traced by means of reagents from cell to cell. Two, especially, give origin to detailed observations, namely starch and tannin. The dissemination of the former in the different tissues, its transportation through the starchy layers of the fibro-vascular bundles, its disappearance toward the point of vegetation, at the surface of which it speedily reappears as cellulose—all these different phases are taken up step by step ; and here, again, we find a confirmation of all that theory led us to foresee. As to tannin, it is developed in all the cells of the bud ; and when once it has made its appearance it persists there, without appreciable change. Its function has greatly embarrassed M. Schrœder, as he was unable to recognise in it any of the characters of an excrementitial product, properly so called. The fact that it is constantly to be found in the youngest tissues (in which life is most intense), seems to indicate that it is a sort of final product, charged with a still unknown office in the life of the cell. If the true chemical nature of this substance were better known, the solution of the problem would, perhaps, become easier." A. G.

9. *Botany for Beginners ; an Introduction to the Study of Plants*, by MAXWELL T. MASTERS, M.D., F.R.S. London, 1872: Bradbury, Evans & Co. Pp. 185, 18mo. A series of articles treating of elementary botany with admirable freshness and clearness, and illustrated by wood-cuts of uncommon excellence, attracted our attention during the past year in the pages of successive numbers of the Gardeners' Chronicle. These are gathered into this little volume. The articles, it appears, were from the pen of Dr. Masters; the illustrations were contributed by Mr. Worthington Smith. There are ten chapters, or lessons. The first, explaining how to begin, and starting with early spring flowers, is a study of a willow and poplar, followed by the ash and elm. The second, tulip and hyacinth. The third, the apple and cherry, followed by the lilac compared with the ash, and so on. At the end are short chapters on fruit and seeds; on seedling plants and growth; on classification, description, and points to be looked to in each organ; and finally, a particularly good one on plant life.

The book is truly admirable in plan and execution,—especially so for the skill with which the main points are chosen and handled, and less relevant matter passed by. There is not a particle of rubbish from old books; but room is found for some notice of phyllotaxy, a view of insect-fertilization in orchids, and a fair account of natural selection. The natural classification of plants is characterized as an attempt to determine their degree of relationship, and to ascertain their lineage. A. G.

We learn that Dr. Robert Wright, the distinguished Indian botanist, recently died at Granby Lodge, his residence in England since he returned from active service.

G. F. Reuter, the curator for many years past of the herbarium of M. Boisner, and an excellent botanist, died in June at Geneva. A. G.

10. *Musci Appalachiani: or specimens of Mosses collected mostly in the eastern part of North America;* by Coe F. Austin, Closter, New Jersey, 1870.—To the students of bryology no auxiliary is more important than a collection of well-prepared and reliably-named specimens. Such a collection Mr. Austin has given us under the title above cited; and it comes quite opportunely, as those of a similar kind, relating to North American bryology, are now with difficulty obtained.

The *Musci Appalachiani* are put up in uniform sets, each set consisting of copious and excellent specimens of 450 species and varieties, fastened on white paper of suitable sizes, and arranged in proper order within folio sheets of strong brown paper, the whole secured between two thick paper boards. To each species and variety is attached a printed label giving the name, the necessary synonyms, the habitat, and in the case of a new species, a sufficient Latin character. A title-page, and complete index, together with a separate pamphlet containing the labels, accompany each set.

The specimens, with very few exceptions, were collected by Mr. Austin in New Jersey—a state representing more fully, perhaps, than any other, our bryological flora east of the Mississippi river. Its northern portion, being traversed by ranges of the Appalachian mountains, furnishes the species peculiar to such stations. In its central portions are found the species of the middle and western States; and on its eastern and southern borders occur in "The Pines," so-called, species ranging south on the Atlantic coast as far as Florida and even beyond to the West Indies. This latter portion of the State is remarkably rich in species of Sphagnum, fine and abundant specimens of which form a special feature of the collection.

It is gratifying to find, as is shown by the collection, that within the limits indicated in the title, large additions have recently been made in new species, and no species which, though previously known, have not heretofore been detected in this country. For these additions to no one are we so much indebted as to Mr. Austin himself. Among them may be mentioned—*Sphagnum Portoricense*, *Sph. Austini*, *Sph. fim-*

*briatum*, *Sph. Girgensohnii*, *Sph. teres*, *Sph. Pylaesii* (in fruit), *Sph. cyclophyllum* (in fruit), *Sph. neglectum*, *Sph. laricinum*, *Sph. Wulfianum*, *Sph. Lindbergii* ; *Micromitrium Austini*, *Micr. synoicum*, *Micr. megalosporum* ; *Ephemorum papillosum* ; *Anoectangium Peckii* ; *Conomitrium Hallianum* ; *Pottia riparia* ; *Didymodon cylindricus* ; *Didym. diversifolius* ; *Desmatodon Porteri* ; *Orthotrichum Porteri*, *Orthot. Peckii*, *Orthot. Lescurii*, *Orthot. sordidum*, *Orthot. Ohioense*, *Orthot. citrinum* ; *Bryum cyclophyllum* ; *Fontinalis Sullivanti*. *Font. filiformis* ; *Omalia gracilis* ; *Leskea Austinii* ; *Rhynchostegium geophilum* ; *Plagiothecium Mullerianum*, *Plag. Passaicense*, *Plag. latebricola*, *Plag. turfaceum*, *Plag. subfalcatum* ; *Hypnum Bergense*, *Hyp. Closteri*, *Hyp. Novae-Caesareae*. The *Musci Appalachiani* can be had of Mr. Austin at the exceedingly low price of twenty-five dollars per set. His address is Closter, New Jersey. W. S. S.

11. *Gray's Botanical Series. Botany for Young People*, Part II. *How Plants behave* ; by Asa Gray. 46 pp. 16mo, with 40 wood-cuts. New York. 1872. (Ivison, Blakeman, Taylor & Co.) —This little volume for young people has been made every way attractive:—in its tinted paper and excellent printing by the publishers, but more, in its simple and interesting account of the habits of certain plants, by the author. The special subjects of the chapters are: I, How plants move, climb and take positions; II, How plants employ insects to work for them; How certain plants capture insects.

12. *Journal of Zoology.*—A new Zoological Journal (Journal de Géologie) has been commenced in Paris by Prof. Paul Gervais of the Muséum de Paris. Its first number appeared in January of the current year, and it will be issued every other month. Each number is to contain 5 or 6 sheets and 4 or 5 plates. Those desiring it should address M. Arthur Bertrand, 12, rue Hautefeuille, Paris. Price for subscribers in foreign countries, 30 francs a year.

13. *Zoölogie et Paléontologie Générales, par M. Paul Gervais.* —Under this title, 5 numbers have been issued of a work on Fossil Vertebrates and their relation to living species. It will be completed in about 13 numbers, containing each 3 sheets of text, and 4 lithographic plates, costing each 5 francs.

14. *Rectification of T. A. Conrad's "Synopsis of the Family of Naiades of North America"* ; by ISAAC LEA, L.L.D. 46 pp., 8vo. Philadelphia, 1872.—A new edition, originally from the Proceedings of the Acad. of Nat. Sci. of Philadelphia, Feb., 1853.

## III. ASTRONOMY.

1. *The Double Star Castor.*—In the *Astronomical Notices* for May, Mr. Wilson advances the somewhat startling idea that the components of the double star castor are describing an hyperbolic orbit. The observations since 1740, when plotted, give an apparent orbit of eccentricity 2·2. The real orbit, he claims, has an eccentricity 3·16. The recent observations show very decidedly the necessity of extending the periodic time of the orbit, even if

Mr. Wilson's views be not confirmed. The periods given by Sir John Herschel and Admiral Smyth were 253 and 240 years, but Mr. Hind obtained in 1845 a period of 632 years, and Captain Jacob, in 1846, a period of 653 years. The observations since 1845 seem to require a still further extension of the period, and may require us to accept Mr. Wilson's remarkable conclusions.

2. *Meteorite of Ibbenbühren, Westphalia;* G. Rose.—This meteorite fell on the 17th of June, 1870. It is peculiar in consisting of but a single mineral, and that bronzite, composed of silica 54·51, protoxide of iron 17·53, protoxide of manganese 0·29, magnesia 26·43, lime 1·04, alumina 1·26=101·06. The specific gravity of the mass was 3·405 to 3·404, and that of the crystalline grains of bronzite 3·428 and 3·425.

Rose remarks that one other meteorite, that of Manegaum, investigated by Maskelyne, has essentially the same constitution ($\ddot{\mathrm{Si}}$ 55·70, $\dot{\mathrm{F}}\mathrm{e}$ 20·54, $\dot{\mathrm{M}}\mathrm{g}$ 22·80, $\dot{\mathrm{C}}\mathrm{a}$ 1·32=100·36); while two others consist of a single mineral only, the Chassigny being made up of olivine alone, and the Bishopville of enstatite.

The Ibbenbühren mass is somewhat ovoidal, has a black smooth exterior rind, showing evidences of fusion. The length is nearly five inches (one-eighth of a meter).

3. *Meteorites of India.*—Tschermak has described two Indian meteorites. One fell at Shergotty on the 25th of August, 1865, and closely resembles those of Stannern (1807). It consists of augite, a colorless tesseral silicate of the composition of labradorrite which has been named *maskelynite*, and magnetite. This is the first mention of the latter two species in a meteorite. The other fell near Gopalpoor on May 23, 1865. It consists of nickeliferous iron, magnetic pyrites, chromite, chrysolite, bronzite, and a feldspar-like substance.—*Akad. Wiss. Wien*, Feb., 1872.

## IV. Miscellaneous Scientific Intelligence.

1. *Cause of the blue and violet chatoyant colors of Fishes.*—G. Pouchet finds that there is a constant anatomic cause for the bluish or violet chatoyant color of some fishes. There is under the skin a layer of small ovoidal or irregularly spherical granules, yellow by transmitted light, which is the source of the color, this color being a complementary one to the yellow. The diameter of these *irising* granules varies from 2 to 4 or 5 thousandths of a millimeter; and each is formed of a pile of extraordinarily thin lamellæ, applied one to the other, but separately distinguishable under the microscope. Pouchet concludes that the color is due to a kind of fluorescence.—*Acad. Sci., Paris, May, Les Mondes, June* 6.

2. *Iron in the blood.*—Boussingault finds the amount of metallic iron in aliments as follows: the minimum, in carots, 0·0009 gram; the maximum, in the blood of hogs, 0·0534; in beer, ·0040; in the wine of Beaujolais ·0109. The ration of a French sailor in the marine service contains 0·0661 gr.; of a French soldier, 0·0780 gr.; of an English workman, 0·0912 gr.; of an Irish workman, 0·1000 gr.;

of a horse of the reserve cavalry, 1·0166 gr.; of a cow, 1·365 gr. In vertebrates the quantity of iron does not exceed a thousandth of the weight; in invertebrates, probably not four ten-thousandths. It is usual to attribute the red color of the blood to the presence of iron. Yet the white blood of invertebrates contains almost as much iron as the red of vertebrates. Also, plants not green, like mushrooms, contain as much iron as the green plants. Boussingault concludes that of all substances the blood is that which contains the largest amount of iron, and of assimilable iron, since it has already been assimilated.—*Acad. Sci. Paris*, *May*, *Les Mondes*, *June* 6.

3. *Prismatic bows on the surface of the Lake of Geneva.*—On February 11, between two and three o'clock, M. Elie Wartman observed two concentric bows with the colors of the rainbow on the surface of Lake Geneva, and during part of the time a third bow. He attributes the phenomenon to the presence of numberless particles floating on the surface of the lake. He remarks that early in February the wind from the southwest carried over the lake immense quantities of fine particles, mixed with smoke from the many chimneys of the city of Geneva, and that these materials, arrested by the severe storm which continued for several days over the country, were distributed over the waters with remarkable regularity; and so vast was the quantity, that the current of the Rhone took several days to carry it off.—*L'Institut*, *June* 5.

4. *Chesnut tree* (*Castanea vesca*).—Mr. C. D'ETTINGSHAUSEN remarks that a species of Castanea was common in the Tertiary flora of Leoben (Styria), and its leaves present a series of variations in form not observed in other species, but that the same have recently been met with in the *Castanea vesca* of the present time, and hence he concludes that the latter is a descendant from the former.—*Akad. Wiss. Wien*, Feb., 1872.

5. *Ueber krystallinischen Hagel im thrialethischen Gebirge, und über die Abhängigkeit der Hydrometeore von der Physik des Bodens;* von H. ABICH. 260 pp. 8vo, with 5 plates and a chart. Tiflis (Caucasus), 1871.—This large volume on the crystalline hail of the Thrialeth mountains in the Caucasus, is a part of a work entitled "Materialen zu einer Klimatologie des Kaukasus," issued under the direction of A. Moritz, Director of the Observatory at Tiflis. The work has more than a meteorological value, for it treats of the orography of the mountain region of Thrialeth and of the volcanic Meridian mountains, devoting over twenty pages to these subjects. It then describes a large number of hail storms that occurred in the region, giving careful statements as to the attending phenomena, and the special peculiarities of the storms in different parts of the Caucasus, and discussing the theories of the origin of hail. Prof. Abich is an eminent geologist as well as physicist, and has here made the most important contribution to the science of hail that has appeared. Of the figures of hailstones, those of plates 2 and 3 are of very remarkable form. Most of them have a central portion, 1¼ to 1½ inches in diameter, radiated outward in structure, but divided into six segments by

radiating planes; and over the exterior large angular prismatic crystals stand out that are half an inch and larger in diameter, and in some cases an inch or more long.

6. *Die Wirbelstürme, Tornadoes und Wettersaülen in der Erd-Atmosphäre, mit Berücksichtigung der Stürme in der Sonnen-Atmosphäre; dargestellt und wissenschaftlich erklärt* von Dr. THEODOR REYE, Prof. Univ. Strasburg. 248 pp. 8vo, with 4 storm-charts and 30 woodcuts and lithographs. Hanover. 1872. (Carl Rümpler.)—In this work Professor Reye has treated the subject of whirlwinds, tornadoes and waterspouts with great fulness and system. He presents the facts at length, discusses freely the various theories, and gives the practical rules which flow from the observations hitherto made. He does full justice to Redfield's views and observations, quotes largely from his papers, copying some of his charts; and also cites freely from the papers of Professor Loomis, Reid and Piddington, and from various European sources. He adopts the idea of cyclones, and illustrates the earth's cyclones by descriptions, and a fine plate illustrating the cyclones in the sun's atmosphere. The frontispiece of the work is a copy of Olmsted's plate of "Whirlwinds from the burning of a cane-brake," from II, vol. xi, of this Journal (1851). The work closes with a list of books and articles on the subject of which it treats.

7. *American Journal of Conchology;* GEORGE W. TRYON, Jr., Editor.—The cover of the 4th number of vol. vii. announces that the publication of this journal will be therewith discontinued. It has done good service both to zoölogy and geology, and we greatly regret for the sake of American science that it cannot be sustained. The last number, like very many of those preceding it, contains excellent colored plates of shells; in this case illustrating an article by A. J. Garrett of Tahiti, on Feejee fresh water and terrestrial gasteropods. The papers hereafter presented will be published by the Academy of Natural Sciences, to whose conchological committee its subscription list has been transferred.

8. *Notes sur les Tremblements de Terre en* 1869, *avec Supplements pour les annèes antèrieures de* 1843 *à* 1868, par M. ALEXIS PERREY, Prof. Hon. à la Faculté des Sciences de Dijon. From *Mem. Cour.* of the Belgian Academy for 1872.—Prof. Perrey, the indefatigable investigator of earthquakes, here gives a review of the facts connected with the earthquakes of 1869, besides adding to his collection of facts pertaining to previous years.

9. *Half-Hour Recreations in Science*, published in numbers of 20 to 60 pages, by Lee & Shepard, Boston. Price of each, 25 cts. —The first number of the series contains Proctor's excellent paper on "Strange Discoveries respecting the Aurora and Recent Solar Researches, and the second is an instructive lecture by Prof. R. Virchow on "The Cranial Affinities of Man and the Ape. The others out, or announced as in progress, are of equal interest, and give full assurance that the series will be a valuable addition to any library. The paper and printing are excellent.

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[THIRD SERIES.]

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## ART. XIII.—*On the Evaporative Efficiency of Steam Boilers;* by WM. P. TROWBRIDGE, Professor of Dynamic Engineering in the Sheffield Scientific School.

*Transfer of heat.*—The quantity of water which a steam boiler apparatus will evaporate in a given time depends primarily upon the temperatures to which those parts of the plates of the boiler, known as heating surfaces, are exposed. In the furnace, the heating surface is exposed to the radiant heat of the incandescent fuel, and to the contact of the heated gases; and in the flues to the contact of the gaseous products of combustion alone. The temperature of the fuel, and the initial temperature of the gases, depend on the intensity of the combustion, or the quantity of fuel burned on each square foot of grate surface in a unit of time, and also on the kind of combustion which takes place: *perfect combustion*, in this connection, designating that in which no combustible gases or uncombined oxygen escape to the chimney. This condition is presumed to give rise to the highest possible temperatures in the residue of fuel, and in the escaping products.

The quantity of fuel burned, the constitution of the escaping gases, and the resulting temperatures, depend on the following conditions:

1. The quantity of air which passes through the furnace in a unit of time.

2. The amount of surface of the fuel with which this air comes in contact.

The quantity of heat transferred to the water within the boiler will then depend on the amount of heating surface exposed to direct radiation, the amount of heating surface exposed to the contact of the gases, and the *laws* of absorption or *transfer of heat* under these conditions.

The quantity of air supplied to the furnace, in a unit of time, depends upon the chimney, or other apparatus for producing the draft, and the surface of contact of air and fuel; upon the kind of fuel, size of lumps, thickness of bed, etc. There are other conditions which influence the evaporative efficiency of boilers, when the total heat of combustion is to be compared with the quantity of heat transferred to the water; such as the losses from imperfect combustion, diffusion of heat, escape of heat through the chimney, etc., making an aggregate of losses which must be estimated.

The following general discussion of the problems involved is given, as suggesting a mode of investigation which may lead to more satisfactory experiments on the laws of transfer of heat.

Let Q represent the quantity of heat transferred to the water of the boiler in a unit of time, one hour for instance.

$Q_1$ the portion of this heat which is transferred by radiation in the furnace.

$Q_2$ that part which is transferred by contact of heated gases in the furnace.

$Q_3$ the part which is transferred by contact of heated gases in the flues.

Then $Q = Q_1 + Q_2 + Q_3$.

In this expression, the first member may be regarded as known, because it may be easily ascertained by experiment. Of the terms of the second member, $Q_1$ represents the quantity of heat transferred by radiation from the surface of the fuel.

According to the laws of Dulong and Petit, this quantity may be represented by

$$Q_1 = q \times G = C.\, a^{\theta} (a^{t} - 1). \times G.$$

in which $q$ represents the quantity of heat transferred by radiation from one square foot of grate surface in a unit of time, G the grate surface in square feet, C a constant, $a = 1{\cdot}0077$. $\theta$ represents the temperature of the absorbent body, or the water in the boiler (a constant which may be determined by observation), and $t$ the difference between this temperature and the higher temperature of the incandescent fuel.

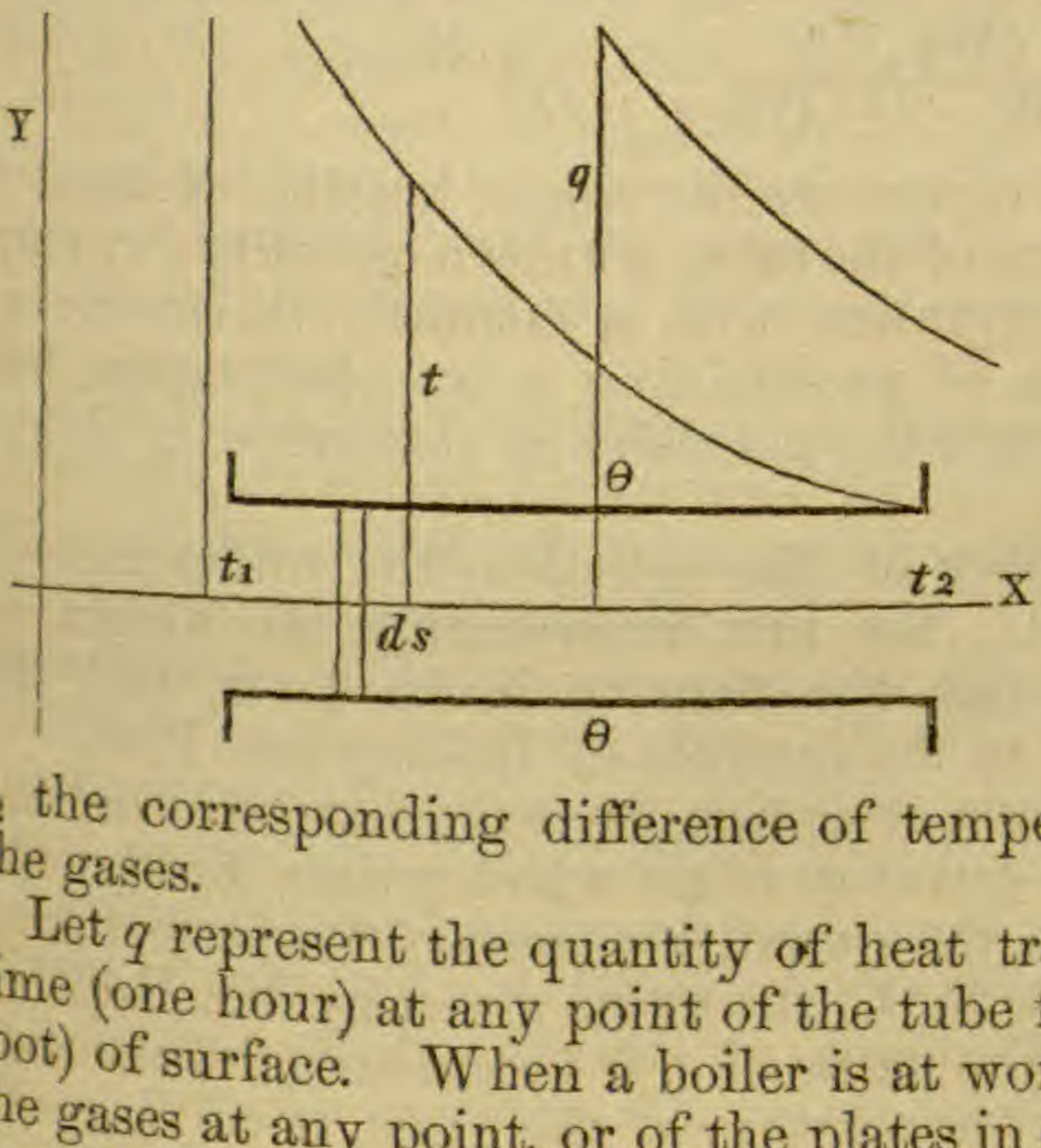
By the law of transfer of heat by contact of gases, given by Dulong and Petit, we shall have

$$Q_2 = F. \times C.'\, t_1^{1{\cdot}233}.$$

F representing in square feet the furnace surface, $C' t_1^{1{\cdot}233}$ the quantity of heat transferred by contact of the gases in the furnace in a unit of time; in which $t_1$ represents the difference of temperature between the gases in the furnace, and the temperature, $\theta$, of the water in the boiler; this difference being constant for the whole furnace surface. It is presumed that the law of Dulong and Petit for *the cooling of bodies by contact of a cooler gas is also the law of heating by a hot gas;* the side on which the excess of temperature exists being a matter of indifference.

For the transfer of heat by contact in the flues, the application of the law renders it necessary to take into account the diminution of temperature, from the initial temperature $t_1$ to the temperature $t_2$, of the gases, as they leave the flues and pass into the chimney.

Let the combined flue surfaces be represented by a single cylindrical surface in the form of a tube, as in the figure below, and assume two coördinate axes, one in the axis of the tube and the other perpendicular to it; and let $t_1$ be the difference of temperature of the gases and the water outside of the tube, at the entrance, and $t_2$ the corresponding difference of temperature at the exit of the gases.



Let $q$ represent the quantity of heat transferred in a unit of time (one hour) at any point of the tube from one unit (square foot) of surface. When a boiler is at work the temperature of the gases at any point, or of the plates in contact with the gases, may be regarded as constant, and it will not, therefore, be necessary to regard the conductivity of the plates; this being necessary only when the surfaces of the plates undergo a change of temperature, as in getting up steam.

If the heating surface represented by the surface of the tube be divided into elementary portions by planes perpendicular to the axis, the element of the surface between two planes may be represented by $d\,s$, and the quantity of heat transferred by contact through the element will be represented by $q\,d\,s$.

If we represent by $c$ the specific heat of the gases, by W the weight of gas which passes the element $ds$, in a unit of time, (one hour), and by $dt$ the element of time; it is evident that the cooling of the gas in the time $d\,t$ will be $c\,\mathrm{W}\,d\,t$, expressed in units of heat, and this quantity must be equivalent to that received or absorbed by the water, represented above by $q\,d\,s$: a relation expressed by the equation:

$$q\,ds=c\mathrm{W}\,dt \qquad \text{from which} \qquad ds=\frac{c\mathrm{W}\,dt}{q}.$$

Introducing now the law of Dulong and Petit, by substituting for $q$ its equivalent $\mathrm{C}'\,t^{1\cdot233}$, the expression becomes

$$ds=\frac{c\,\mathrm{W}\,dt}{\mathrm{C}'.\,t^{1\cdot233}} \qquad \text{or} \qquad \frac{ds}{c\,\mathrm{W}}=\frac{t^{-1\cdot233}\,dt.}{\mathrm{C}'}$$

Integrating between the limits $t_1$ and $t_2$ we obtain

$$-\frac{s}{c\,\mathrm{W}}=\frac{t_1^{-\cdot233}-t_2^{-\cdot233}}{-\cdot233\cdot\mathrm{C}'}$$

from which the value of $t_1$ may be obtained,

$$t_1=\left(\frac{c\mathrm{W}t_2^{\cdot233}}{c\mathrm{W}-\cdot233\,\mathrm{C}'\mathrm{S}.\,t_2^{\cdot233}}\right)^{\frac{1}{\cdot233}}.$$

In this expression S represents the whole heating surface of the boiler, or the surface of the tube, a known quantity; $t_2$ may be ascertained by observation with a common thermometer. But no accurate mode of ascertaining $t_1$ by observation has yet been found, and hence the necessity of this mode of determination.

In the second member of the equation the undetermined quantities are W and C′, the first representing the weight of gas which passes through the furnace in an hour, and the second the constant C′ of the formula of Dulong and Petit.

If W be taken on the assumption, *generally acquiesced in*, that in ordinary boilers the quantity of air which passes through the furnace is just double the quantity necessary for perfect combustion, and we assume for C′ the value given by Mr. Hopkins for carbonic acid gas, the value of $t_1$ is completely determined; and to find the initial temperature of the gases, it will be only necessary to observe, with the aid of a common thermometer, the temperature of the gases at their exit from the tube or boiler. Having found $t_1$, the quantity of heat transferred by contact of the gases, in the flues, to the water, will be represented by

$$c\,\mathrm{W}\,(t_1-t_2).$$

The value of Q then becomes

$$\mathrm{Q}=\mathrm{C}\,.\,a^{\theta}.\,(a^{t}-1)\,.\,\mathrm{G}+\mathrm{F}\,.\,\mathrm{C}'.\,t_1^{\,1\cdot233}+c\,\mathrm{W}\,(t_1-t_2).$$

in which all the terms become known, except the first term of the second member.

The value of the expression $\mathrm{G}\times\mathrm{C}\,.\,a^{\theta}\,(a^{t}-1)$ being thus determined, the separate influence of the radiant heat of the furnace becomes known, and the value of $t$, the difference of the temperature between the *fuel and the water*, may, by the aid of Hopkins' value of C, be ascertained.

Resuming the expression $\frac{d\,s}{c\,\mathrm{W}} = \frac{t^{-1\cdot233}dt}{\mathrm{C}'}$, if we suppose the tube to be extended indefinitely and the gases to be forced through the tube by some extraneous pressure, the difference between the temperature of the gas within the tube and the water would ultimately become zero.

Integrating the above expression between the limits $t_1$ and $t_0$ we should obtain an expression by which the relations of $t$ and S. and of $q$ and S. become known. $t$, the difference of temperature at any point, will be $t = \mathrm{C}'' \frac{1}{\mathrm{S}^{4\cdot3}}$ and $q = \mathrm{C}''' \frac{1}{\mathrm{S}^{5\cdot3}}$ equations which indicate the form of curve of temperatures along the axis of the tube, and also the form of curve of emissions of heat along the axis. It is evident that these curves approach the axis rapidly. The constants C″ and C‴ involve both $c$ W and the constants of the law of Dulong and Petit, and the curves may therefore be constructed by assuming these as before.

Unfortunately, however, it is not probable that either of these quantities can be assumed in the present state of knowledge of the action of chimneys, the combustion of fuel, and the phenomena of transfer of heat. It is certain that the assumption of twice the quantity of air necessary for combustion is not a universal law. And in regard to the constants of the formulas of Dulong and Petit, it can hardly be said that they have been completely determined or interpreted. The quantity of heat transferred by contact of a gas depends on the mode or rate at which the gas is supplied to, or removed from, the surface to be heated or cooled. This is the principal element in the problem. It seems evident that, according to the present state of knowledge of the *conductivity* of gases, heat must be transferred with excessive slowness, unless currents are established in order to bring fresh particles of gas in contact with the surface to be heated or cooled each instant.

By making use of an apparatus like the steam boiler it may be possible to determine these constants, as well as the quantity $c$ W, for different dimensions of chimneys or force of draft, thickness of bed of fuel, etc. It would be necessary to make experiments under such conditions that some of the quantities could be eliminated, as, for instance, the initial temperature. If the same chimney be employed with the same kind of fuel, thickness of fuel on the grate, and under the same general conditions of barometer and thermometer, the initial temperatures must be the same, and, by properly varying the heating surface, different values of the final temperatures might be obtained and observed. The quantity of air used being measured, the law of change of temperature, or of transfer of heat, might then be ascertained for the flues and for the furnace separately.

If the laws of Dulong and Petit be true for high temperatures, it is apparent that the influence of the furnace at high temperatures must preponderate. Under any circumstances, it seems impossible to find a *scientific* solution of the important problem of efficiency of boilers in the generation and transfer of heat, until these questions are solved.

The laws of Dulong and Petit have not been verified for high temperatures. At very low temperatures the quantity of heat transferred by contact of a gas and by radiation, according to these laws, will be nearly identical, and the higher the temperature the greater becomes the difference in the effects.

Common observation shows that to heat or cool a body rapidly by contact of a gas, the gas must be supplied and removed rapidly. It seems improbable, therefore, that there can be any general expression for the quantity of heat transferred in this manner which does not involve this idea in some other way *than by the constants which have been adopted.*

The mode of determining the initial temperatures of the furnace and gaseous products of combustion generally employed, up to the present time, has been to assume, from the partial experiments of Péclet and others, that half the heat of combustion is usually imparted to the gases, while half passes off as radiant heat, and then to estimate the temperatures on the further assumption that a definite known amount of air passes through the furnace for each pound of coal burned. From the nature and phenomena of combustion it can hardly be supposed that any such law as that assumed by Péclet can be universally true. The temperature of the residue of the solid combustible must depend on the special circumstances of combustion in each case, and especially on the law of radiation, at different temperatures.

If it were possible to observe accurately the temperatures of furnaces and flues, the solution of all these questions would be greatly simplified; and the discovery most needed to advance this branch of physical research is a thermometer for determining high temperatures. The only method available at present seems to be that of going back from a temperature which falls within the range of ordinary thermometers to the initial temperatures, by analysis. And the mode of investigation herein suggested is offered as one which apparently conforms to known or accepted laws, and does not require doubtful assumptions. Whether the laws of Dulong and Petit are true for all differences of temperature, and whether these laws may not be revised so as to involve directly the dynamic theory of heat, are questions which such investigation may help to solve.

The complete expression for the total heat of combustion of fuel in the boiler will be

$$\begin{aligned} E &= Q + E_1 \\ &= Q_1 + Q_2 + Q_3 + Q_4 + Q_5 \end{aligned}$$

in which $Q_4$ represents the heat in the gases after they leave the flues represented by $c\,W\,(t_2 - t_0)$, and $Q_5$ the losses from external radiation, incomplete combustion, etc., which can only be estimated.

## ART. XIV.—*Description of two new Land Snails from the Coal-measures;* by F. H. BRADLEY.

IN the summer of 1869, I found that the concretionary limestone accompanying the underclay of coal No. 6 of the Wabash Valley section (see Illinois Rep., iv, 254), at Pettys's Ford of the Little Vermilion river, below Georgetown, Vermilion county, Illinois, contained numerous minute shells. (A small fragment of fish-bone is the only other fossil yet found in it.) A *Pupa* was readily recognized, and, from its outline alone, referred to *P. vetusta* Dawson. The other of the two species was doubtfully referred to *Zonites.* Later, in cleaning out the mouths of some individuals, a distinct tooth was found on the columella of the *Pupa*, and a broad lamellar extension of the columella of the so-called *Zonites.* The *Pupa* was evidently not *P. vetusta;* and the correction was sent to Prof. Worthen, but was overlooked when my report on the county was printed. Upon showing the so-called *Zonites*, in its cleaned condition, to Mr. F. B. Meek, he at once recognized it as congeneric with a minute species, *Anomphalus rotulus*, described by Meek and Worthen from Macoupin Co., Illinois (Proc. Phil. Acad., 1866, p. 268). They refer the genus to the Rotellidæ: I am rather inclined to refer it to the Helicidæ.

I append descriptions and figures.

### *Pupa Vermilionensis*, n. sp., Fig. 1.

Shell imperforate, spindle-shaped, tapering to an obtuse apex, covered with fine ridges (25 or 30 to the millimeter) parallel with the lines of growth. Aperture oblique, oval, rarely compressed. Outer lip thin, slightly reflexed. Columellar lip reflexed, thickened, furnished with a single central tooth projecting about $4^{mm}$. Junction of columellar and outer lips sometimes angular and slightly dentiform. In old individuals, the columellar tooth is often continuous through an entire turn or farther—not seen on shells having less than three turns.

1.

2.

Adult shells consist of five or six turns. Last turn forms nearly half of the shell. Turns rounded. Suture impressed. Surface glossy. Color bluish black.

Total length, 3·6$^{mm}$.; width, 2$^{mm}$.

*Anomphalus Meeki*, n. sp., Fig. 2.

Shell broad, depressed, helicoid; spine obtuse, consisting of 3 or 3½ turns. Surface glossy, nearly smooth, showing, under a lens, fine oblique lines of growth too indistinct and irregular to be counted, perhaps fifteen to the millimeter. Aperture oblique, oval, greatly contracted by a broad lamellar expansion of the columella extending more than half way across, even in small individuals. Outer lip thickened, slightly reflexed. Suture slightly impressed. Each turn including only about half of the preceding one, thus distinguishing the species from *A. rotulus*, the type of the genus. Imperforate, but last turn slightly excavated in the umbilical region. Last turn more than half of the shell.

Total length, 3·2$^{mm}$.; width, 4$^{mm}$.

## ART. XV.—*On Glacial Phenomena in the vicinity of New York City;* by R. P. STEVENS, M.D.

THE evidences of a glacier once moving over the island of New York are of three classes: 1st, The grooves or striæ, and other results of the abrasion of the rocks of the island, wherever they are visible. 2nd, The mantle of drift which partially conceals the rocks. 3rd, Facts observed over the hills of the neighboring island of Long Island. All the evidences of the first class show that the movement and agencies causing them proceeded from the northwest toward the southeast. I have made many examinations and measurements, from one end of the island to the other, and have never found any single instance to the contrary. My observations have also extended to Staten Island, New Jersey, and northwestward to the Delaware river, and up the Hudson river on both shores—and also over on the Highlands and mountains separating New York State from the States of Connecticut and Massachusetts. All my observations show the same general direction.

Since commencing this paper I have made many observations in the Central Park, and find the range to be from N. 20° W. to N. 30° W., or S. 20° E. to S. 30° E. Prof. Cooke, in his report on the Geological Survey of New Jersey, found all his measurements on the Palisades west of the Hudson river and opposite New York to lie between N. 20° W. and N. 75° W. The glacier, then, moved from the N.W., as Prof. Dana has demonstrated it did in New England. Following this northwest direction from this island over the Highland range of "Archæan" rocks at the Ramapo Gap, N. Y., we find the same general evidence that we do elsewhere eastward. The same evidences can be seen in the Pompton Gap, Dover, and at Lake Hopatcong, N. J.

Some years ago I traversed the heights from this lake to West Point on the Hudson, and everywhere the evidence of some agent moving southeastward over them, rounding their summits, stossing them on their western slopes, was always present before me. The sum of all this evidence confirms Prof. Dana's theory of a glacial plateau on the highlands of Canada.

The second class of evidence—the material composing the mantle of drift—always shows it to have been transported from the northwest. Both on this island and Long Island the material is from rocks known to lie to the northwestward. Thus on the island we find boulders and huge masses of the serpentine and trap rocks of New Jersey blended with the red sand rock of the same State. In Brooklyn on Long Island we find, in addition to the rocks of New Jersey, those from New York island blended with the others. I have seen huge masses of anthophylite in Atlantic street, Brooklyn, which must have come from the parent bed of this rock on 10th Avenue and from West 50th to West 60th street. Careful measurement of the direction of the movement which must have transported these rocks shows it to have been from N. 10° W. to S. 10° E. This course tallies with measurements made on the palisades by Prof. Cooke. The agency which threw this mantle over the island had power to take up and transport immense masses of red sandstone from New Jersey to New York and Long Island. Many blocks in the city, as at East 73d, East 74th, East 75th, and East 76th streets, Third avenue, N. Y., lying beneath the surface soil, are four, six, and eight feet thick, giving in the excavations an appearance of being independent red deposits in the drift.

The third class of evidence is the immense drift deposits on Long Island. These stretch from Oyster Bay S. 60° W. to Fort Hamilton, and over to Staten Island. Was not this ridge a terminal moraine? Through this moraine the Hudson river breaks at the Narrows at almost right angles to the trend of the Hudson valley.

The material composing this moraine is made up of detritus from New Jersey and Manhattan Island. Boulders of trap, and gneiss and granite cover all the surface east as far as Oyster Bay. The shore of Long Island between Oyster Bay and Smithtown I have not visited. At the latter point, and along the Long Island railroad, beyond Brushville, there is an absence of all kinds of boulders. Underneath the surface the land is full of boulders of trap and gneiss through all the moraine.

On this island I have never seen any boulders of fossiliferous rock. They have, however, been seen by others. On the Jersey side I have seen them from the Corniferous limestone of the Roundout valley. Now, as this rock is not seen in the lower end of the Roundout valley, i. e., the northeast end, but in the southwest on the Neversink portion—we have in this instance further evidence of a movement from the northwest. Under this class of evidence we may notice the boulders of granular gneiss from Archæan rocks of the Highlands, which I have found as far east as East New York.

On the island of New York, in the deep excavations for sub-cellars for blocks of buildings, we often find modified drift, to the depth of twenty to thirty feet, entirely composed of clean washed sands—derived from the sandstones of New Jersey.

New York, 24 Pine St., June 17, 1872.

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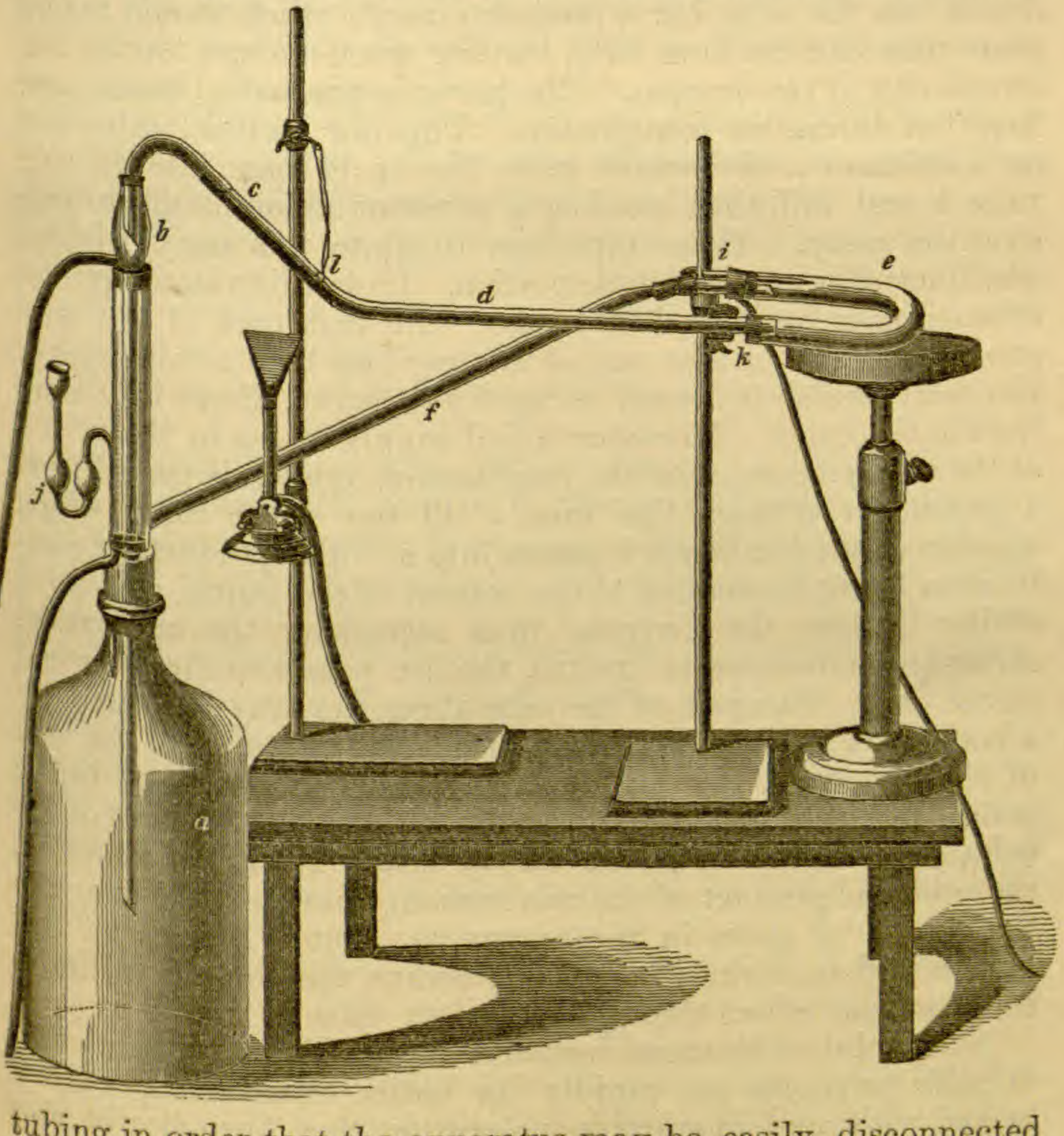
## ART. XVI.—*Contributions from the Laboratory of the Sheffield Scientific School.* No. XXIV.—*On the Estimation of Sulphur in Coal and Organic Compounds;* by W. G. MIXTER.

THE determination of sulphur in organic substances by many of the methods in use is not only a difficult and tedious operation, but the amount of fixed reagents often employed greatly increases the liability to error. In the process here described, substances are burned in oxygen and the sulphur is condensed from the gaseous products in the form of sulphuric acid.

Experiments made by passing the products of combustion of sulphur-compounds through nitric acid failed to give satisfactory results. A variable loss was due to a dense white fume containing sulphuric acid, which was not completely absorbed by water or by caustic alkalies. The apparatus here described was designed for making the combustions in a confined volume of gas, to avoid this source of error. The bottle (*a*), see figure, has a capacity of from 4 to 10 litres, according to the amount of oxygen required. The neck should be large enough for a stopper 35 to 40 mm. in diameter. The condenser *b* is made of rather thin tubing 14 mm. in diameter; at the upper end it is expanded to a bulb in order to admit some motion to the tube *c d*. Below the bulb it is surrounded by a water-jacket 22 cm. high: from the point where it enters the stopper of the bottle it is narrowed somewhat for convenience of fitting. The combustion tube *c d* is made of hard glass of 12–15 mm. internal diameter; the portion *c* is 18 cm. from curve to curve, and is protected by a sheet-iron trough lined with asbestos; the part *d* is from 35 to 45 cm. in length. The wire attached at *l* is to sustain *c* in case *d* breaks; *c* is joined to *b* by a collar of black-rubber. The U-tube *e* is connected with *d* by a rubber collar drawn over the latter at *k*; this U-tube is slightly inclined, that no liquid may run against the rubber connectors. The tube *f* connects *a* with *e*; it is narrowed at both ends to 10 mm. diameter, Near the upper end it is jointed by a piece of black-rubber tubing in order that the apparatus may be easily disconnected at *k*. The ends of *f* extend 2 cm. or more beyond the stoppers. Through the rubber stopper *i* a small glass tube passes beyond the end of *f*, where it is narrowed to an opening of 1 mm. The double bulb tube *j* is to accommodate variations of pressure, and to admit air as the original volume of gas diminishes during the combustion. The tubes *b*, *c*, *d* and *f* should at no point have an internal diameter less than 8 mm.—10 mm. is preferable—and the narrowed ends should be cut obliquely that drops of water may not obstruct the circulation. The rubber stoppers and connections should be freed from adhering sulphur by heating in a solution of sodium hydrate. The joints



of the apparatus are sufficiently tight when water will stand in one limb of the safety tube.

The bottle *a* is filled over water with oxygen, and, if necessary, rinsed with distilled water; a few drops of bromine are poured in, the tubes adjusted and a slow stream of water made to flow through the water-jacket. The assay, if not volatile, is introduced into the tube *d* in a platinum tray*, which should not fill more than half the bore of *d*, leaving space enough for the free circulation of the oxygen. The part *c* is gradually heated and kept hot during the combustion. This hot inclined tube acts as a chimney; the heated gases rise in it, pass into the cold tube *b* and fall, thus causing a constant stream of gas to pass over the assay. It is important to ignite the assay without distilling off any considerable portion. To do this a small splinter of wood may be placed in contact with that part of the substance nearest *l*, or that end of the tray may hold a thin layer of the assay, which is heated as rapidly as safety allows by a lamp held in the hand. To ensure a full supply of gas in the tube *d* at the commencement of the combustion, oxygen is passed from a gasometer through the tube *i* till the white fume which appears in the condenser *b* passes into *a*. The products of combustion being denser fall to the bottom of the bottle, and for a while displace the oxygen, thus increasing the circulation. After the substance is ignited, the fire passes to the other end of the tray. The part of the tube about the tray is heated by a lamp as is required to keep up the combustion. At the end of the operation the heat is increased. If drops of liquid collect in *c*, and are liable to run down to the hotter parts of the tube, they should be driven off by heat. If carbonic acid be the principal product of the combustion, there is little change in the volume of gases in the apparatus; but if water and sulphuric acid are formed in much quantity, the volume is diminished and air enters through the safety tube.

Most solid substances heated alone in the open tray yield volatile products too rapidly for entire combustion, but if mixed with sand in suitable proportion they burn slowly and completely. Liquids should be enclosed in narrow tubes sealed at one end and drawn out at the other to a capillary bore for two or three inches of length. Upon the point of the tube a bit of platinum sponge is fixed to assist the oxidation. The liquid should not fill more than two-thirds of the wider part of the tube.

Before introducing very volatile substances, the 10 cm. of the combustion tube *l d* should be heated to dull redness.

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* A platinum tray which answers well may be made 10 to 20 cm. long, 10 mm. wide, and 7 to 10 mm. deep by bending thin foil over a glass tube. The ends may be roughly bent together or left open.

Oxygen is passed in at *i*, the tubes are disjointed at *k* and the tube holding the assay is then pushed in, till the platinum just reaches the heated zone. The apparatus being connected at *k* slow volatilization of the liquid is effected by cautiously applying a flame under the empty portion of the tube containing the substance, so as to maintain the platinum sponge in a steady glow. As soon as a cloud of combustion-products appears in the vessel *a*, oxygen is shut off from *i*. When all the liquid has distilled from the interior tube, the tube *c d* is cooled slowly and the apparatus is left for two hours or until the fume has entirely subsided. If no odor of bromine be perceptible when the apparatus is disconnected at *k* to remove the tray or tube, a few drops of it should be poured through a funnel-tube put in the place of *j*, and the whole allowed to stand some time to ensure complete oxidation of the sulphur-compounds, and deposition of the sulphuric acid.

The tubes *d* and *e* are then rinsed into a beaker, this water is poured into *b*, which is then thoroughly washed by the aid of the wash-bottle; the large rubber stopper is lifted from the bottle and the lower part of *b* rinsed; without removing the tube *f* from the stopper, it is rinsed into a beaker and finally the bottle is carefully washed. The solution obtained, which need not exceed 500 cc., is evaporated to a small volume, filtered if necessary and the sulphuric acid precipitated by barium chloride. The barium sulphate washes easily, as the solution contains no nitrates or fixed salts. Its purity is ascertained by treatment with water and a few drops of chlorhydric acid, warming some time, filtering and reweighing. In case the substance leaves an ash or residue in the tray, this must be dissolved in aqua regia, the nitric acid removed by evaporation with strong chlorhydric acid, and any sulphuric acid it may contain separated in the usual manner. In the use of this apparatus there is no danger from explosions if care be taken to have the combustion tube hot enough to ignite combustible vapor. Before attempting to burn a substance in the apparatus, it is best to try it in a large inclined tube open at both ends, or with oxygen supplied at the lower end. Such a preliminary trial will usually indicate the precautions necessary in burning the substance in the apparatus.

The writer found that when oxygen prepared from a mixture of potassium chlorate and manganese dioxide, or from the chlorate alone (no rubber stoppers or connections being employed) stood some time in a bottle containing a few drops of bromine and a little water, the water gave a slight turbidity with barium chloride. Neither the bromine nor the solution of the chlorate gave reactions for sulphuric acid; the manganese dioxide contained, however, a trace of sulphur. Combustions of alcohol and of sugar-charcoal made by the method here described, yielded with barium chloride, a precipitate apparently no greater than that obtained from the oxygen alone, and too slight to influence ordinary results. This shows that with suitable care rubber stoppers are not objectionable.

The following results, obtained in the order they are given, show the applicability of the method, while some of the details mentioned may help to explain the use of the apparatus.

| | Weight taken. | Per cent. found. |
|---|---|---|
| 1. Iron pyrites (mixed with carbon), | 0·0658 | 51·20 |
| 2. " " | 0·0597 | 51·26 |
| 3. Sulphur, | 0·2070 | 99·76 |
| 4. " | 0·2807 | 99·92 |
| 5. " | 0·4951 | 99·93 |
| 6. " | 0·5882 | 100·02 |
| 7. Carbon disulphide, | 0·7725 | 84·12 |
| 8. " " | 0·4598 | 84·16 |
| 9. Bituminous Coal, | 0·6640 | 2·97 |
| 10. " " | 0·7860 | 2·99 |
| 11. Wool, | 0·4640 | 3·44 |
| 12. " | 0·4675 | 3·46 |
| 13. Tobacco, | 2·0720 | 0·37 |
| 14. " | 2·1370 | 0·36 |

Nine liters of oxygen were used in Nos. 1, 2, 13 and 14, and four liters in each of the other analyses.

In No. 1 the iron pyrites was mixed with 1·3 gr., and in No. 2 with 2 gr. of sugar-charcoal, and the residues remaining after the first combustion were mingled with a small additional portion of charcoal, and the combustion repeated to remove the last traces of sulphur. The solutions of the final residues in aqua regia, after evaporation nearly to dryness to expel the excess of acid, gave no turbidity with barium chloride. The sulphur in this pyrites was estimated by another method by way of control.* This mixture of pyrites with charcoal was

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* The analyses of pyrites were mostly made by a modification of the method of Storer and Pearson (this Journal, xlviii, 190). The pulverized iron pyrites (0·2-0·5 grm.) was mixed with thrice its bulk of powdered potassium chlorate, about 25 cc. nitric acid, sp. gr. 1·40 were then added to the mixed powders, and the whole was heated cautiously but not to boiling, for fear of melting the sulphur which separated. A complete solution was obtained in five to ten minutes. It was evaporated to dryness and the evaporation twice repeated with the addition of chlorhydric acid. The barium sulphate was finally precipitated from the solution containing but a few drops of free acid. It was washed first by decantation with water, was then digested with three separate portions of ammonium acetate, and the washing was continued with water till sulphuric acid gave no turbidity in the washings. These precipitates after ignition yielded no barium-salts to hot dilute chlorhydric acid; they were slightly reddish in color, and the sulphur calculated from their weight amounted to 51·69 and 51·73 per cent. respectively.

In two other determinations, potassium-sodium tartrate (no pure tartaric acid being at hand) and chlorhydric acid were added to the solutions before precipitating, with the hope of retaining the iron in solution more perfectly; the barium-used as an imitation of coal, which should contain a known amount of sulphur.

The sulphur used in Nos. 3, 4, 5 and 6 was purified by crystallization from carbon disulphide solution and fused. It was placed in a narrow weighed glass tube from 5 to 7 cm. long, boiled, allowed to cool and the whole weighed. During the combustion of No. 4, the sulphur volatilized so rapidly at one time that a portion escaped unburned, and formed a coating in the neck of the combustion tube and in the condenser, which soon disappeared, probably oxidized by the action of sulphuric acid and bromine.

The carbon disulphide was the commercial article purified by distillation from calcium chloride and quick lime. The boiling point was constant. The per cent. of sulphur calculated is 84·21. On account of its volatility and resistance to the action of bromine, it required more care in its combustion than pure sulphur. It was weighed in a sealed tube having a neck three inches long with a very fine bore. The end of this neck was broken off, and the open point was thrust into a cylinder of platinum sponge wrapped in foil, and the whole was quickly placed in the combustion tube as has been described.

The powdered bituminous coal was burned in a tray. In Exp. No. 10 some tarry matter passed into the condenser. No sulphur was found in the reddish ash, which amounted to 5·57 and 5·22 per cents.

The wool was from South Down sheep. It was purified by washing with soap and afterward with ether, and was dried at 212°. Wool swells so by heat that it cannot conveniently be burned in an open tray. No. 11 was contained in a glass tube sealed at one end. After the volatile matters had been burned off, the tube was taken from the apparatus, the closed end was broken and the charred residue mostly transferred to a tray, which together with the tube was returned to the combustion

sulphate was treated as above described. The precipitates had notwithstanding a reddish color and corresponded to 51·78 and 51·63 per cent. of sulphur respectively. They yielded nothing to dilute chlorhydric acid, and were accordingly free from adhering barium-salts. They were, according to the suggestion of Mitscherlich (Jour. Prakt. Chem. 83, 456), dissolved in strong sulphuric acid, thrown down by water, washed and again weighed. They were now free from iron and corresponded to 51·28 and 51·40 per cent. of sulphur respectively.

In another estimation the barium-sulphate which contained a large amount of ferric oxide was slightly washed with water, then fused with sodium-carbonate extracted with water, and the sulphuric acid thrown down from the aqueous solution after addition of a slight excess of chlorhydric acid. The barium-sulphate, thus obtained free from iron, was digested with ammonium acetate, and finally washed with water. The sulphur calculated from it was 51·27 per cent.

The latter plan of purifying barium-sulphate has long been employed by Prof. O. D. Allen in the analysis of iron ores, and its publication was made nearly simultaneously in the Am. Ed. of Fresenius' Quantitative Analysis, p. 525, from notes furnished by Prof. Allen, and in a paper by Fresenius in his Zeitschrift, x, 52.

tube to complete the oxidation. In No. 12 a small platinum tube, extemporized from foil, closed at one end by a glass plug, was employed.

The pulverized tobacco of No. 13 was burned in an open tray, and the combustion occupied less than five minutes. A small amount of hydrocarbons passed over unconsumed, and owing to the intense heat a white sublimate formed above and beyond the tray. No. 14 (same tobacco) was mixed and covered with sand; the combustion lasted twenty minutes, and the oxidation was complete. The ash of the tobacco retained by far the larger part of the sulphur. In Exp. No. 13, 0·11, and in No. 14, 0·03 per cent. sulphur was obtained from the gaseous products. Mr. E. S. Breidenbaugh found in the same sample by Reichhardt's method* 0·36 per cent.

It is plain that the sulphuric acid of the gaseous products is obtained by this process under conditions highly favorable for its exact estimation. The solution from which it is precipitated contains no fixed salts and no nitric acid, the presence of which renders the barium sulphate difficult to purify, but only sulphuric and bromhydric acids. The only impurities that can attach to the barium sulphate are accordingly barium chloride and bromide. The former may be perfectly removed by warming the ignited precipitate with dilute chlorhydric acid, as Fresenius has conclusively shown in a late paper.† This method of purifying the barium sulphate has also the sanction of Bunsen.‡ It is not probable that barium bromide would resist treatment which removes the chloride. In the analyses given the writer added barium chloride slowly to the concentrated boiling solution, and after twelve hours or more decanted the supernatant liquid, boiled the precipitate with two or more portions of water and washed with hot water till sulphuric acid gave no turbidity in the washings. The precipitate washed rapidly, and an hour to an hour and a half generally sufficed for the largest ones. In purifying the larger precipitates, they were placed in a beaker with from 50 to 100 cc. water and a few drops acid. Any lumps were broken up by a rod, and the whole was boiled half an hour or more. The smaller precipitates were treated in the crucibles. In all cases the purifying was continued till sulphuric acid gave no reaction for barium salts in the last filtrate. The precipitates, weighing from 2 to 4 gr., lost by digesting with dilute chlorhydric acid from 0·007 to 0·020 gr.

To Professors Johnson and Allen I desire to return thanks for their assistance and for many valuable suggestions.

Sheffield Laboratory, July 1st, 1872.

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* Caldwell's Agricultural Qualitative and Quantitative Analysis, p. 246.

† Zeit. für Anal. Chem., Bd. ix, s. 52.

‡ Zeit. für Anal. Chem., Bd. x, s. 396.

## ART. XVII.—*On the Address before the American Association of Prof. T. Sterry Hunt;* by JAMES D. DANA. No. II.

THE aim of Professor Hunt in the latter section of his Address was apparently to show that all writers on metamorphism were deeply in error, except himself and a small circle of honored savants sufficient in number for a new School in that department of science. And in my reply it was my purpose, laudable, as I thought, to let him know that Delesse and Naumann were not to be depended on for the school; and, further, to show that the views of the outsiders were not altogether "contrary jargon," as he, in his intense love of truth (*i.e.*, *his* truth) had said, hoping by this last to quiet that vexation of spirit which had been excited by the alleged "sophistries." But in the reply to my criticisms (page 41 of this volume) Mr. Hunt still persists in denouncing multitudes of men for opinions they do not hold, and in claiming Delesse and Naumann as on his side. He throws out long statements against my eleven positions; yet, I have to say, without essentially weakening them. The multitudes do not need my further aid in their defense; and still it seems best once more to state the facts with regard to Professor Hunt's misreadings and misrepresentations. It is plain that for some reason he is yet unable to read rightly the opinions of others.

I will, therefore, set forth again those of my objections to which Prof. Hunt has replied, following the order nearly of his paper, and add such remarks as seem necessary.

Objection 1. *That Professor Hunt, while accepting the ordinary views on the origin of most pseudomorphs, rejects them with respect to many silicates, such as those consisting of serpentine, steatite, and pinite.*—I gave the reasons why crystals of serpentine and other similar pseudomorphs are not true crystals, mentioning facts that establish this, as I believe, beyond question. Mr. Hunt's only reply is: "Until we can watch the transmutation of one of these species into another, the argument from supposed intermediate forms is worth no more in the mineral than in the inorganic world." Thus chemical ignorance as to the how, in his use of scientific logic, is made to set aside all arguments as to the fact. The comparison he uses, if hard on Darwin, is not so on the crystallographer. The change of a crystal making it a pseudomorph is simply a chemical change without a change of form by means of hot or cold mineral solutions or vapors (agents that have been common in the course of the earth's history); and since very many such changes, as Mr. Hunt admits, have taken place among species not silicates, some of them yet unexplained, the above argument is poor support for the conclusion that they may not have occurred among silicates. In this country no investigator of pseudomorphs has shown any leaning toward Mr. Hunt's "more rational" view, and only one or two in Europe.

But, further, the gradations in the transmutation have been in many cases seen, if not watched, so that direct observation of this kind has satisfied almost everybody else. Naumann holds to the derivation of serpentine from chrysolite, and states that large crystals occur at Snarum that are all serpentine excepting a remaining kernel of chrysolite at the center. Mr. Hunt would say that this is a case of cotemporaneous crystallization of the two minerals, or of *envelopment.* But the last step occurs also in the same region, that is, crystals of pure serpentine, with no chrysolite left and therefore nothing to envelop, and yet not having the qualities of true crystals, inasmuch as the interior physical and optical characters do not correspond to the crystalline form; and this, as the final step in the process, Naumann and the "many others" would say makes it a well-clinched demonstration.

Objection 2. *That Mr. Hunt claims that Delesse sustains a "theory of envelopment," as a substitute for the ordinary "theory of pseudomorphism," and in this denies Delesse's own statements.**—From the example just mentioned it is plain what is here meant by *envelopment*—that it is mixture from cotemporaneous crystallization. A considerable part of Prof. Hunt's reply on the above point is an endeavor to make what Delesse says on envelopment in crystals a substitute for what he says on pseudomorphism; when the truth is reached by taking Delesse's word for it, that his chapter on envelopment is introductory to the other,† a

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* In my former paper I used, instead of the word *sustains*. the expression *is the author of*. Mr. Hunt claims rightly that Scheerer first brought out the idea of a kind of envelopment. But, as is obvious above, the question of authorship has nothing to do with my argument. Delesse was the first to present, in a detailed and systematic form, the facts connected with envelopment in crystals, and this word envelopment, as thus employed, is from him. Moreover, his idea of "envelopment" did not embrace Scheerer's principle, which was that of *isomorphism*. As Mr. Hunt has it, in this Journal, II, xvi, 217, Scheerer had in view a simultaneous crystallization of two *isomorphous* species, as, for instance, a hydrous and anhydrous silicate (iolite and fahlunite in the same crystal being an example). But Delesse's envelopment is simple mixture; he has no allusion in his chapter on envelopment to any *isomorphism* in the associated minerals. Mr. Hunt's sentence in his Address (p. 47), claiming that Delesse's view in his work on Pseudomorphism "is identical with the view suggested by Scheerer," is therefore at fault. Delesse, later in the volume, mentions Scheerer's principle as a possible case under his "envelopment," as follows: "If, as Scheerer has remarked, water acts as a base in silicates, then anhydrous and hydrous silicates may crystallize together and be isomorphous:" and then follows the single example of the association of hornblende and diallage in euphotide.

† Delesse says, at the close of his chapter on envelopment, "Ce préambule sur l'enveloppement des minéraux était nécessaire pour l'intelligence du pseudomorphisme, qui va maintenant nous occuper."

showing of *what is not* a pseudomorph preparatory to explaining *what is*; and observing, also, that this second part, the main part of the work, is treated, with the exception of a few examples unessential to the point in dispute, just as is done by Haidinger and all other writers on pseudomorphs, and contains a table, many pages long, of true pseudomorphs, in which are those of *serpentine*, *gieseckite* (var. of pinite), *steatite* or *talc*, *sepiolite* and *chlorite*, besides other silicates. There is no fog in Delesse's statements on this point. Mr. Hunt cites some seemingly opposing sentences from Delesse: but these relate to the exceptions, cases that for the most part are generally admitted to be doubtful; they do not touch the species abovementioned, those with regard to which Mr. Hunt would be glad to gather support from Delesse.

Prof. Hunt says that Delesse's views underwent a change about 1861, as appears in his work on metamorphism then published, in which Delesse "adopts" Mr. HUNT'S VIEW, that *beds* of serpentine have been formed from the alteration of chemically deposited beds of a hydrous magnesian silicate related to sepiolite (meerschaum). But the evidence is positive that, while Delesse accepted this hypothesis for *beds* of serpentine, he did not change his views on serpentine pseudomorphs. For in the successive numbers of his "Revue de Géologie," published by him and de Lapparent between 1860 and the present time, he sets forth the very same principles that are the basis of his work on Pseudomorphism (1859), and adds to the number of cited facts under both the heads *Envelopment* and *Pseudomorphism.*

In the number of the Review published in 1861, the very year his paper on metamorphism appeared, after citing a long list of cases of envelopment from an article by Söchting, he mentions cases of pseudomorphs of *steatite* after pectolite, analcite, natrolite and barite. In that issued in 1865, he commences (p. 169) his chapter on pseudomorphs with the following paragraph: "Lorsqu'une substance conserve sa forme primitive, bien qu'elle soit plus ou moins modifiée dans sa composition et même entièrement remplacée par une autre substance, il se produit ce que l'on peut appeler un pseudomorphisme. Ce phénomène a souvent lieu sur une grande échelle, comme l'a surtout fait remarquer M. G. Bischof, et par conséquent son étude est importante pour la géologie." Delesse here goes so far as to say (and this in 1865, it should be noted) that *pseudomorphic changes have often taken place on a grand scale, as has been especially remarked by G. Bischof, and consequently its study is important to geology.* He then cites a table of pseudomorphs, in which occur three of *serpentine*, one after actinolite, another after asbestus, and a third after bronzite; and this he does without a word of protest or objection, just as he did in his treatise of

1859. Again, in the Review published in 1872, after stating the fact of the occurrence of magnetite in mica, ascertained by Prof. Brush, as an example of *envelopment*, he next mentions a pseudomorph of *talc* after enstatite. Thus he declares his belief in the old views to the present year, excepting, as I have said, only a few species not bearing on the question here under consideration. Delesse, in adopting in 1861 the hypothesis with regard to *beds* of serpentine just stated, did not assume or imply that serpentine pseudomorphs or crystals were so made. He was aware that the method was wholly inapplicable to pseudomorphs, knowing well that a serpentine pseudomorph of chrysolite had been made by the alteration, not of a hydrous magnesian silicate, but of a crystal of chrysolite; that a serpentine pseudomorph of hornblende had been made by the alteration or removal of a crystal of hornblende; and the same in other such cases of "epigenic" pseudomorphism. For this is the view he presents.

Mr. Hunt attempts to explain Delesse's combination of opinions by "hazarding the conjecture" that Delesse wrote his views on envelopment and metamorphism "while he still inclined to the views of the opposite school." Thus, Delesse's direct and consistent account of his views is set aside on the ground of stupidity, or his not knowing clearly what he believed—a damning apology for Delesse, Engineer-in-Chief of the Mines of France, if it were needed: but, not needed, most damaging to the argument of his apologist.*

OBJECTION 3. *That Prof. Hunt makes Naumann sustain the theory of envelopment, when, in fact, this veteran crystallographer and mineralogist presents the ordinary views on pseudomorphism in the successive editions of his Mineralogy down to the last of* 1871. In reply to this Mr. Hunt adds one more sentence to the citation in his Address from Naumann's published letter to Delesse. But Naumann's letter related only to Delesse's ideas on envelopment, and is utterly misused by Prof. Hunt. I repeat from my former article that Naumann's chapter on Pseudomorphism contains not a word on envelopment, while it does present the old views; and his work is full of examples according therewith. Even fahlunite and some related species are admitted to be (p. 455, note) products of the alteration of iolite,† contrary to Scheerer and Hunt.

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* In my review, I did not say, as Mr. Hunt implies, that Delesse *does not in any way countenance the views* of Mr. Hunt, for I remarked that he did agree with him with regard to the origin of *beds* of serpentine, and with respect to the envelopment nature of a few of the kinds of pseudomorphs; but I did say that with regard to pseudomorphs of serpentine and all the species under dispute, as well as most other kinds, and with regard to the use to be made of the facts under envelopment, Delesse holds directly opposite views to those of Mr. Hunt.

† Naumann, in the note here referred to in his Mineralogy of 1871, alludes to Haidinger's "excellent article" on the relations of fahlunite, chlorophyllite, pinite,

Mr. Hunt protests against expressions cited by him from Naumann and Delesse being "set aside because *traces* of the doctrine of epigenic pseudomorphism still hold a place in the last edition of Naumann's Mineralogy, or in the 'Revue de Géologie,' of which Delesse is one of the authors." *Traces!* when a systematic statement of facts on pseudomorphism essentially after the old views is the object of each! TRACES!!!

Delesse and Naumann might well be excused for some vexation of spirit after such "sophistries" personal to them, and, probably, if they were to speak out, they would show their vexation without the use of poetry.

OBJECTION 4. *That Prof. Hunt grossly misrepresented the views of nearly every writer on pseudomorphism in saying that the doctrine of Gustaf Rose, Haidinger, Blum, Volger, Rammelsberg, Dana, Bischof, and many others, leads them to maintain the possibility of converting almost any silicate into any other; and adding, in the same paragraph, that "in this way we are led from gneiss or granite to limestone, from limestone to dolomite, and from dolomite to serpentine, or more directly from granite, granulite or diorite to serpentine at once, without passing through the intermediate stages of limestone and dolomite."*

Prof. Hunt seems to think that he meets the objection in saying (page 50) that—

HAIDINGER and others have held to the conversion of limestone to dolomite;

G. ROSE, BLUM and the writer, to that of dolomite and some other rocks to serpentine, or to talcose, steatitic or chlorite schist;

RAMMELSBERG ———? [nothing is mentioned];

BLUM, again, to that of limestone to granite or gneiss, when this author has nothing of the kind in his works, and the nearest to it is the fact cited by Bischof that feldspar occurs as a pseudomorph after crystals of calcite;

BISCHOF, MÜLLER, and VOLGER, to that of various rocks into others by extravagant methods; and

DANA, again, ———! [see below].

But this gathering of objections from the opinions of a variety of individuals, and then charging the whole, with the help of a few lines of poetry (see Address, p. 42), on all collectively, while it may be a smart thing to do, is not the best course to give success to the truth. With scarcely an exception, all writers on the subject under consideration, Naumann and Delesse included, have a right to feel badly at being so summarily knocked over in ten-pin style.

etc., to iolite [showing that they are *altered* iolite], and then observes that these minerals are to be marked as independent species only "so far as they correspond to *definite stages or phases in the decomposition of iolite*." There is no mistaking Naumann's opinion.

But the most extraordinary feat is Mr. Hunt's making out that the writer has virtually sustained the view of the *metamorphosis of granite or gneiss to limestone* (p. 50), when, as I said before, it is an idea that never entered my head until the reading of Mr. Hunt's caricature of the subject. The proof which he gives is remarkable. In the first place, he says that my Mineralogy contains the fact that calcite is sometimes found pseudomorphous after quartz; and, in another place, the fact that calcite is found pseudomorphous after feldspar. Hence the conclusion, *granite or gneiss to limestone.* Q. E. D.

Now, if the facts respecting the pseudomorphs *were facts*, it would still require great constructive powers to make out from the statements the conclusion that I ever held to the "metamorphosis of granite or gneiss into limestone." But, as to the facts: (1) The mineralogy *does not* mention any case of the pseudomorphism of calcite after quartz; and (2) the pseudomorphs of calcite after feldspar are spoken of as examples not of an *alteration* of the feldspar, but of its removal, and the substitution of calcite (4th edit., p. 249, and also 5th edit., p. 361). Now, by this substitution process, the above-mentioned metamorphosis would consist (*supposing* fact No. 1 to be a fact, and that mica crystals may be similarly changed to calcite, which Mr. Hunt omits to include) in a removal of all the materials of the granite by a process of solution, and the contemporaneous or subsequent substitution of calcite!

All will admit that the use of facts and not-facts exhibited in the above charge is most extraordinary; and can judge from it, and from other like cases stated, of Mr. Hunt's ability to appreciate, or do justice to, the views of others.

In accordance with the opinions on the origin of serpentine rocks which I hold, in common with Gustaf Rose and others, we learn from Prof. C. U. Shepard (see this Journal, iii, 237, 1872) that specimens of serpentine from the vicinity of Havana (Cuba) are plainly, as he says, pseudomorphous after chrysolite, amphibolite, augite, and titanite, and that these pseudomorphs so abound in the matrix, that they constitute the largest portion of the mass, and may be easily separated by a blow of the hammer.* Again, Mr. T. D. Rand states (this volume, p. 71, from the Proceedings of the Academy of Natural Sciences of Philadelphia, 1871, 302) that a serpentine and steatite rock on Mill Creek, Pa., near the line between Philadelphia and Montgomery counties, contains crystals partly cruciform which have precisely the form and angles of staurolite (as ascertained by running lead into the cavity left by one of them, and thus obtaining a cast), and which yet consist of serpentine;

* It is to be noted that serpentine pseudomorphs are sometimes pseudomorphs *by substitution*, as well as *by alteration*. Either method is a result of "epigenic" change, since it is produced by the action of external chemical agents.

and Mr. Rand adds that "the whole aspect of this curious formation suggests a rock originally containing crystals, but metamorphosed almost beyond recognition." Remembering what rocks contain crystals of staurolite, that they are usually mica schist, or clay slate, we learn here some of the probabilities as to the kinds of rock that may have undergone this change to steatite and serpentine. The argument with regard to the magnesian alterations is not from simple crystals alone, but from these and the rock combined.

In view of such facts, the writer still holds, as in 1845, that—

* * * "The same causes that have originated the steatitic scapolites, occasionally picked out of the rocks, have given magnesia to whole rock-formations, and altered throughout their physical and chemical characters. If it be true that the crystals of serpentine are pseudomorphous crystals, altered from chrysolite, it is also true, as Breithaupt has suggested, that the beds of serpentine containing them are likewise altered; though often covering square leagues in extent, and common in most primary formations. The beds of steatite, the still more extensive talcose formations, contain everywhere evidence of the same agents."—*This Journ.*, xlviii, 92, 1845.

Besides this paragraph, expressive of my views, Mr. Hunt cites also another of the same purport from my Mineralogy of 1854, and in this, also, I see little to modify. It is as follows: that—

"The various examples of pseudomorphism should be understood as cases not simply of alteration of crystals, but in many instances of changes in beds of rock. [Delesse admits this; see p. 99.] Thus all *serpentine*, whether in mountain-masses, or the simple crystal, has been formed through a process of pseudomorphism, or in more general language, of metamorphism; the same is true of *other magnesian* rocks, as steatitic, talcose or chloritic slates. Thus the subject of metamorphism, as it bears on all crystalline rocks, and of pseudomorphism, are but branches of one system of phenomena."—*Min.*, 4*th edit.*, i, 226.

The larger part of the kinds of alteration or metamorphism made out against authors by Prof. Hunt, on pages 50, 51 of his article, are of this *magnesian* class, the results being serpentine, or talcose, steatitic or chlorite schist. It is to be remembered that this class is but one among several that have been made by metamorphism. If we use, as above, the argument from hydrous magnesian pseudomorphs to hydrous magnesian rocks, we are not chargeable with applying a like argument to the origin of calcite from gneiss or granite, or the reverse, until we so apply it. There is no like basis in pseudomorphism for such conclusions. This is made manifest, for one of them, in the explanation of Mr. Hunt's "extraordinary feat" on the preceding page; and the other is equally preposterous.

Objection 5. *That Prof. Hunt's Address of* 1871 *misrepresents my views in attributing to me the doctrine that "metamorphism is pseudomorphism on a broad scale," without alluding to the views I actually now hold as presented in the chapter on Metamorphism in* *my Manual of Geology published in* 1863.—Mr. Hunt's reply to this is simply that I once held the view and have never formally retracted it—as if presenting other views in a formal chapter on the subject were not a sufficient retraction. As to my own expression of the doctrine that "metamorphism is pseudomorphism on a broad scale," he brings out a fact I had overlooked when writing my former article. I examined the Mineralogy, and all my papers in this Journal, in search of the line above cited, and failed to find it because of its occurrence only in a short book notice. I did not deny having used it, though ignorant where or when, but only asserted that it was not in the Mineralogy. The statement in my article as to the views contained in the Mineralogy (4th ed.) is strictly correct. My general expressions in that work are strong; but I mention as examples, under those views, no rocks except serpentine and other magnesian rocks; and to these, as I have said, I still apply it. See, for my views in 1854, the sentence on the preceding page, cited from it. Moreover I state, in the same chapter (p. 336), that *few will follow Bischof in all his methods of rock-making.*

Objection 6. *That Prof. Hunt points out the existence of a Green Mountain series of rocks, and a White Mountain series, basing his deductions largely on lithological evidence, without any sufficient stratigraphical evidence, and without properly defining the limits of the two regions.*—Mr. Hunt's reply to these objections are confined to three points. (1.) In opposition to my remark "that there are gneisses, mica schist and chloritic and talcoid schists in the Taconic series," he says "that Emmons, the author of the Taconic system, expressly excluded therefrom the crystalline rocks." This exclusion is an easy feat for a speculator with pen in hand, like many closet feats; but it is more than herculean in actual fact, since the very Taconic mountains themselves, that is, the very rocks called Taconic by Emmons, are partly gneiss, gneissoid mica schist, and chloritic talcoid schist, as well as talcoid schist; and these rocks are so involved together that speculation will never bring them into that kind of order which Mr. Hunt's "notions" require.

(2.) To my enquiry whether any one has *proved* by careful observation that crystals of staurolite, cyanite or andalusite are restricted to rocks of a certain geological period, Mr. Hunt answers that "it has not yet been proved that they belong to any *later* period than the one already indicated" (the Pre-cambrian); and that "it is only by bringing together observations, as I have done, that we can ever hope to determine the geological value of these mineral fossils." Now the fact is that those same Taconic rocks, unquestionably of the Taconic system according to Emmons himself, and, therefore, Hunt attesting, of

Lower Silurian age, contain in some places staurolite crystals. Percival first noticed the fact, and states this even of the rocks of Mt. Washington, the main part of the Taconic range. He speaks of the rock of "Taconic mountain" as fine-grained micaceous or talco-micaceous schist, containing garnet and *staurolite;* and adds, "sometimes it is greenish and subchloritic, with seams and patches of compact green chlorite, and yet accompanied with the same minerals [garnets and staurolites]. This is particularly the case in the south and northeast part of Taconic mountain." Hence staurolites, and chlorite also, occur in rocks admitted to be Silurian.

(3.) Mr. Hunt denies that he makes, in his Address, "the crystalline schists of the White Mountains a newer series than the Green Mountain rocks."—I had read on pages 29 and 33 of the Address approving announcements that Macfarlane had made the crystalline rocks of the Green Mountains *Huronian;* and then, on page 34 of the Address, the statement that the White Mountain series is largely developed in Newfoundland, and that this fact had led him (Mr. Hunt) to propose for it [the year before] the name of the *Terranovan* System. At this point in the Address there is a reference to this Journal of the preceding year, vol. 1, p. 87, 1870; and consequently by referring back to this article by Mr. Hunt, I found this Terranovan defined, Mr. Hunt saying that, according to Mr. Murray, the series comprises "several thousand feet of strata, including soft bluish-gray mica slates and micaceous limestones belonging to the *Potsdam* group, besides a great mass of whitish granitoid mica slates whose relation to the Potsdam is still uncertain." As the Huronian is older than the Potsdam, and this equivalency of the Terranovan is not corrected in the Address, I thought I had reason for supposing that Mr. Hunt made the White Mountain series the newer. I acknowledge I prefer the view he now presents, since the less definite the statement the better as long as we have no sufficient facts for a conclusion.

## ART. XVIII.—*Reply to a "Note on a question of Priority;"** by JAMES HALL.

IN the April number of this Journal there is published an article with the above title, in which the author questions the fact of publication of a small pamphlet entitled "Notes on some new or imperfectly known forms among the Brachiopoda." I perhaps owe to myself and to the scientific public a few words in reply.

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*By E. Billings, who has treated the same question, essentially in the same style and manner, in two or more articles in the Canadian Naturalist.

The pamphlet referred to was printed in March, 1871, and a number between twenty-five and thirty copies delivered to me at that time. The type was left standing in order to print a larger number, to be accompanied by a plate of figures then in progress, with descriptions of the same. Of these copies, the greater part were distributed in the United States soon after publication. Copies were sent to the Geological Society of London, to Mr. Davidson, Mr. A. C. Ramsay, M. Barrande, Dr. Lindström, Dr. Geinitz, Prof. DeKoninck, Dr. F. Rœmer, Edward Desor, Dr. A. von Volborth, and the Imperial Society of Naturalists of Moscow. These, with one exception, were sent in packages with other publications, through the Smithsonian Institution, and are marked in my list as having been forwarded from Albany on the 7th of April, 1871. The pamphlet is noticed in the Jahrbuch for 1871, p. 989.

On the 7th of April, 1871, the printing establishment of Weed, Parsons & Co. was destroyed by fire, together with the 23d Report on the State Museum (printed to nearly 200 pages), the lithographic stones, and everything else pertaining to that work. In the confusion which followed, and with the necessity on the part of the State printer to furnish certain documents as soon as possible, no attention was given to the State Museum Report for several months. Had there existed in my mind the least doubt about publication, I should naturally have procured an additional number of copies; for this work could easily have been done at any printing office. It has usually been my practice to distribute at least one hundred copies of publications made in advance of the regular reports; and this would have been done in the present instance within a month after the first publication, together with the plate of figures, and description, but for the disastrous fire referred to.

These are the facts of the case; the scientific public will decide the question of publication.* And here I might close: but there are a few points in Mr. Billings' article which require attention.

From the tenor of Mr. Billings' statements in this Journal and especially in the Naturalist, any reader would suppose that I had borrowed specimens from the Canadian Geological Survey on which to found my descriptions, or conclusions, concerning the genera there published as RHYNOBOLUS and DINOBOLUS, and then endeavored to keep him in ignorance of what I had done. This would certainly have been an absurdity, and moreover it is not true. The only specimens borrowed of the Survey, having the remotest relation to RHYNOBOLUS, were of *Tri-*

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* If the fact of being on sale with booksellers is necessary for publication, the question could certainly be raised regarding all the State Museum Reports; for the State of New York has never authorized their sale.

*merella.* I wished to compare authentic specimens of the latter with *Dinobolus*, which under the name of *Obolus Conradi* had been stated by Mr. Dall to be a true *Trimerella.* The idea of designedly keeping Mr. Billings in ignorance of what I had done would have been simply silly and purposeless.

The question regarding these oboloid forms had occupied my attention for a long time; and in 1862, I wrote to Mr. Davidson my views of *O. Conradi*,* sending a description and figures. Thus this was no new idea of mine; but the progress of my work in 1871 required some action on my part in order to prepare the supplementary plates of vol. iv, Pal. N. Y., and these were among the things to be first done. *Obolus Canadensis* I did desire to see, for I had known since 1854 that it was a new and distinct genus; and Mr. Selwyn did say that Mr. Billings was at work at *O. Canadensis*, but did not mention any Galt specimens or species. Mr. Billings says that his genus *Obolellina* "*is intended to include at least one of the forms described*" by him *as* "*Obolus Canadensis.*" It may include also RHYNOBOLUS, but I think that has not yet been shown by Mr. Billings' figures.

As an explanation of applying on "two occasions," I may say that I understood Mr. Selwyn's reply to my first letter to be a refusal, and the matter was of course dropped. Subsequently, Dr. T. Sterry Hunt, authorized by Mr. Selwyn, gave an explanation which induced me to renew the request. I was taking no advantage of Mr. Billings in any way, for neither himself nor Mr. Selwyn had indicated his intention in regard to Galt specimens, and those which I used had been in my possession since 1848.

As to the compact or agreement about describing New York or Canadian fossils, referred to and written about by Mr. Billings, I can only say that I never heard of it before now. I have alway had in my collections undescribed species of Canadian fossils, which I have refrained from describing from a natural sense of propriety. If I am not mistaken, Mr. Billings has derived much material from New York, by collectors sent expressly for that purpose, and I have no doubt has made good use of it; but I have never thought of complaining; and I have not entered into petty contrivances or insinuations to prevent fossils going into his hands. If Mr. Billings' published statements and private letters agree in regard to this matter, it is all that I can require.

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* In my letter to Mr. Davidson, of date 31st October, 1862, I wrote—"I enclose you drawings of what I have proposed as a new genus of Brachiopoda. In some respects it is like OBOLUS, but is a large calcareous shell, in my opinion of quite a new type. I had originally communicated the description in my Wisconsin Report, but afterward withdrew it. Please give me your opinion of it. * * * I propose the name Conradia for this fossil."

In my letter to Mr. Selwyn, of the 10th of April, 1871, alluding to my work, I said, "The question of the Linguloid shells, OBOLUS and TRIMERELLA, was one requiring early determination;" and it was for this reason that I had desired to see the Canadian forms. I was certainly under the impression that I had previously given Mr. Selwyn full information of what I proposed to do; but if otherwise, this letter of the 10th of April was sufficient; and if after that no pamphlet was received, it seems a little remarkable that Mr. Billings should wait till the 30th of January following before making any further inquiries about it.

On the 23d of February, 1872, during my absence from Albany, a letter was received from Mr. Davidson, of date February 8th, in which my attention was called for the first time to this question of publication. I replied, stating the facts as I have done here. In a subsequent letter, Mr. Davidson discussed more fully the refusal of Mr. Billings to admit publication, and, as I understood the language, had proposed to refer the matter to several scientific gentlemen in England. I wrote quite agreeing to this, and, while feeling no doubt of the publication of my pamphlet, proposed a plan in which I consented to drop the name RHYNOBOLUS, thus hoping to prevent a controversy, and saving the *amour propre* of Mr. Billings. In the mean time Mr. Billings published the article in this Journal, and I wrote immediately withdrawing the proposition.

The public have here the facts. I have not been aware of any "unfortunate collision," nor of any cause for the succession of statements in the last page and a half of Mr. Billings' article against me, that "it is not my fault that this difficulty has arisen," etc.

It has unfortunately happened, in nearly all cases where I have proposed new genera during the last ten or fifteen years, that I have, according to Mr. Billings' expressed opinions, infringed upon his rights, or violated some rule of scientific procedure.*

I fully admit that the party at fault in this or any other case should be the sufferer. Mr. Billings has inaugurated and thus far managed both sides of what he denominates "this controversy," with his usual tact and adroitness. I have said nothing, while he has published I believe three or four articles on the subject. I have entered into no controversy, and hope to be saved from one. It will not distress me if my name of RHYNOBOLUS should not be adopted. Unquestionably the pamphlet should have been reprinted at once after the fire; but in such a condition of things as then existed, every one is naturally absorbed in *what appears to be the present duty*, and may easily for-

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* For example, TRIPLESIA, RENSSELAERIA, MERISTELLA, STROPHODONTA, etc.

get some things which afterward may prove to have been of more imperative importance. I think this is the experience of the world, judging from the aphorisms ancient and modern.

I can say, however, in all sincerity, that had Mr. Selwyn or Mr. Billings at any time made me a civil statement of the facts, with a request to withdraw the name RHYNOBOLUS, I would unhesitatingly have done it, so far as in my power; and it would have given me great pleasure to show my good will toward every member of a corps with whose former chief I have been for more than thirty years in uninterrupted friendly and harmonious intercourse; and always in more or less intimate relations of friendship with every member of the staff, except Mr. Billings, who has chosen for himself an attitude of hostility without any reason or cause of provocation on my part.

With regard to the accusations and insinuations of dishonest purposes and practices, to which I at first felt inclined to reply, I shall say nothing at this time.

Albany, N. Y., May, 1872.

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## ART. XIX.—*On the Corundum region of North Carolina and Georgia, with descriptions of two gigantic crystals of that species;* by CHARLES UPHAM SHEPARD, Sr., Prof. of Natural History in Amherst College, Mass.

CORUNDUM has been recognized for above thirty years at several of the gold washings in the mountainous counties of North Carolina and Georgia, though rarely occurring in masses larger than would be called a coarse gravel. Upward of twenty years ago I received from Mr. Plant, banker at Macon, Ga., an hexagonal prism of this mineral of a ruby-red color, measuring one and a quarter inches in diameter by $\frac{3}{4}$ of an inch in height, said to have come from a gold mine in Habersham Co., Ga. About the same period, I was indebted to the Hon. T. Clingman of Asheville, N. Car., for several pounds weight of a coarse sapphire, derived from a crystal originally ten or fifteen pounds weight, that had been picked up at the base of a mountain on the French Broad river in Madison Co., N. Car. No farther discoveries of the kind appear to have attracted attention until the last two or three years. Within this period, however, under the stimulus of discovering an improved description of emery, many new localities of corundum have been brought to light in this region, of two or three of which I propose to give some account, derived from the examination of numerous specimens, and from information afforded by Rev. C. D. Smith and Col. C. W. Jenks of Franklin, Macon county,

N. Car., two gentlemen to whom we are chiefly indebted for the developments thus far made.

The corundum localities are already known to occupy a stretch of country at least 170 miles long, with a breadth of about ten miles. As the region is little inhabited and very mountainous, it is probable that the corundum zone, as it has been called, will hereafter be much extended. It is situated in a sub-alpine country, partly within the northeastern corner of Georgia, and extending thence, in the direction of the crest of the Blue Ridge, into several contiguous counties of North Carolina. Beginning for example, in the northeastern corner of Jackson Co. (N. Car.), Mr. Smith sketches it, as running in a southwesterly course across Macon Co., where it strikes the Georgia State line, its general direction coinciding with the trend of the Blue Ridge until it reaches the head of Tennessee river, when it suddenly ceases on encountering the Nantegalee mountain (a spur of the Blue Ridge here running due north), to reappear 10 miles to the northwest on Buck creek; whence it pursues its original course of N.E. and S.W. across the Chunckygal mountains, where it again enters the Blue Ridge, and probably continues through several of the upper counties of Georgia, as Union, Habersham, Lumpkin and Hall. Thus far, the corundum is known to occur only in a single formation, which may be designated as chrysolitic rock; though from its color and some other peculiarities, it has often been confounded with serpentine. Strictly speaking, as will more fully appear farther on, it is not the true chrysolite, though containing this species to some extent, in an intermingled or disseminated condition.

This chrysolitic rock does not show itself as a continuous, uninterrupted belt or formation, throughout the tract above indicated; but exists in lenticular patches or beds, accompanied more or less by a hornblendic layer on one side (the southeast), and possibly on both sides; but the prevailing rock of the region—that enclosing the chrysolitic as well as the syenitic rock—is a gneiss, somewhat characterized by containing garnet and staurolite, as well as occasional deposits of iron and copper pyrites. The interruptions in the linear continuity of the chrysolitic rock vary in extent, from one to 15 miles; though these may in some cases be reduced, after a more careful survey of the country, The transverse diameter of the chrysolitic beds varies from half a mile to a mile, or even more in some places; but narrows down to 40 feet and less, as we approach the running out of the discs.

The principal exposure of the corundum has been effected at what is known as the Culsagee mine, situated in the township of Elegée (sometimes written Elijay) situate 8 miles S.E. from

Franklin Court-house, in Macon Co. This is the center of operations of the American Corundum Co., whose works are superintended by Col. C. W. Jenks. The chief excavations have been made on the northern slope of a mountain, at an elevation of about 2,700 feet above tide water. The strata here developed dip to the N.W. at an angle of about 45°, and exhibit the following order of formations, commencing on the N.W. side of the opening:—1, chrysolitic rock somewhat mixed with anthophyllite; 2, a layer of micaceous rock; 3, a seam of chalcedony; 4, a stratum of chloritic rock (ripidolite); 5, the same, through which the corundum is irregularly diffused, sometimes in narrow veins or widening out to several feet. The chloritic layers 4 and 5 do not possess the character of a true chlorite-slate, but constitute rather a coarse rock, in which the chlorite (ripidolite) is mostly in large crystals and foliated masses, segregated without order, much as mica is in large-grained granites; but what is here chiefly remarkable is, the entire absence of quartz. Narrow veins, containing besides the chlorite and corundum, a dark blackish green spinel (mostly massive), more or less mingled with black tourmaline, traverse layers 4 and 5, in various directions. The workings have been carried down to a depth of above 50 feet, and extend over a considerable space, without thus far establishing the existence of a concentrated stratum or vein for the corundum, similar to that at the Chester (Mass.) emery mine. I have not been informed as to the underlying rock of the above mentioned strata, but suppose it to be chrysolitic, which I am led to infer is the bounding formation of the corundum series, just as gneiss rock is of the chrysolite and syenite themselves. I am not accurately informed either respecting the aggregate thickness of the layers above mentioned, but infer that they are from 12 to 20 feet. As to their linear prolongation, nothing is yet established; but it is quite probable that it is not very considerable in proportion to the thickness of the beds; and it is most likely that the corundum-bearing layers themselves will be found to constitute a series of ovoidal or discoidal masses, often repeated over the chrysolic tract, inasmuch as the under soil and the beds of the brooks largely afford traces of a corundum sand and gravel, that must have originated from outcropings similar to that of the Culsagee mine.

I am informed by Col. Jenks that the corundum occurs under very similar circumstances at the Rabun Co. (Ga.) locality, situated on the road from Walhalla, S. Car., to Franklin, and 10 or 15 miles to the S.E. of that above described. At the Cullakenee (sometimes written Cullakenih) mine, on Buck creek, in Clay Co., and about 15 miles to the S.W. of the Culsagee, the corundum, though contained in the same chrysolitic rock, is nevertheless attended by quite a different series of minerals. The outcrop extends over three hundred acres. Arfvedsonite, zoisite, albite and margarite are here found as its most frequent attendants. The corundum is either white or gray like common feldspar, or else a delicate and often deep ruby-red color. No tourmaline or spinel have thus far been noticed, though occasional detached specimens of picrolite in long and rather coarse fibres are here found.

After this outline of the leading geological features of the corundum formation, we proceed to speak of it mineralogically; and first, it will be appropriate to treat of the constituents of the chrysolitic rock. As a whole, it has nothing but color to assimilate it to serpentine. Indeed it more resembles a certain fine granular variety of epidote,—its color often bordering on pistachio-green. Even where most exposed to the weather, its hardness =5·0 to 5·5; and in no instance, so far as I have seen, does it emit, on being moistened, the peculiar odor of serpentine. Where a partial decomposition has taken place, we only see a thin crust or coating of a ferruginous powder on the surface. Nothing soft or talcy appears, or any change indicating a metamorphosis to serpentine. Though usually quite homogeneous, it still contains in places considerable grains of true chrysolite, having the hardness and other properties of the unchanged species. It also embraces occasionally broad bladed crystals of that variety of anthophyllite known as gedrite; and also has diffused through its mass, very generally, minute octahedral crystals of chromite. Almost the entire aggregate, however, consists of what some would denominate an altered chrysolite, while others again may prefer to regard it as having been originally formed as now found. It has the following characters: Color yellowish olive-green; structure fine granular to compact; luster glimmering, vitreous. H.=5·5 to 6·0. Gr.=3·04 to 3·06. When heated to redness in powder, it turns pale cinnamon-red. Infusible. Easily attacked by hydrochloric acid, with which it forms a stiff semi-transparent jelly. Composition:

| | |
|---|---|
| Silica, | 41·49 |
| Protoxide of iron, | 8·62 |
| Magnesia, | 44·00 |
| Water, | 5·69 |
| | 99·80 |

with traces of the oxides of chrome and nickel. The foregoing characters obviously place this abundant mineral under the species villarsite.

The mineral next in importance is the green chloritic one already mentioned as the immediate gangue of the corundum.

It is that variety of chlorite properly called ripidolite. Much of it, even to a depth of 50 feet at the Culsagee mine, assumes a light pinchbeck brown color, constituting that modification of vermiculite (itself a hydrated ripidolite), called jefferisite. This brown mineral is in broad, highly crystalline foliæ and crystals, often attached to and forming portions of unaltered ripidolite, in which cases they are softer and more easily cleavable than the latter mineral. From this condition, they pass insensibly to a pulverulent, scaly, clay-like aggregate, much resembling the original vermiculite of Millbury, Mass., except in possessing a lighter color. The ripidolite has a blackish grass-green color, closely resembling that of the corundophilite, but unlike the latter, it emits on being breathed upon the peculiar chloritic odor; and before the blowpipe exfoliates, turns white and fuses on the edges into a grayish white glass; whereas corundophilite does not exfoliate, but turns black and melts on the edges into a black magnetic glass. The vermiculite (both varieties, the broad foliated and the fine pulverulent) exhibits on heating the usual exfoliation of the species. The soft and slightly coherent condition of the vermiculite where it happens to prevail in the workings, renders the separation of the corundum, especially that of the crystals, easy; whereas the ripidolite, when by itself, forms rather a tough gangue rock. Occasionally narrow seams of blue and white halloysite traverse the masses of vermiculite, among which also occur greenish-gray crystals of a softened hydrated arfvedsonite, a very ambiguous mineral, but recognized by its crystalline structure and easy fusibility before the blowpipe. Into this aggregate small scales of white margarite also enter.

The spinel appears to be confined to narrow irregular seams of a more closely grained ripidolite, mixed also with black tourmaline and corundum, which seams traverse irregularly the coarser ripidolite rock. Its color when massive is almost black, though its powder is blackish-green. Before the blowpipe, it gives the reaction of chromium. It possesses the peculiarity when in crystals (octahedra with truncated edges) of being thinly coated or varnished as it were, with a silky, pearl-gray envelope,—giving them so much the aspect of the altered zircons from Henderson Co., N. C., as to have caused them at first to be referred to that species. This coating has about the hardness of steatite or agalmatolite, and probably also a composition approaching one or the other of these substances.

Quite different appear to be the minerals associated with the corundum of the Cullakenee mine in Clay Co. Indeed the variety of corundum differs considerably from that of the region generally. It is less perfectly crystallized, and has a delicate rose-red or pink as its prevailing color. The blue tints are wanting, so far as the specimens thus far submitted show; but a grayish-white, like that of common feldspar, is the most common shade. The most characteristic associated species would appear to be arfvedsonite of a grayish, brownish black color, often in crystals and coarse fibres interpenetrating the corundum. It also occurs in short crystals and even granular, of a rich grass-green color, coating or including small crystals and laminæ of the ruby,—constituting a rock of much beauty. This green or chromiferous arfvedsonite much resembles the most highly colored variety of omphacite, or eclogite with garnet from Hof Bavaria, but is quickly distinguished therefrom by its easy fusibility, attended with the soda colored flame so characteristic of arfvedsonite, in which peculiarity it precisely agrees with the brownish black and more perfectly crystallized variety. Instead of the disseminated garnet in the omphacite, we have the beautiful grains of ruby in the present aggregate. Zoisite is the next most characteristic mineral of this locality. It occurs in pretty distinct crystals and columnar masses of a pure white or slightly grayish white color, sometimes semi-transparent. The more massive variety is so intermingled with the corundum as often to be mistaken for feldspar. Albite is a third species that rarely enters into the composition of the corundum gangue. Margarite is wanting in the variety found in the green arfvedsonite, though a very deep sky-blue kyanite occasionally traverses the green aggregate in thin veins. It should be mentioned that this peculiar gangue of the red corundum shows a decided stratification like certain varieties of gneiss, that also embraces granular epidote, such as occurs at Grace mountain, in Warwick, Mass.

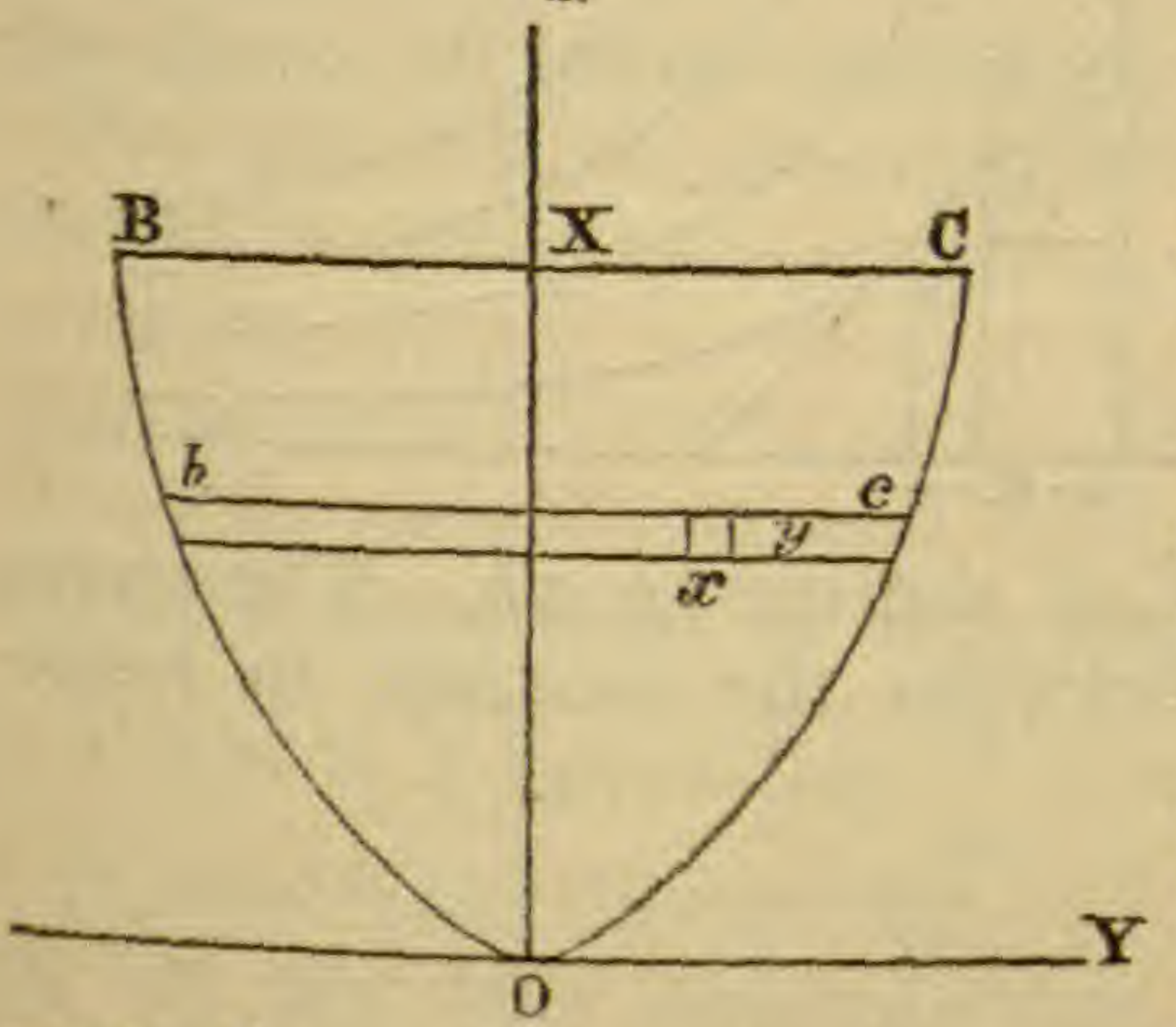
The foregoing include all the species I have thus far had submitted to my notice, as belonging to the corundum formations under consideration. The specimens did indeed embrace single examples of a pale actinolite, a white radiating asbestus and an isolated crystal of rutile, as well as one of a large and very beautifully colored epidote, but I am led to infer that neither of these minerals are in any sense characteristic attendants of the corundum of the region. One specimen of white, granular dolomitic limestone, containing scales of graphite, was sent, only as having been found in the vicinity of the Cullakenee locality.

[To be concluded.]

## ART. XX.—*Ohm's Law considered from a geometrical point of view*; by JOHN TROWBRIDGE, Assistant Professor of Physics, Harvard College.

OHM'S law is briefly expressed thus: the strength of a current passing in any conductor of a resistance R is equal to the electromotive force producing the current divided by the resistance or $S=\frac{E}{R}$. Let us suppose that a certain quantity of electricity arriving at the point O, is transmitted by the conductor B O C to the surface B C. The quantity passing through any unit $x\,y$ of the conductor will vary inversely as the section $b\,c$, and inversely as the distance of the section from O.

1.



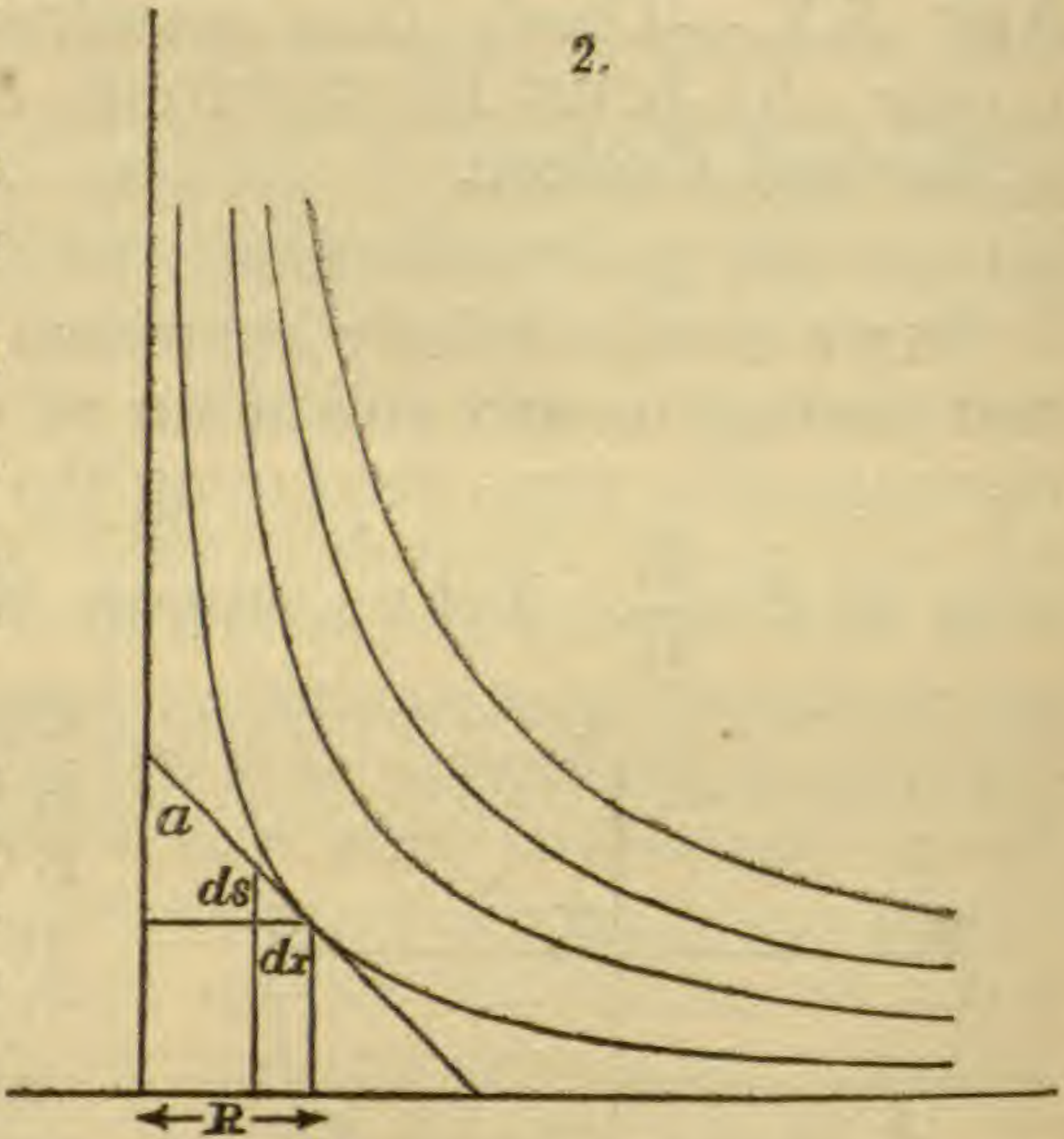
Hence we shall have for an expression of this quantity $q=\frac{Q}{Sx}$ Eq. (1). In which Q represents the entire quantity passing through any section S; and $x$ is the distance of the section from O. If we suppose that the conductor B O C is formed by the revolution of any curve, whose equation is $y=F(x)$, about the axis of X, equation (1) becomes $q=\frac{O}{\pi y^2 x}$. By substituting for $y$ its value from the equation of the curve which generates the conductor, we shall obtain equations which represent, when constructed as curves, the variations in the quantity of electricity passing through a unit section of the conductor.

When the generating curve is $y^2=\frac{1}{x}$ the equation becomes $q=\frac{Q}{\pi}=$ constant; a straight line parallel to the axis of X. If the curve is an equilateral hyperbola, whose equation is $y=\frac{C}{x}$, we shall have $q=\frac{Q}{\frac{\pi C^2 x}{x^2}}=m\,x$ where $m$ is any constant. This is the equation of a straight line passing through the origin. When the equation of the generating curve is $y=C'$ the conductor B O C becomes a cylinder; and $q=\frac{C}{x}$ where C is any constant. This is the equation of an equilateral hyperbola, and is identical with the equation $S=\frac{E}{R}$, which is our original expression for Ohm's law.

If we construct the various curves represented by the equation $S=\frac{E}{R}$, taking X as the axis of resistance, E being constant for each curve, we shall have a series of hyperbolas.

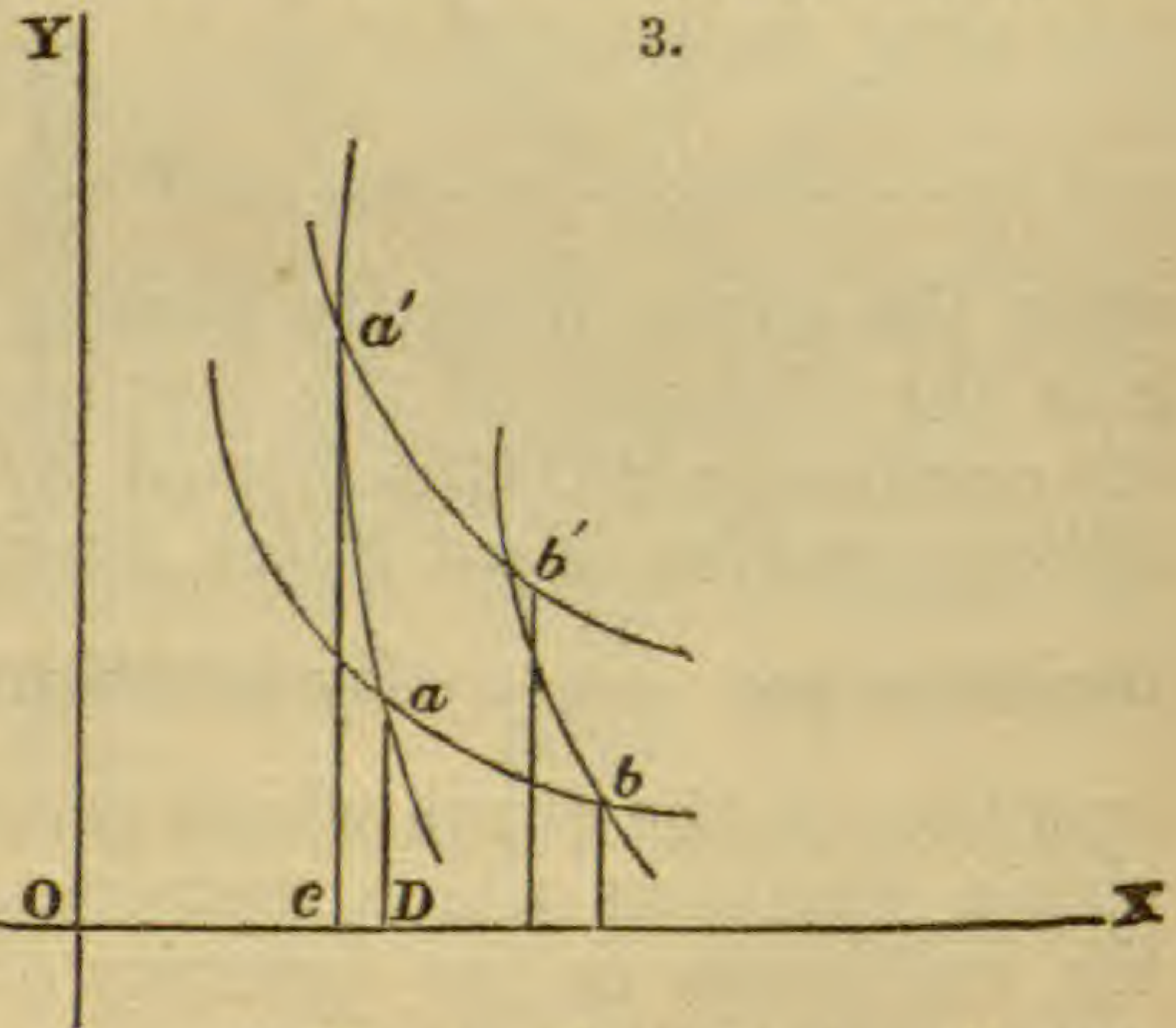
The series of curves represented in fig. 2 may be called isoelectric curves, and present some remarkable analogies to the isothermal curves of thermo-dynamics.

2.



Let $m=F(Sk)$ be the equation of the curve $a\,b$. The curve $a\,b$ will represent the relation between the increase in resistance and the decrease in tension. The curve $a'\,b'$ will represent this relation for $m+dm$. The two curves $a\,b$ and $a'b'$ differ from each other by a constant; the same is true of the curves $a'\,a$ and $b'\,b$, which are similar to the adiabatic curves of thermodynamics. Perhaps the subject is best exemplified by an application to the electromagnetic engine.

3.



"The performance of external work by an electro circuit produces a counteractive force whose magnitude is equal to the external work performed in an unit of time divided by the strength of the current.

"Let W be the external work performed in any unit of time by the engine. This gives rise to a counteractive force which causes the current to be of less strength than that which the battery produces when *idle*. Let $\gamma$ be the strength of the current in the idle circuit, and $\gamma'$ the strength when the work W is performed per unit of time; then the counteractive force is $\frac{W}{\gamma}$, and the strength of the current $\gamma'$ is the same as if the electromotive force instead of being E were $E-\frac{W}{\gamma}$."—(*Prof.*

*W. J. M. Rankine on the General Law of the Transformation of Energy, Phil. Mag.*, 1853.

In fig. 3, if we suppose that the electro-magnetic engine does work, the strength of the current will fall in passing from the resistance O C to O D, by reason of the counteractive force, to the lower isoelectric curve $a\,b$; and $a'\,a\,b\,b'\,a'$ will represent the cycle of operations gone through by the performance of work and a return to the condition of an idle battery.

In fig. 2 $a = \mathrm{R}\frac{ds}{d\mathrm{R}}$. In thermo-dynamics $a$ represents the elasticity of a gas whose volume is represented by the line R. In electro-dynamics $a$ may be taken to represent the capacity of a circuit, of given resistance, for work.

The area of the figure $a\,b\,b'\,a'$ represents the work accomplished in going through the cycle of operations $a\,b\,b'\,a'\,a$. As in thermo-dynamics we shall have $\mathrm{Q}'-\mathrm{Q} = \mathrm{A\,F}$, where F represents the area $a\,b\,b'\,a'$ and Q and Q′ the distribution of tensions. When the area F becomes infinitely small we have $d\mathrm{Q} = \mathrm{A}\,d\mathrm{F}$, and by a discussion of this area in reference to a change from $m$ to $d\,m$ it is found that $\frac{\mathrm{Q}}{m} =$ constant.

In an electric circuit if $\mathrm{T}-t$ represents the difference of potential at any two points, then we have $\text{Heat} = \mathrm{Q}'\,(\mathrm{T}-t)$ or $\frac{\text{Heat}}{\mathrm{T}-t} = \mathrm{Q}' = a$ constant for any one epoch, and $m$ may be taken to represent $\mathrm{T}-t$. The remarkable fact that the efficiency of both the thermo-dynamic and electro-dynamic engine is expressed by the same function $\frac{a-b}{a}$ has already been noticed by Mr. J. P. Joule, Manchester Transactions, vol. x.

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## ART. XXI.—*Brief Contributions to Zoölogy, from the Museum of Yale College.* No. XXIII.—*Descriptions of New North American Myriopods;* by O. HARGER.

THE Museum has lately received a number of interesting Myriopods from various parts of the country, collected in part by the writer while traveling across the continent as a member of Prof. Marsh's Geological Expedition to the Rocky Mountains and Pacific Coast. Owing to the comparative neglect of this group by entomologists, a large proportion of these species are new, and in the following article a few of the most interesting and characteristic forms are described.

*Lithobius pinetorum*, sp. nov.

Ferruginous, head and sometimes a few of the anterior segments of deeper color. Cephalic segment polished, its posterior margin elevated. Ocelli on each side ten to fourteen. Antennæ polished, smooth, except a few scattered hairs near the base, segments gradually decreasing distally, but the terminal segment is usually elongated. Labium with a median groove, a few hairs about the dental lamina; teeth two on each side, acute and spreading; there is sometimes a third smaller external tooth on each side. Mandibles sparsely hairy. Alternate larger scuta polished, with the posterior and lateral margins elevated, except in the last two scuta, where the lateral margins only are elevated, emarginate behind, especially on the posterior segments. Excavations on the coxæ small, nearly circular. Length $15^{mm}$.

This species resembles *L. paucidens* Wood, but is easily distinguished by its smaller size, less number of ocelli, and by the smooth and polished antennæ. It was collected in considerable abundance under the bark of decaying pine logs, by Prof. G. H. Collier and the writer, in the valley of the John Day river, Oregon, in October, 1871.

*Geophilus gracilis*, sp. nov.

Very light orange, head much darker, slender, small. Cephalic segment nearly quadrate. Antennæ hairy, filiform, joints short obconic. Mandibles unarmed. Scuto-episcutal sutures distinct posteriorly. Feet pilose, thirty-nine or forty-one pairs, occasionally forty, last pair thickened and elongated. Sterno-episternal sutures distinct. Body slightly hairy throughout. Length, $15^{mm}$.

This species is not uncommon under stones and rubbish in moist places about New Haven.

### *Trichopetalum*,* gen. nov.

Sterna not closely united with scuta; third and fifth joints of antennæ elongated; scuta furnished with bristles; no lateral pores; eyes present.

This genus belongs to the family *Lysiopetalidæ*, and is closely related to *Pseudotremia* of Cope (Proc. Am. Phil. Soc., vol. xi, p. 179, 1869). It differs from that genus in having no pores, instead of having the "annuli with two pores on each side of the median line." The species further differ from *P. cavernarum* and *P. Vudii* Cope in having seven only, instead of eight, joints in the antennæ. In this point they also differ from *Spirostrephon Copei* Packard, from the Mammoth Cave (Am.

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* From *θρίξ*, a bristle, and *πέταλον*, a leaf or plate.

Naturalist, p. 748, Dec., 1871). It may be remarked that, in the descriptions above referred to, Prof. Cope, in stating the relative lengths of the joints of the antennæ in each of the two species, omits all mention of the 6th joint; and, in the same manner, Prof. Packard omits the second. Prof. Packard's figures also represent only seven joints in the antennæ. *Craspedosoma*, as defined and figured by Gervais (Aptères, vol. iv, p. 119, plate 45, fig. 5), has the sterna and scuta consolidated into a complete ring as in *Polydesmus* and *Iulus*, and therefore differs from this genus, as shown in plate II, fig. 4, by a character considered of family importance.*

*Trichopetalum lunatum*, sp. nov. Pl. II, figs. 1–4.

Dirty white, banded transversely and mottled with light brown anteriorly. Segments 28; males with 45, females with 46 pairs of legs. Head large, dilated laterally, covered with short, erect, bristly hairs. Eyes (fig. 2) of 10 ocelli, in a lunate group, convex toward the bases of the antennæ. Antennæ (fig. 2) pilose, seven-jointed; the joints measure, the first ·07$^{mm}$, second ·10$^{mm}$, third ·23$^{mm}$, fourth ·11$^{mm}$, fifth ·22$^{mm}$, sixth ·09$^{mm}$, seventh ·07$^{mm}$. First scutum semicircular, with the posterior margin slightly concave. Near the outer angles of this scutum are two small tubercles on each side, each bearing a stout bristle, and higher up a third tubercle on each side bears also a bristle. The remaining scuta (fig. 4) throughout are furnished with three bristles on each side, springing from tubercles, the two lower being approximate and situated on the upper surface of the short lateral processes, and the third higher up on the scutum. On a few of the posterior segments these bristles are in a transverse row, and on the last scutum, which is broad and truncate, the two inner ones are thickened at their bases. There is an impressed dorsal line. Legs slender, white, hairy, with the penultimate joint lengthened. The under side of the seventh segment of the male (fig. 3) is furnished anteriorly with a pair of appendages directed backward and curved upward; and posteriorly with a pair of cylindrical jointed organs, directed horizontally outward, tipped with a short bristle, and appearing like modified legs of the posterior sub-segment. In crawling these organs have a motion similar to that of the basal joints of the adjacent legs. Length 6$^{mm}$.

This species is not uncommon under or among decaying leaves in moist woods about New Haven.

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* Since the above was in type Prof. Cope, in an article on the Wyandotte Cave and its Fauna (Am. Naturalist, July, 1872, p. 414), has referred *Spirostrephon* (*Pseudotremia*) *Copei* Packard to a new genus *Scoterpes*, which he characterizes as destitute of eyes and lateral pores. Agreeing with Dr. Packard, he also doubts the validity of his own genus *Pseudotremia*, and refers *P. cavernarum* to *Spirostrephon*. The lateral pores of *P. Vudii* are thus left somewhat doubtful, and without actual examination it is impossible to decide whether or not it is congeneric with the species of *Trichopetalum*.

*Trichopetalum glomeratum*, sp. nov.

This species is somewhat larger than the preceding, but closely resembles it, except in the following points. The general color is somewhat darker. The eyes (pl. II, fig. 5) of 19 ocelli in a subtriangular patch. There are 31 segments, and the fifth joint of the antennæ (fig. 5) is much shorter than the third. These joints, except the first, measure: second, ·20$^{mm}$, third ·40$^{mm}$, fourth ·24$^{mm}$, fifth ·33$^{mm}$, sixth ·18$^{mm}$, seventh ·12$^{mm}$. Length of animal, 10$^{mm}$.

A single specimen of this species was collected by the writer in the valley of the John Day river, Oregon, in October, 1871.

*Trichopetalum iuloides*, sp. nov.

Animal with the aspect of an Iulus, being destitute of lateral processes. Light chestnut, with a dorsal yellow line, and along the sides nearly obsolete yellowish spots. Segments 30. Head somewhat dilated laterally, sparsely bristly, hairy. Eyes triangular. Antennæ (pl. II, fig. 6) sparsely hairy, filiform, seven-jointed, fifth joint longest. The joints measure respectively, first ·10$^{mm}$, second ·12$^{mm}$, third ·21$^{mm}$, fourth ·12$^{mm}$, fifth ·22$^{mm}$, sixth ·08$^{mm}$, seventh ·05$^{mm}$. First scutum nearly semi-circular, but with the lateral angles acute, furnished with a transverse row of six short bristles, as are the other scuta; these bristles are much stronger on the posterior segments, and on the anal segment two of them are thickened at their bases. Under a high power, the scuta are seen to be minutely wrinkled transversely across the back, and longitudinally along the sides. Legs hairy. Length 8$^{mm}$.

This species was collected under stones at Simmons' Harbor, on the north shore of Lake Superior, by Sidney I. Smith, Naturalist to the U. S. Lake Survey.

*Iulus furcifer*, sp. nov.

Dark chestnut brown, beautifully ornamented with a black dorsal line, a lateral row of black spots and transverse bright yellow bands, which are very narrow and interrupted across the back. Feet and under part of body much lighter; segments about 55. Eyes triangular, connected by an impressed line along the upper margin of a dark band, which is encroached upon below by yellowish spots. Antennæ filiform, pilose and nearly black at tip, last joint very short; scuta with impressed lines on the sides, and under a lens the surface of the back is seen to be covered with minute oblong pits; anal scutum not mucronate. Male organs (pl. II, fig. 7) of three pieces on each side directed backward, the outer (fig. 7, *a*) cylindrical and distally hairy on the inner side; within this is a much larger piece (fig. 7, *b*) in the form of an elongated narrow plate bent around a robust spine (fig. 7, *c*), which is the inner and at its base the upper of the three pieces, and is unequally forked at the tip, where it is inclosed by the larger piece. Length 35$^{\mathrm{mm}}$.

This beautiful species was collected by Prof. G. H. Collier and the writer in the John Day valley, Oregon, in October, 1871.

*Polydesmus armatus*, sp. nov.

Color various shades of chestnut brown or sometimes olivaceous, with the lateral laminæ and tip of anal scutum yellow; a few of the posterior scuta are sometimes lighter colored than the others. The inferior border of the face, and the basal joints of the antennæ are yellow; distally the antennæ are much darker. The anal scutum is much prolonged into a truncated spine. The large male appendages (pl. II, fig. 8) are hairy at their base, and consist of two principal portions; the larger and inner (fig. 8, *a*) is cylindrical for the first third of its course, and directed downward, inward, and forward; it then becomes lamelliform, and sends inward and upward a much excavated process (fig. 8, *c*), distally a smaller and less excavated one (fig. 8, *d*), and is at this point contracted, but expands so as to terminate in a much bent plate. The other portion (fig. 8, *b*) is a long curved spine on a bristly cylindrical base, arising a little behind and outside of the former, and curving spirally around it, so that its attenuated tip is received in the excavated process. A small stout hooked spine (fig. 8, *e*) is nearly concealed by the bristles that spring from the base of the larger spine. Length 28$^{\mathrm{mm}}$.

This species resembles *P. Haydenianus* Wood, but may be at once distinguished by the much produced anal scutum, and by the male organs. It was collected in the John Day valley, Oregon, by Prof. G. H. Collier and the writer, in October, 1871.

Yale College, New Haven, Conn., June 27, 1872.

EXPLANATION OF PLATE II.

Figure 1. *Trichopetalum lunatum*, female, magnified 15 diameters.
" 2. Antenna and right eye of the same, magnified 40 diameters.
" 3. Inferior view of seventh segment of male of the same, magnified 40 diameters.
" 4. Diagram of transverse section of segment of the same, magnified 30 diameters.
" 5. *Trichopetalum glomeratum*. Antenna and right eye, magnified 25 diameters.
" 6. *Trichopetalum iuloides*. Antenna, magnified 25 diameters.
" 7. *Iulus furcifer*. Male appendages of left side, magnified 20 diameters, seen from above; *a*, outer cylindrical process; *b*, bent plate; *c*, inner forked process. The distal portion of this process being concealed by the bent plate, the corresponding process on the right side is figured in position.
" 8. *Polydesmus armatus*. Male appendages of left side magnified 20 diameters, seen from below; *a*, larger process; *b*, spine-like process; *c* and *d*, processes from the upper surface of *a*; *e*, curved basal spine.

## ART. XXII.—*Preliminary Description of New Tertiary Mammals;* by O. C. MARSH. Part I.

THE explorations of the Yale College party in the Rocky Mountain region, during the past season, brought to light, in addition to the extinct Birds and Reptiles already described by the writer, many interesting species of new fossil Mammals, and in the present communication a number of these from Wyoming Territory are briefly characterized. Others will be noticed in the succeeding numbers of this Journal, and it is intended, at an early day, to give a full description with illustrations of all the new fossil vertebrates discovered by the two Yale expeditions of 1870 and 1871.

### *Palæosyops laticeps*, sp. nov.

An examination of the large collection of mammalian remains from near Fort Bridger, Wyoming, now in the Yale Museum, shows clearly that there are at least four well-marked species of large pachyderms represented, which have hitherto been referred to *Palæosyops paludosus* Leidy. This species was established on a number of teeth, more or less imperfect, which were all strongly rugose, although evidently belonging to an adult animal. Unfortunately no other portions of this skeleton were secured, so that it may be difficult if not impossible to determine with certainty its exact specific relations.

One of the treasures obtained by the Yale expedition of 1870, which first explored the Green River Tertiary basin, was the nearly complete skeleton of a species of *Palæosyops*, somewhat smaller than the one described by Dr. Leidy. The animal was adult, with the dental series in full perfection, although the epiphyses were not completely coössified with the vertebræ. The teeth in this specimen have apparently the same general structure as those in the type of *P. paludosus*, but differ in being nearly smooth; and this is not the result of age, as this individual was younger than the original of the larger species. The proportions, moreover, given for the molar described* ("22 lines fore-and-aft and 18 transversely"), would not apply to any of the series in the present specimen. The last upper molar of the latter has two well developed internal cones.

The cranium in *Palæosyops laticeps* is broad, and the zygomatic arches much expanded. The squamosal portion is especially massive. The nasals are narrow and elongated, and more like the corresponding bones in *Hyrax* than those in the larger pachyderms. They are prominently convex transversely, and strongly arched longitudinally. The inner edges are thickened

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* Proceedings Philadelphia Academy, 1870, p. 113.

below at the suture, indicating a strong cartilaginous nasal septum. The anterior extremities are truncated, with the external angles rounded. The upper teeth form a complete series. The canine is large, and broadly oval at its base. The outer incisor is the largest, and at its posterior edge the premaxillary is subtriangular in transverse section. The sagittal and occipital crests are strongly developed, and the coronoid process of the lower jaw is short and recurved. The remaining portion of the skeleton, which will be described in detail in the full description, shows conclusively that *Palæosyops* belongs to the Perissodactyls, and not to the Artiodactyl group of mammalia, as supposed by Dr. Leidy.

*Measurements.*

| | |
|---|---|
| Length of entire upper molar series, | 155· mm. |
| Antero-posterior extent of three true upper molars, | 94· |
| Antero-posterior diameter of last upper molar, | 36· |
| Transverse diameter, | 40· |
| Antero-posterior diameter of upper canine at base, | 29· |
| Transverse diameter, | 22· |
| Space occupied by three right upper incisors, | 34· |
| Vertical extent of zygomatic process of squamosal, | 51· |
| Transverse diameter of both nasals near anterior margin, | 42· |
| Width between bases of upper canines, | 49· |
| Width between bases of fourth upper premolars, | 40· |

This unique specimen was discovered in September, 1870, by Mr. A. H. Ewing of the Yale exploring party. The locality was near Marsh's Fork, about fifteen miles from Fort Bridger, Wyoming. The geological horizon was Eocene, or lower Miocene. Other specimens of the same species have since been found in the same region by members of both the expeditions.

*Telmatherium validus*, gen. et sp. nov.

A new genus of large mammals, allied to *Palæosyops*, is indicated by the greater portion of a skull with teeth, and portions of several other skeletons, obtained by the Yale party last year in the Tertiary deposits of the Green River basin. The dentition of this genus, so far as known, appears to be similar to that of *Palæosyops*; but the two may readily be distinguished by the anterior portion of the skull, which in the present genus has the premaxillaries compressed, with an elongated median suture. The zygomatic arch is also much less strongly developed, and the squamosal portion of it is comparatively slender.

The present species exceeded in size *Palæosyops paludosus*, and with the exception of *Titanotherium? anceps* Marsh,[*] is

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[*] This Journal, vol. ii, July, 1871, p. 35. Additional remains of this animal, obtained during our explorations last year, show clearly that it belongs to the Proboscidea, as at first suspected. The species may therefore be called *Mastodon anceps*.

the largest mammal yet discovered in the Fort Bridger beds. The upper molar teeth have the inner cones more elevated and more pointed than in *Palæosyops*, and the basal ridge is well developed. The last upper molar has but a single internal cone. The upper canines are large, pointed, and have strong cutting edges. The outer incisors are the largest, and all these teeth have a strong inner basal ridge. The roof of the mouth is deeply excavated between the premolars. The nasals are decurved laterally, and much compressed.

*Measurements.*

| | |
|---|---|
| Extent of upper molar series, | 224· mm· |
| Extent of upper true molars, | 130· |
| Antero-posterior diameter of last upper molar, | 54· |
| Antero-posterior diameter of last upper premolar, | 28· |
| Transverse diameter, | 33· |
| Space occupied by three right incisors, | 47·5 |
| Antero-posterior diameter of upper canine at base, | 27· |
| Transverse diameter, | 22· |
| Vertical diameter of zygomatic process of squamosal, | 34· |

The specimen on which this description is mainly based was discovered in September last by Mr. J. F. Quigley of the Yale party. The locality was near Henry's Fork of the Green River, in Wyoming, and the geological formation the same essentially as that near Fort Bridger, which contained the previous species.

### *Limnohyus robustus*, gen. et sp. nov.

Among the other remains of large mammals in the Yale Museum, which resemble *Palæosyops paludosus*, in their dentition at least, are portions of several skeletons with the more important parts well preserved. These remains show conclusively that there are two genera represented among them. One of these is doubtless *Palæosyops*, but the type of that genus is too imperfectly known to determine its more important characters. These two genera agree apparently in the structure of the anterior portion of the skull, but differ somewhat in their dentition. In some specimens, which agree best with Dr. Leidy's original description of *Palæosyops paludosus*, the last upper molar has two inner cones, and to this group the name *Palæosyops* may in future be restricted. The other specimens have but a single internal cone on the last upper molar, and for the genus thus represented the name *Limnohyus* is proposed. These genera may be distinguished from *Telmatherium* by the premaxillaries, which are short, stout and depressed, with a small median suture. Other distinctive characters of the three genera will be given in the full description.

The present species may be distinguished from those above described, especially by the strong basal ridge of the molars.

On the last lower molar it extends entirely around the posterior lobe. The first of the upper true molars has the two inner cones nearly of the same size. The small intermediate median tubercles are well developed on the upper molars, and all the teeth are strongly rugose, even in fully adult animals. The nasal bones contract anteriorly and are rounded in front. The outer margin is decurved and thickened. The premaxillaries unite by a very short median suture, similar to that in *Palæosyops laticeps.* The zygomatic process of the squamosal is stout, but much compressed, thus differing widely from both the species already described.

*Measurements.*

| | |
|---|---|
| Antero-posterior extent of last three upper molars, | 110· mm. |
| Antero-posterior diameter of last upper molar, | 41· |
| Transverse diameter, | 43·5 |
| Antero-posterior diameter of last upper premolar, | 20· |
| Transverse diameter, | 26·5 |
| Antero-posterior diameter of last lower molar, | 51· |
| Vertical diameter of zygomatic process of squamosal, | 34· |

The specimen on which the above description is chiefly based was discovered, in September last, by Mr. F. Mead, Jr., near Henry's Fork, Wyoming. Other specimens of the same species were found in the same deposits by Mr. G. G. Lobdell, Jr., Mr. G. M. Keasbey, Mr. O. Harger, and the writer. The geological formation was lower Miocene, or Eocene.

### *Hyrachyus princeps*, sp. nov.

This well-marked species includes the largest of the Tapiridæ yet found in this country. The remains representing it indicate an animal nearly three times the bulk of *Lophiodon Bairdianus* Marsh, and probably twice that of the individual named *Hyrachyus eximius* by Dr. Leidy. The specimens on which the species is based consist of a nearly complete series of upper teeth, and several lower molars, with the more important parts of the skeleton, all pertaining to one animal, and remarkably well preserved. The last two upper molars are unusually large in proportion to the rest of the series, and have the antero-external lobe quite separate, and with its apex incurved.

*Measurements.*

| | |
|---|---|
| Extent of entire upper molar series, | 134· mm. |
| Extent of upper true molar series, | 76· |
| Antero-posterior diameter of last upper molar, | 21·2 |
| Transverse diameter, | 31· |
| Antero-posterior diameter of penultimate lower molar, | 28·75 |
| Transverse diameter, | 17· |

The remains here described were found by the writer, last autumn, near Henry's Fork, Wyoming, in the same Tertiary deposits that yielded the specimens already noticed.

### *Homacodon vagans*, gen. et sp. nov.

A new and very small suilline pachyderm is well represented by the greater part of the skull and skeleton, in excellent preservation. The animal was apparently allied to *Hyopsodus*, and was somewhat larger than *H. paulus*. From that species, it may readily be distinguished by the lower true molars, which have the constituent cones isolated, not alternate, and of nearly equal size. The inner anterior cone is, however, somewhat the largest, and is slightly bifid. The upper molars, likewise, have their cusps conical, and of similar size. The lower premolars are compressed, and resemble those of some carnivors. The skull has a well developed sagittal crest. The astragalus is of the suilline type.

*Measurements.*

| | |
|---|---|
| Antero-posterior extent of the three lower true molars,... | 17·5 mm. |
| Antero-posterior diameter of last lower molar,.......... | 7·3 |
| Transverse diameter in front,.......................... | 4· |
| Antero-posterior extent of three upper molars,.......... | 15·2 |
| Antero-posterior diameter of last upper molar,.......... | 5·5 |
| Transverse diameter in front,.......................... | 6·4 |
| Length of astragalus,.................................. | 14· |

This very perfect specimen was discovered, in September last, by Mr. G. G. Lobdell, Jr., of the Yale party, near Henry's Fork, Wyoming, in the *Mauvaises terres* Tertiary deposits of that region.

### *Limnocyon verus*, gen. et sp. nov.

An interesting new carnivor, somewhat larger than a fox, is indicated by the remains of several individuals, which agree closely in all respects excepting size. One series of these remains includes the greater portion of a skull with most of the upper teeth well preserved. The premolars in this specimen are compressed and obtuse, as in the Canidæ. The first upper premolar is large, and near the canine. There are no true sectorial teeth. The crowns of the first and second upper molars are triangular. The first has a tubercle at each angle, and a large compressed one near the center. The second molar, which is the largest of the series, has an angular tubercle at each corner, and a large bifid cusp near the middle of the crown. The last upper molar is very narrow and elongated transversely. Its straight anterior margin is at right angles to the axis of the skull. The affinities of this genus appear to be with the Viverridæ.

*Measurements.*

| | mm. |
|---|---|
| Extent of last three upper molars, | 23· |
| Antero-posterior diameter of first upper true molar, | 9·6 |
| Transverse diameter, | 8·8 |
| Antero-posterior diameter of penultimate upper molar, | 10·75 |
| Transverse diameter, | 11· |
| Antero-posterior diameter of last upper molar, | 4·5 |
| Transverse diameter, | 12·5 |

The specimens on which this species is established were discovered at Grizzly Buttes, near Fort Bridger, in September last, by Mr. J. F. Quigley and the writer.

### *Viverravus gracilis*, gen. et sp. nov.

A much smaller carnivor, about the size of the common mink, is represented in our collections by two lower jaws with teeth, and a sectorial upper molar of one individual, and portions apparently of several others. The lower jaws in this genus are long, very slender, and compressed; the last two lower molars are tubercular. Both have the posterior part of the crown quite low, and the anterior half elevated, and composed of three angular cusps. The four teeth anterior to these are much compressed. The upper flesh tooth closely resembles that in some of the Viverridæ, and the genus should probably be referred to that group.

*Measurements.*

| | mm. |
|---|---|
| Extent of pre-molar and molar series of lower jaw, | 28· |
| Extent of last three lower teeth, | 15· |
| Antero-posterior diameter of last lower molar, | 4·5 |
| Greatest transverse diameter, | 2·5 |
| Antero-posterior diameter of penultimate lower tooth, | 5·2 |
| Greatest transverse diameter, | 3·4 |
| Antero-posterior diameter of upper sectorial molar, | 7· |

The type specimen of this species was discovered at Grizzly Buttes, Wyoming, last autumn, by G. G. Lobdell, Jr., of the Yale party.

### *Nyctitherium velox*, gen. et sp. nov.

One of the most interesting discoveries of the last Yale expedition in the Tertiary of Wyoming, was the remains of a species of bat, which is of special importance, as no fossil specimen of Cheiroptera has hitherto been detected in this country. The most characteristic specimen obtained is part of a lower jaw, with the last three molars in perfect preservation. This fragment indicates an animal about the size of *Scotophilus fuscus*, and the teeth resemble those in that genus, but are not so wide. They have a distinct basal ridge externally. The jaw below the teeth is much compressed, and its lower border slightly convex longitudinally.

*Measurements.*

| | |
|---|---|
| Antero-posterior extent of last three molars, | 5· mm. |
| Antero-posterior diameter of last lower molar, | 1·75 |
| Transverse diameter, | 1· |
| Antero-posterior diameter of penultimate lower molar, | 1·85 |
| Transverse diameter, | 1·1 |
| Depth of jaw below last molar, | 2· |

The remains on which this species is based were found by the writer, in September last, near Henry's Fork, Wyoming. The formation is Eocene, or lower Miocene.

### *Nyctitherium priscus*, sp. nov.

A somewhat larger species, apparently of the same genus, is indicated by part of a lower jaw, with the penultimate molar perfect. The jaw is less compressed, and the tooth proportionally larger than in the above species. There is no external basal ridge.

*Measurements.*

| | |
|---|---|
| Antero-posterior extent of last three lower molars, | 5·5 mm. |
| Antero-posterior diameter of penultimate lower molar, | 2· |
| Transverse diameter, | 1·5 |
| Depth of lower jaw below last molar, | 2.5 |

This interesting specimen was found by Mr. G. G. Lobdell, Jr., last autumn, at the same locality as the preceding species.

### *Talpavus nitidus*, gen. et sp. nov.

A very small insectivor, apparently allied to the moles, is well represented by several fragments of lower jaws with teeth, and probably by some isolated upper molars. Two characteristic specimens of these remains were found together, and doubtless belonged to the same animal, which was about the size of a mouse. One of these is part of a lower jaw, containing the first and second true molars; the other is an anterior portion with only the last premolar in position. The lower molars resemble externally those of *Talpa*, but on the inner side are more like those of *Scalops:* they have no external basal ridge. The lower jaws are more slender and compressed than those in most recent insectivors. The last premolar is compressed and pointed.

*Measurements.*

| | |
|---|---|
| Antero-posterior extent of first two lower molars, | 2·9 mm. |
| Antero-posterior diameter of penultimate lower molar, | 1·5 |
| Depth of jaw below penultimate molar, | 2· |
| Depth of jaw below last lower pre-molar, | 2· |

The type specimen of this species was found by the writer, in September last, near Henry's Fork, Wyoming.

Yale College, New Haven, July 18, 1872.

# SCIENTIFIC INTELLIGENCE.

## I. CHEMISTRY AND PHYSICS.

1. *An experiment in reference to the question as to Vapor-vesicles;* by T. PLATEAU.—From a research of M. Duprez,* it is known that when a vessel of water is inverted with the opening downward, it is not necessary that this opening be very narrow for the water to remain suspended. M. Duprez kept water suspended in a vertical tube which was nearly 20 millimeters in internal diameter.

Assuming now that when water is hanging in such a tube with the surface downward a small hollow air-bubble is brought into contact with this surface, the air in it, in virtue of the pressure of the envelope, will immediately penetrate into the interior of the liquid, and will rise in it in virtue of the smaller specific gravity. This I have confirmed by means of an experiment. I took a small glass tube of about 4 millims. diameter in the clear, drew it out at one end to an aperture of about 0·4 millim. diameter, and closed the wider end by a cork coated with grease. By touching the drawn-out end with a piece of filtering paper, which was soaked with distilled water, I succeeded in bringing into the narrow aperture a column of this liquid not more than a millimeter in length. By carefully depressing the cork, a hollow bubble is seen to form at the drawn-out aperture, which may have a diameter of less than a millimeter, and usually lasts seven or eight seconds. In this operation the wider part of the tube must be covered with several layers of a non-conducting material in order to eliminate the influence of the warmth of the fingers. After having thus acquired the power of procuring very small hollow bubbles, water was suspended in a tube kept vertical by a suitable stand. The internal diameter of this tube was only a centimeter; and with such a small diameter the suspension is very easy. It is only necessary, after filling the tube with water and closing the mouth with a piece of paper, to invert it and then draw the paper aside to get a free surface; a hollow water bubble of less than a millimeter diameter is then produced as described above, and brought to the free surface of the suspended water. When they are in contact, the bubble detaches itself from the drawn-out tube; the air contained in it penetrates and ascends in the liquid. The experiment, repeated several times, always gave the same result.

Let us now assume that at a certain distance below the surface of the suspended water there is a current of visible aqueous vapor; if this vapor consists of vesicles, each of them, on coming into contact with the surface, will introduce a microscopic air-bubble into the water, which will immediately ascend in it; and the whole of

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* "Mém. sur un cas particulier de l'équilibre des liquides," Mém. de l'Académie Roy. de Belgique, vol. xxvi, 1851, and xxviii, 1854.

these vesicles will form a cloud in the water of the tube, which slowly rises and destroys the transparency.

M. Duprez was good enough to make the experiment at my request. The water was suspended in a glass tube of 13 millims. internal diameter. A small metal vessel with an aperture of several centimeters diameter and containing a certain quantity of water, was placed under the free surface of the water of the tube over a lamp; the mouth of this vessel was about 12 centims. from the surface. A continuous boiling was thus obtained, and a current of visible vapor which rose to the surface of the suspended water; but though the experiment lasted more than half an hour, no cloud was observed in the water of the tube. The vapor condensed on the outside of the tube, and from time to time was wiped away; but the water inside retained all its transparency.

After this it seems difficult to retain any doubt as to the non-existence of the vesicular state. It seems to me, in fact, that only three objections could here be raised. It might either be said that the air-bubbles on penetrating into the water, from their unusual smallness and the very considerable capillary pressure which they have to support from the liquid, dissolve in the liquid, or that all vesicles burst on reaching the surface of the water, or that they roll along the surface of this liquid, separated from it by a thin layer of air or vapor, until they reach the outside edge of the tube to escape thence into the atmosphere.

But the first of these assumptions must be rejected; for the water had previously been shaken with air so long as to be completely saturated, and, secondly, while it was exposed to the action of vapor it must have lost whatever solvent power it possessed; and, moreover, sometimes even comparatively large air-bubbles appear on the upper part of the inside of the tube, where the hotter part of the water ascends.

The second supposition, though not quite inadmissible, is at any rate not very probable. We have seen that our small bubbles, measuring less than a millimeter, do not burst on coming into contact with the surface of water; why should it be otherwise with the vesicles? It may, perhaps, be urged that their envelope is far thinner than that of our small bubbles. But if vesicles exist, their envelope must be so thick that they are colorless; otherwise a cloud irradiated by the sun could have no bright luster; moreover, from the long duration of large clouds, they must be very permanent.

As regards the third supposition, is it probable that all vesicles could roll along the surface without touching? Moreover M. Duprez has repeated the experiment in such a manner that this surface was concave and remained so in spite of the fact that the volume of water increased owing to expansion by heat and the condensation of vapor; but now in this case a large number of vesicles should have rolled toward the apex of the concavity, have accumulated there, and therefore must soon have placed themselves in contact with the fluid surface: but nothing in the result was different; no cloud disturbed the transparency of the water.

I consider the above experiment, though not decisive, yet a very powerful argument against the hypothesis of the vesicular condition.

May I here be permitted to recall another experiment, which I have described in the eighth series of my investigation *Sur les figures d'équilibre d'une masse liquide sans pésanteur.* One of the chief objections which have been raised against the vesicular condition is, that the air contained in a vesicle would be exposed on the part of the fluid envelope to a considerable pressure, the effect of which would be that this air would gradually dissolve in the envelope, and would traverse it, by which the vesicle would soon change into a complete globule. But when a laminar calotte, about a centimeter in diameter, is formed by means of a solution of 1 part of Marseilles soap in 40 of water, and this is allowed to stand in an atmosphere laden with aqueous vapor, it sometimes lasts for more than twenty-four hours and becomes quite black. At the same time a remarkable phenomenon is witnessed; the calotte gradually decreases and ultimately disappears,—from which it follows that the enclosed air has gradually traversed the lamina. This lamina is indeed far thinner than a vesicle would be; but, on the other hand, theory shows, from the difference of the liquids and the diameter, that in the interior of a vesicle the pressure would be more than a thousand times as great as in the interior of a calotte of soap-solution at its original dimensions.—*Bull. Acad. Belgique*, vol. xxxii.—*Phil. Mag.*, IV, xliii, 316.

2. *On the nitro-compounds of the fatty series.*—By the action of ethylic iodide upon argentic nitrite, Meyer and Stüber have obtained a new substance isomeric with ethylic nitrite. When ethylic iodide is poured upon argentic nitrite, violent ebullition ensues. To complete the reaction, the mixture may be heated for some hours with a reversed condenser. On distillation, a mixture of ethylic iodide and nitrite passes over at first; afterward the new substance, which boils at 111°–113° C. The authors give this body the name of nitro-ethan. It is a perfectly colorless, clear liquid, of a peculiar agreeable, etherial odor. Its density at 13° C. is 1·0582 (taken with reference to water at the same temperature); it is insoluble in water, does not explode on heating, and burns with a pale flame. Analysis and a determination of the vapor density gave the formula $Ꞓ_2H_5N\Theta_2$, which is also that of ethylic nitrite, the boiling point of which is 96° C. lower. When nitro-ethan is heated with iron filings and acetic acid, a violent reaction ensues, which must be moderated by plunging the flask into cold water so that the liquid does not boil. On subsequent distillation with caustic potash, ethylamin passes over in large quantity, and in a state of great purity. Hence it appears that nitro-ethan corresponds to the aromatic nitro-compounds. The relation between this body and ethylic nitrite may be expressed by the formulas:

| $Ꞓ_2H_5-\Theta-N\Theta.$ | $\Theta_2N-Ꞓ_2H_5.$ |
|---|---|
| Ethylic nitrite (B. p. 16° C.). | Nitro-ethan (B. p. 111°–113° C.). |

A solution of caustic potash dissolves nitro-ethan, which appears to possess weak acid properties. Sodium attacks it with evolution of gas and formation of a white powder, which explodes on heating. The authors promise a further investigation of this very interesting subject.—*Berichte der Deutschen Chem. Gesell., Jahrgang* v, p. 399. W. G.

[*Note.* Is it not possible that in the extraordinarily stable double salts of cobalt, iridium and rhodium, having respectively the formulas, $\text{Ȼo}_2(\text{N}\Theta_2)_{12}\text{K}_6$, $\text{Ɨr}_2(\text{N}\Theta_2)_{12}\text{K}_6$, and $\text{Ɍh}_2(\text{N}\Theta_2)_{12}\text{K}_6$, the nitrous atom is $(\Theta_2\text{N}-)$ and not $(-\Theta-\text{N}\Theta)$? W. G.]

3. *On the reduction of glutanic acid by iodhydric acid.*—The researches of Ritthausen have rendered it probable that glutanic acid is one of the homologues of malic acid containing $\text{C}_5$ in the molecule. DITTMAR has given the subject a careful study, and has arrived at the following results: Glutamic acid was first prepared by the action of sulphuric acid upon pure conglutin. The pure glutamic acid, $\text{Ȼ}_5\text{H}_9\text{N}\Theta_4$, was then converted into glutanic acid by the action of nitrous acid. The glutanic acid obtained in this manner, $\text{Ȼ}_5\text{H}_8\Theta_5$, was then heated in a sealed tube with a saturated solution of iodhydric acid, by which it was converted into desoxy-glutanic acid, $\text{Ȼ}_5\text{H}_8\Theta_4$. The new acid is bibasic, forms large crystals which belong to the monoclinic system, and is easily soluble in water. The formula of this acid is the same as that of pyrotartaric acid, with which it is not, however, identical. From this it appears that glutanic acid is not identical with either of the three homo-malic acids derived from aconitic acid, since all of them yield pyrotartaric acid by reduction.—*Journal für prakt. Chemie, Neue Folge*, Band 5, p. 338. W. G.

4. *On an aldehyd-alcohol.*—WURTZ has obtained a new polymer of aldehyd, having the formula $\text{Ȼ}_4\text{H}_8\Theta_2$, to which he gives the name of aldol. It is a perfectly colorless liquid, which after cooling becomes thick like a pure solution of sugar. It is so viscid at 0° that the tube containing it may be inverted without any flow of liquid. When gently heated, it becomes as fluid as water, but it regains its viscid character only some hours after cooling. Its density at 0° is 1·1208: it has a strong aromatic and bitter taste, and mixes in all proportions with water and alcohol. When heated to 135°, aldol is resolved into crotonic aldehyd and water:

$$\text{Ȼ}_4\text{H}_8\Theta_2 = \text{Ȼ}_4\text{H}_6\Theta + \Theta\text{H}_2.$$

Aldol reduces argentic nitrate and cupropotassic sulphate. When heated for some days with anhydrous acetic acid, aldol forms an acetate having the formula $\text{Ȼ}_4\text{H}_7\Theta(\text{Ȼ}_2\text{H}_3\Theta_2)$; and a diacetate having the formula $\text{Ȼ}_4\text{H}_6(\text{Ȼ}_2\text{H}_3\Theta_2)_2$, which may be regarded as the diacetate of crotonic aldehyd. Nitric acid oxidizes aldol with great energy, forming several organic acids not yet described. Phosphoric chloride also acts energetically upon aldol, forming a chloride which is probably $\text{Ȼ}_4\text{H}_7\text{Cl}_2$. This is a thick, colorless liquid almost impossible to purify. Wurtz considers aldol to have a constitution represented by the formula:

$$\text{ȻH}_3\text{—ȻH}(\Theta\text{H})-\text{ȻH}_2\text{—ȻH}\Theta.$$

By the prolonged action of chlorhydric acid upon aldehyd, Wurtz obtained also the anhydride of aldol $(\text{Ȼ}_4\text{H}_7\Theta)_2\Theta$. The author remarks that aldol in many respects resembles the sugars, glucose being like aldol, at the same time an aldehyd and an alcohol.—*Comptes Rendus*, Tome lxxiv, p. 1361. W. G.

## II. GEOLOGY AND MINERALOGY.

1. *Fossils probably of the Chazy era in the Eolian Limestone of West Rutland;* by E. BILLINGS. (From a letter to J. D. Dana, dated Montreal, May 23.)—I received last summer some fossils from Rev. A. Wing, and made the following note upon them before I sent them back.

15th June, 1871, from the Rev. A. Wing, twenty specimens with the following ticket:

"Encrinites and obscure fossils, supposed to be Trenton, collected May, 1870, at the marble quarries, West Rutland, not one hundred yards northwest of an abandoned marble quarry,—the most northern one worked on the southwest side of the valley, say one hundred and fifty rods southwest from Barns' hotel, West Rutland."

This is the description of the locality given by Mr. Wing. My note on the fossils in the book I keep for such purposes is—

"These consist of numerous obscurely preserved forms like *Pleurotomaria staminea*, small encrinal joints, and a single plate of *Palæocystites tenuiradiatus*. I think this collection is Chazy. The rock is a gray fragile limestone with white crystalline seams in it."

If Hitchcock's Eolian is Stockbridge limestone, then the latter includes the Chazy. The plate of the Cystidean, *P. tenuiradiatus* Hall, is a never-failing guide to the Chazy; at least it is so on the west side of Lake Champlain.

2. *Hayden Exploring Expedition. On the Discovery of the Quebec formation in the Territory of Idaho;* by Prof. F. H. BRADLEY, Geologist of the Expedition. (From a letter to J. D. Dana, dated Fort Hall, Idaho, July 7th, 1872.)—I write to announce the discovery of the Quebec group. I first found it in the mountains on the east side of Malade valley, about six miles south of Malade City. It is here full of trilobites, with some gasteropods and brachiopods. It is at least 1,000 feet thick, and probably much more than that. (I hope to work out the details as we pass southward, in October.) The limestone is underlaid by a compact ferruginous quartzite, which, from its position, may be considered as representing the Potsdam sandstone. I traced the bed, nearly continuously, to the angle of Port Neuf Cañon—a distance of over fifty miles. At this latter point the bed seems to thin somewhat, and is overlaid by 1,000 feet or more of quartzites.

I am hoping to get again upon this group as we pass northeastward to the Tetons.

3. *La Névé de Justedal et ses Glaciers;* par C. DE SEUE, adjoint à l'Institut Météorologique de l'Université Royale de Christiania. 56 pp. 4to, with a chart, 9 photographs, and a lithographic plate. Christiania, 1870.—We cite here a few facts from this important memoir. The great névé of Justedal covers a high plateau, lying between Sogn on one side and the Nordfiord and Söndfiord on the other, having a height of 1400 to 1650 meters above the sea, but passing northeastward into a chain of mountains, the culminating summits of which are the Lodalskaobe, 2076 meters in height, St. Cæciliekronen in the Olden, 1950 meters, Suphellenipa, near the middle of the névé, 1720 meters, and Gottop-hesten, to the west of the valley of Vetlefiord, 1710 meters in height. The névé, or uninterrupted snowfield, takes its origin northeast of Lodalskaobe, and terminates to the southeast by the Skarvedalsbræ, where it descends the valley of Viksdalen into the Söndfiord. The length of the *névé* region is thus about 42 miles, and its surface over nine hundred square miles.

The glaciers of the first class that descend from the snow-plateau are those of—1, Vetelfiord; 2, Boium; 3 and 4, Suphelle; 5, Langedal; 6, 7, Optag; 8, Austerdal; 9, Tunsbergdal; 10, 11, 12, Bergset; 13, Nigar; 14, Faobergstol; 15, Lodal; 16, Stegeholt; 17, Gredung; 18, Bödal; 19, Nesdal; 20, Aobrække; 21, Brigsdal; 22, Melkevold; 23, Fond, in the valley of Stardal; 24, Lunde. After numerous measurements of the rate of progress in the movement of the ice in the glaciers of Boium, of Tunsbergdal, and of Lodal, in the course of the month of August, the author gives the following as the mean results.

In the case of the glacier of Boium, the rate per hour near its extremity was 0·28 in. (Norwegian and English); 3,000 feet above the extremity it was 0·66 in. 108 yards from the less convex side, 0·87 in. at the middle, and 0·81 about 100 yards from the opposite or more convex side; while at points along the less convex side within 20 to 40 yards of the margin the rate was 0·16 in. to 0·20 in. an hour.

For the glacier of Tunsbergdal, the rate per hour near the extremity was 0·23 in.; about 2,900 feet above it was 0·37 in. 135 yards from the concave side; about the middle 0·41 in. to 0·51 in.; and toward the convex side 0·62 in. to 0·63 in.

For the glacier of Lodal, the rate per hour, about 490 yards above the extremity, was at the middle 0·104 in. to 0·091; about 1,875 yards above the extremity 0·183 in., 0·218 in. and 0·212 in.; about 2,500 yards from the extremity, just below where the two great tributary glaciers come in from the right and left, 0·047 in. near the margin; 100 yards from this margin, 0·140 in.; about the middle of the glacier 0·279 in. to 0·297 in.

The author states that the inclination of the surface beneath the glacier where the measurements were made is about the same for each, and that the difference in the rate of motion depended on the pressure from the upper part of the glacier. This pressure was, therefore, much the greatest for the glacier of Boium. The conclusions are drawn that the movement diminishes in ratio toward the sides, bottom, and extremity of the glaciers; and when the course bends, so that one side is convex and the other concave, the movement is most rapid on the convex side.

These are only a few of the results brought out in this Memoir. It is illustrated by a map and diagrams, and also by a photographic plate made up of nine photographic views of the Norwegian glaciers.

4. *The Ancient Glacier of the Rhone.*—It is known that, at a former period, the glacier of the Rhone spread westward over lower Switzerland to the Juras, and lodged boulders on these mountains, and that Prof. Guyot followed the lines of these boulders from the Juras across the plain to their sources in the Alps. Messrs. Falson and Chantre have recently traced the path of this glacier all the way from Lake Geneva, southwestward to Lyons, passing by Seillon, Châtillon, Ars and Sattonay, and even ten miles farther south, to Vienne in Dauphiny. In its course, after being joined by the glacier o. the Arve (of which the Mer de Glace was one of the sources), it encountered the local glaciers of some of the valleys of Bugey; but it ended in surmounting the latter and depositing its moraines of crystalline rocks over their moraines of limestone rocks.—*Bibl Univ.*, 1870, xxxviii, 118, and *Bull. Soc. Geol. de France*, 1869, xxvi, 360; from *Bibl. Univ.*, 1872, xliv, 46.

5. *Glacial action in Fuegia and Patagonia.* Abstract of a letter by Prof. Agassiz of the Hassler expedition, addressed to Prof. B. Peirce, dated Talcahuano, April 27.—In the straits of Magellan, passing Sandy Point, the first unquestionable *roches moutonnées* seen were on the nearest coast opposite Cape Froward. In Port Gallant large boulders were observed, some 6 feet across, one 12 × 6 × 5 feet, well polished and scratched. In Fortescue Bay there are similar scratched boulders; and also glacier scratches on the rocks, their trend W.N.W.; and glacial phenomena continue from this region to Jerome Point, which is itself well polished, especially on the south side. York River valley, which trends north, is well polished on both sides, and so also a gorge opposite, showing that the denudation was not due to an agent following the east-and-west course of the Magellan straits. The two heads of the narrowest part of the straits are beautifully polished and rounded. Similar glacial effects were observed in Borgia Bay; and Pourtales traced them up a peak to a height above the sea of about 1,500 feet. Glacier Bay and Sholl Bay were also very remarkable for their glacial phenomena. In Glacier Bay and other points there was evidence that the glacier once had a much greater extension than now.

Prof. Agassiz concluded from the character of the north and south sides of the summits in Fuegia, and other facts, that the movement of the ice was to the north, and independent mainly of the present slopes of the land.

The region over which Prof. Agassiz states that he observed glacial phenomena in southern South America includes all of the continent south of 37° of S. latitude both on the Atlantic side (Bay of St. Matthias) and the Pacific side. At Talcahuano large erratic boulders and *roches moutonnées* were observed at the mouth of the Biobio, on the hills of Hualpen. In the Bay of San Vincente (between Concepcion Bay and the Bay of Arauco) glacial markings were found by Mr. Pourtales, at the sea level—"a magnificent polished surface," says Prof. Agassiz, "as well preserved as any I have seen under glaciers of the present day, with well-marked furrows and scratches;" and this is in latitude 37°. The place is only a few feet above tide level, upon the slope of a hill on which stand the ruins of a Spanish fort. The course of the scratches is nearly east and west; but some cross the main trend, and run southeast. The magnetic variation at Talcahuano is 18°3′, the true meridian being to the right of the magnetic.

6. *Annual Report of Prof. D. F. Boyd, Superintendent of the Louisiana State University, for the year* 1871, *to the Governor of the State of Louisiana.* 222 pp. 8vo. New Orleans, 1872.—This report, besides its interesting information on the condition, prospects, and needs of the Louisiana State University, contains also the Third Annual Report of the Botanical Survey of Southwest and Northwest Louisiana, made in 1871, by Prof. A. Featherman; and that of the

*Geological Survey of the State* by F. V. HOPKINS, M.D., Prof. Geol., Chem. and Min., in the University.

In the Geological Report, Prof. Hopkins gives many important facts with regard to the Post-tertiary deposits, and also a colored geological map of the State. The Post-tertiary is stated to consist, following the order of age, of (1) the Drift, (2) the Port Hudson group (so named by Hilgard), (3) the Loess, (4) the Yellow Loam. To the last three the term *Bluff formation* is here applied; and it is stated that "the delta formed by the Mississippi from the end of the Drift period to the era of the Loess was composed of its strata."

The beds of the Port Hudson group are mostly hard sand-beds, sandy-clay and clay; they are more or less calcareous, and are often characterized by calcareous concretions. The thickness at the sulphur wells of Calcasieu is 160 to 282 feet. Prof. Hilgard made the thickness at New Orleans 650 feet; but Prof. Hopkins gives reasons for suspecting that part of this may belong to the proper loess. Sea shells of recent species are occasionally found in the southern part, and fresh water shells in some places. The deposits also afford bones of various Post-tertiary animals, including those of the *Megalonyx*. Lignite layers also occur in it, and sometimes contain stumps.

The loess is mostly a fine silt, little compacted. The water of slight rains is absorbed into its porous beds, while heavier flows wear the formation into deep channels having nearly vertical sides. It often contains land shells of the genera *Helix*, *Helicina*, *Pupa*, *Cyclostoma*, *Achatina*, and *Succinea*, as long since observed near Natchez by Lyell; and sometimes affords bones of the mastodon.

The yellow loam bed is for the most part between ten and twenty-five feet in thickness. "It is remarkable for the uniformity with which it appears at various levels, proving its deposition since the occurrence of a large amount of denudation in the older Bluff strata themselves."

Prof. Hopkins holds, we believe correctly, that the Port Hudson and the overlying beds were deposited when the land was at a lower level than now; and that the loess was the accumulation over an old flood plain of the Mississippi, as suggested by Lyell,—that the "lake-like expansion of fresh-water so widely spread over the interior [in which it was formed] being really that of an estuary connected with the Gulf," and not of a true lake confined by a supposed barrier.

The drift contains pebbles, which are mainly of reddish and yellowish chert. Among them there are numerous Silurian and Devonian fossils, with some from Carboniferous rocks; and more than a hundred species have been identified, and are included in a list on pages 191 to 195 of the report. Among these there are 12 species of Crinoids, 25 of corals, and over 40 of Brachiopods; they must have come from Tennessee and the States farther north either side of the Mississippi river; some of them from points 400 miles or more distant. The thickness of the drift in Louisiana is stated to average 200 feet. In the northwestern part of the State there are Azoic pebbles from Arkansas. "The drift beds are stratified more or less regularly, as if deposited in running water; they show unmistakably the action of running and not of quiet water." They contain rarely angular masses half as large as a man's head; and near Vicksburg blocks of even 500 lbs. weight have been observed; "and there is no doubt," says Prof. Hopkins, as also in substance, earlier, Prof. Hilgard, "that the passage of this formation into the modified drift of the North is easily demonstrable. The drift was transported by water, and must date from the period of depression, or melting of the glacier."

For the distribution of the drift Prof. Hopkins brings in the agency of icebergs in a sea at least 1,159 feet deep in the Ohio valley, drift-boulders of large size occurring at this elevation in Licking Co., Ohio; and on this point he adds; "We are here far within the Mississippi valley, and must grant that when the water floated an ice raft either up or over a hill 1,159 feet above the present sea-level there was a clear sweep for the polar current from the straits of Belle Isle (Gulf of Newfoundland) to the Gulf of Mexico."

This iceberg theory appears to be here called in just where it cannot serve. If the Gulf of Mexico had then opened up over the Continent either to the Arctic or the Gulf of Saint Lawrence, with the water 1,159 feet—or say 1,000 to 2,000 feet or more—deep at the Gulf, the Gulf stream would be the current, *if any*, that would have occupied the great interior sea of the Mississippi valley; and this would have given icebergs southward-bound a hard up-stream current to beat against. There does not appear to be any good foundation for the conclusion that the Labrador current would have had control of the waters. The latter flow would have had by its position greatly the advantage of the former in the contest for possession of the Mississippi region, and would at least have nullified the Labrador flow if nothing more. The iceberg theory is therefore as wholly inapplicable to Louisiana as it is to New England. Besides the above consideration, and also the objection to a continental submergence in the absence of the skeletons of whales, shells, and other sea relics from the drift regions of the continental interior, there is other evidence against the iceberg theory in the fact that the deposits of sand bear evidence, in many places, as Prof. Hopkins says, that they were made by *running water* in rapid flow. The oceanic currents are slow lazy currents, even the best or largest of them; 3 miles an hour is an unusual rate, and 5 miles an extreme except in narrow channels. So slow a movement is wholly inadequate to produce the kind of beds making up much of the drift; for the sand deposits often bear evidence of the fling of the waves, or of the violent rush of a tidal current, as sketched by Prof. Hilgard in his Geological Report on the State of Mississippi. The deposits of this wave and tidal-current origin in the vicinity of New Haven bay (Connecticut), where it is certain the depth of water was small—not too great for effects from wave-action—are precisely identical with some of those of the stratified drift of the State of Mississippi figured and described by Professor Hilgard.

But, while the glacier may have brought the boulders of Licking Co., Ohio, to their present place, and been a prominent means of transportation over the north, icebergs, or in more commonplace language, *floating ice*, may still have had, during the opening Champlain era, a vast deal to do with the transportation of stones, pebbles, and sand in the region of the Great Lakes and along the Mississippi valley. The melting of the great glacier, "6,000 feet" thick to the north, would have gone forward for a long period with extreme slowness, even after the subsidence of the Champlain epoch had taken place. But sooner or later the flood from the melting would have begun; and, as Prof. Hilgard has shown, it would have been a vast flood, coming as it must have done from ice that buried deeply the whole breadth of the Mississippi water-shed, and even from regions beyond it, since one or more of the Great Lakes then poured their waters southward—an area hardly less than twenty millions of square miles. And, in such a flood, masses or bergs of ice, freighted with stones, would have gone with the current southward; while the waters in rapid violent flow would have transported the sands and even coarser material, and made deposits of all the various kinds observed.

J. D. D.

7. *Investigations on Fossil Birds;* by Mr. A. Milne-Edwards.—At the moment when my investigations upon fossil birds approach their termination, and before the last part is given to the public, I will ask the Academy's permission to explain in a few words the results at which I have arrived during these studies, which have lasted fully twelve years.

I believe I have demonstrated, by the examination of the bones which have been found in the recent deposits in the Mascarene Islands, and which belong, for the most part, to extinct species, such as the dodo, the solitaire, the *Aphanapteryx*, *Fulica Newtoni*, large parrots, &c., that these islands have once been part of a vast extent of land, that these lands by little and little and by a slow depression have been hidden under the waters of the ocean, only leaving visible some of their highest points, such as the islands of Mauritius, Rodriguez, and Bourbon. These islands have served as a refuge for the last representatives of the terrestrial population of these ancient epochs; but the species, confined in too limited a space and exposed to all causes of destruction, have disappeared by degrees; and man has in some measure aided in their extinction.

Madagascar evidently was not in communication with these islands; for when Europeans visited them for the first time, they did not find there any Mammalia, with the exception of some large bats; none of those remarkable Lemuridæ peculiar to the fauna of Madagascar existed in the Mascarene Islands. The study of fossil birds leads to the same result; and the three species of *Æpyornis* which Mr. A. Grandidier and I have been able to recognize among the fossils collected in the swamps of the southwest coast have enabled us to establish the relationship which connects these birds with the *Dinornis*, the *Palapteryx*, and *Aptornis* of New Zealand. All these species belong to the same zoölogical type, and make us feel that at a more or less remote epoch there may have existed some communication between these lands so far away from one another; perhaps groups of islands, now submerged, formed intermediate stations, of which unfortunately we have now no trace.

In France, from the earliest age of man, we remark sometimes in superficial deposits, sometimes in caverns, fragments of birds which furnish us with valuable indications of the climatal conditions of that epoch. Some of these species have now entirely disappeared: others, in considerable numbers, have by degrees retired toward the north—for instance, the grouse and the great hawk owl, which then were extremely common in these countries. Their presence is most significant; for even supposing, according to some naturalists, the reindeer is only found fossil in France because it had been introduced by the Finnish population, we cannot invoke the same explanation for birds which have never been domesticated. Lastly, we also find in our caves a great number of species identical with those which now inhabit temperate Europe—among others, the cock, which was supposed to be a native of India, but which, on the contrary, must have been a contemporary of the first ages of man.

It is especially the Middle Tertiary deposits which have furnished me with a rich harvest. Thus in the Department of the Allier I have recognized the presence of about 70 species belonging to very various groups, some of which no longer belong to our fauna. Parrots and trogons inhabited the woods; swallows built in the fissures of the rocks nests in all probability like those now found in certain parts of Asia and the Indian archipelago. A secretary bird nearly allied to that of the Cape of Good Hope sought in the plains the serpents and reptiles which at that time, as now, must have furnished its nourishment. Large adjutants, cranes, flamingoes, the *Palælodi* (birds of curious forms, partaking both of the characters of the flamingoes and ordinary Grallæ), and ibises frequented the banks of the water-courses where the larvæ of insects and mollusks abounded; pelicans floated in the midst of the lakes; and, lastly, sand-grouse and numerous gallinaceous birds assisted in giving to this ornithological population a physiognomy with which it is impossible not to be struck, and which recalls to one's mind the descriptions which Livingstone has given us of certain lakes of southern Africa.

The list I have given of the birds whose existence I have ascertained in the part of the Miocene lakes the alluvium of which has formed the deposits of St. Géraud le Puy, of Vaumas, &c., indicates the relations in which the different groups of this class of vertebrates lived. Whilst some of them are extremely common, there are others which are found, so to speak, only accidentally, and which are only represented in my collection by a single bone or only a few bones. The species most frequently met with are the water-birds: thus the ducks have left numerous remains; the cormorant is only found at certain places. Evidently at that time, as now, birds had preferences for certain places, certain rocks, &c., from which they departed but little. The little diver (*Colymboides minutus*) is less abundant than the gulls, of which two species, *Larus elegans* and *L. totanoides*, exist in profusion.

It is the same with some of the small shore-waders belonging to the genera *Totanus* and *Tringa*, whilst *Elorius* and *Himantopus* are represented by few individuals. I have found numerous bones of the ibis, and in particular of the *Palælodus ambiguus;* the four other species of the latter genus are by no means so common. Thus out of two hundred bones of these birds hardly one will turn out to be of *P. crassipes*, *P. minutus*, *P. gracilipes*, or *P. goliath*. The portions of the skeleton of the flamingo are rarely found entire at St. Géraud le Puy; whereas at Coumon and Chaptuzat, on the contrary, they are well preserved. I have only once met with the bones of the adjutant; they belonged to two young specimens, and were associated in the same excavation filled with sand. The cranes are rare; their bones are almost always broken and often injured by the teeth of rodents, as if they had lain for a long time on the bank before being carried to the bottom of the lake. The rails, the gallinaceous birds, the pigeons, the sand-grouse, the passerine birds, the raptores, and the parrots have left but few traces of their existence. These birds, from their mode of life, did not remain continually on the shores of the lakes or water-courses; their remains might be eaten or destroyed at once, and it would need a concurrence of exceptional circumstances for them to be transported by the streams into the alluvial deposits of the lakes: thus I had explored these deposits for more than ten years before I met with a single bone of a parrot, sand-grouse, secretary bird, or of several of the raptores; and some, of which I had collected the remains a long time ago, have not appeared since.

All the bones of birds collected in the Miocene beds of Weissenau, in the basin of Mayence, that I have been able to examine, present a complete resemblance to those of the Department of the Allier.

The ornithological population of the celebrated deposit of Sansan, in the Department of the Gers, presents another character; not one of its representatives is found in the lacustrine deposits of the Bourbonnais and the Auvergne: and although the greater part of the species belong to families at present existing in our fauna, not one is known to be actually living, and several of them present characters sufficient to constitute new genera.

I have discovered there a parrot of a more slender form than that of the Allier, and I have designated it by the name of *Psittacus Lartetianus*, to attach the name of my regretted master and friend to one of the most interesting species that I have ever found in this rich deposit. Some gallinaceous birds of a large size, and in this respect hardly inferior to the peacocks and true pheasants, also inhabited the shores of the little lake, where the deposits accumulated which now form the hill of Sansan; numerous passerine birds, resembling the Bengalis and Senegalis, frequented the margins of the waters; lastly, the number of species was not less than 35, and certainly new excavations will not fail to make known more.

The marine faluns of the Loire have only furnished me with a few species of birds. I have been able, however, to recognize a cormorant almost as large as that which now lives on our shores, a goose a little smaller than the bernicle, a heron, and a pheasant.

The beds of gypsum in the environs of Paris contain numerous impressions of skeletons of birds; and it is to be observed that the animals of that period deviated more from the zoological forms which exist at the present day. Thus, despite the unwillingness I feel, especially in paleontological studies, to increase the already too large number of generic groups, I have been obliged to form new genera for many among them. Thus the *Cryptornis antiquus* was nearer the hornbills than any known type; *Laurillardia* and *Palægithalus* belong to the order of passerine birds, but were quite distinct from all those now living. The *Palæortyges* are gallinaceous, of the size of a quail, but very different from those birds. *Gypsornis* is the giant of the family Rallidæ; it must almost have attained the size of a stork. *Agnopterus* approaches the flamingoes, although it displays some peculiar characters.

The singularity of the forms of these Eocene birds makes us doubly regret not knowing those of the Cretaceous period. Unfortunately there exist only a very small number of freshwater deposits dating from that period; therefore it is not astonishing that we have as yet discovered only very few traces of terrestrial animals which lived during the deposition of these important strata. Perhaps new zoological forms will be discovered there filling up the immense gap which exists between the Jurassic *Archæopteryx* and the typical birds of the Tertiary epoch.*—*Comptes Rendus*, April 15, 1872, pp. 1030–1034.—*Ann. Mag. Nat. Hist.*, IV, x, 72.

8. *Fossil Vertebrates from the Niobrara and Upper Missouri.*—Prof. Leidy has founded a species of lion, *Felis augustus*, on several teeth and fragments of jaws from the Loup Fork of the Niobrara, Nebraska, obtained by Dr. Hayden. The most characteristic specimen is an upper sectional molar about as large as that of the Bengal tiger. He has also described a vertebra from the base of the tail, belonging to an animal related to Plesiosaurus and Discosaurus; and "viewing the specimen as probably representing a genus different from those mentioned, he proposes for the species the name *Oligosimus grandævus*." Another specimen obtained by Dr. Hayden in the "Black Foot country" at the head of the Missouri, "looks as if it had formed part of the dermal armor of some huge saurian, or perhaps of an armadillo-like animal." Accompanying this specimen there is a distal phalanx, which may belong to the same species, named *Tylosteus ornatus*.—*Proc. Acad. Nat. Sci.*, April 2, 1872.

9. *Extinct Mammals from the Tertiary of Wyoming;* Prof. *Leidy*.—One specimen here described is a fragment of an upper jaw with two molar teeth, and another a lower jaw with one molar. The upper molars have crowns composed of four lobes, the outer of which are like those of *Anchitherium*. The three upper molars occupied a space of eight lines. They are too large for the known species of *Hyopsodus* or *Microsyops*, and nearly accord with the lower molars of *Notharctus*. The species is named *Hipposyus formosus*.

Dr. Leidy remarked that the tooth of *Anchippodus riparius* was obtained from the Tertiary, Miocene or Eocene, of Monmouth Co., N. J.; and if the determination is correct it would go to show that the Bridger Tertiary formation of Wyoming was contemporaneous with the Tertiary deposit of Monmouth Co., N. J.—*Proc. Acad. Nat. Sci. Philad.*, Apr. 2, 1872, p. 37.

10. *Graptolites.*—Prof. Allman has a valuable article on the morphology and affinities of Graptolites in the May number of the Annals and Magazine of Natural History, which concludes as follows:

"Their alleged Polyzoal [or Bryozoan] affinities, however, have some claim on our acceptance. Indeed, were it not for the discovery of the probable graptolite gonosome (corbulæ?), we should have nearly as much to say for this view as for that which would refer them to the Hydroida, more especially as the discovery of *Rhabdopleura* renders us acquainted with a polyzoon in whose test

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* On this point, see Marsh, this Journal III, iii, 56, 360.

is developed a chitinous rod in almost all respects like that of the graptolites.*

On the whole, then, it would seem that the graptolites constitute a very aberrant hydrozoal group having manifest affinity with the Hydroida, to which they are connected by the nematophore-bearing genera of the latter, while they have also important points of connexion with the Rhizopoda. The undoubted members of this group are further characterized in an eminent way by the possession of a solid supporting rod; and it is this feature which has suggested to me the name of RHABDOPHORA, by which I have proposed to designate them."

11. *Descriptions of New Species of Fossils, from the vicinity of Louisville, Ky.;* by JAMES HALL and R. P. WHITFIELD (continued). 12 pp. 8vo. Published June 12, in advance of the Report on the State Museum.—The species described are Brachiopods, Gasteropods and Conchifers, of the Silurian and Devonian.

12. *Coal of Lota.*—Prof. Agassiz states in his letter (see p. 134) that the coal of Lota, and of other localities in the vicinity, north and south, is unquestionably Cretaceous. It is covered with beds containing *Baculites*, many specimens of which he has collected.

13. *On the Rate of Growth of Coral Reefs;* by JAMES D. DANA.—Allowing that the Madrepora of the wreck, mentioned on page 126, may grow 3 inches in height a year, and that other Madrepores increase in the same ratio, it is still not easy to deduce from it the rate of increase of the reef. In the first place, the whole Madrepore is growing over the sides of its branches, at the rate, if we may judge from the size of the trunk at base, of a tenth of an inch a year, thus increasing annually the diameter a fifth of an inch a year, which, in a large species, is a very great addition to the three inches per year at the extremities of the branches. Again, the branches of the large Madrepore of the wreck were widely spaced, those of *M. cervicornis* having intervals of from six to eighteen inches or more between the branches.

In fact it is impossible to make any exact estimate of the amount of increase without a knowledge of the weight of the part annually added. This ascertained, it would be easy to calculate how much the added coral would, if ground up, raise the area that is covered by the Madrepora. A rough estimate gives the author an average increase to this surface of a fourth of an inch a year. But this fourth must be much reduced, if we would deduce the rate of growth of the reef; because a large part of the reef-grounds—that is, of the region of soundings receiving the coral débris—is bare of growing corals. This is the case with much the larger portion of all lagoons and channels among reefs, the bottoms of which, as already explained, are often sandy or muddy, and to a great extent so because too deep for living corals; and it is true

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* The comparison of the rod of *Rhabdopleura* with that of a graptolite has already been made by Dr. Nicholson ("Manual of Zoology"), though he adopts the more generally accepted view which finds hydrozoal rather than polyzoal affinities in the graptolites.

even of the coral plantations, these including many and large barren areas. These unproductive portions of reef-grounds constitute ordinarily at least two-thirds of the whole; and making this allowance, the estimate of one-fourth of an inch a year would become one-twelfth of an inch.

Again, shells add considerably to the amount of calcareous material, perhaps one-sixth as much as the corals; but against this we may set off the porosity of the coral.

The rate of growth of the *Mæandrina clivosa*, stated on page 125, would make the rate of increase in the reef very much less rapid. The specimen—grown within fourteen years—weighs 24 oz. avoirdupois, and has an average diameter of 7 inches. This gives for the amount of calcareous material—the specific gravity being 2·523 (p. 99)—16·45 cubic inches; which is sufficient to raise a surface seven inches in diameter to a height of 0·428 inch; and consequently the average *yearly* increase would be about 1-33d of an inch. Allowing for two-thirds of the reef-ground being unproductive in corals, the rate of increase for the whole would become 1-100th of an inch. But supposing that shells add one-fourth as much as the corals to the reef material, the rate of increase would become about 1-80th of an inch per year.

The specimen of *Oculina diffusa*, referred to on page 125, weighs 44 ounces, which is five-sixths more than that of the Mæandrina, while the average diameter of the clump is the same. The average annual increase would consequently cover a circular area of 7 inches diameter 1-18th of an inch deep. And making the same allowances as above, the rate for the year for the whole reef-grounds would be 1-44th of an inch. The specimen of Mæandrina mentioned by Major Hunt is not here made the basis of a calculation, because we have not the specimen for examination, and it is not certain that the diameter stated by him was not the horizontal diameter.

These estimates from the *Mæandrina clivosa* and *Oculina diffusa* have this great source of uncertainty, that the growth of the groups may not have been begun in the first year of the fourteen. Further, the corals obtained by Major Hunt near Fort Taylor, Key West, may not have been as favorably situated for growth as those of the outer margin of the reef. Again, we have made no allowance for the carbonate of lime that is supplied by the waters by way of cement, supposing that this must come originally, for the most part, from the reef itself. Besides, we have above supposed all the coral reef-rock to be solid, free from open spaces; and, further, it is not considered that much of it is a coral conglomerate, in which the fragments have their original porosity.

On the other side, we have not allowed for loss of débris from the reef grounds by transportation into the deep seas adjoining, believing the amount to be very small.

Whatever the uncertainties, it is evident that a reef increases its height or extent with extreme slowness. If the rate of upward progress is even one-sixteenth of an inch a year, it would take for an addition of a single foot to its height one hundred and ninety years, and for *five feet a thousand years.*

It is here to be considered that the thickness of a growing reef could not exceed twenty fathoms (except by the few feet added through beach and wind-drift accumulations), even if existing for hundreds of thousands of years, unless there were at the same time a slowly progressing subsidence; so that if we know the possible rate of increase in a reef, we cannot infer from it the actual rate for any particular reef; for it may have been very much slower than that. Without a subsidence in progress, the reef would increase only its breadth.—*Dana's Corals and Coral Islands*, pp. 250–253.

18. *Revue de Géologie pour les années* 1868 and 1869, par M. DELESSE et M. de LAPPARENT. Vol. viii. 1872. Paris (Dunod, Editeur).—This Review will be found of great value to all interested in the progress of geological science, or in its practical applications in connection with mining, artesian explorations, tunneling, etc. It has now reached its *eighth* volume.

19. *Memoirs of the Geological Survey of India.*—Vol. VII, Part 1, contains papers on the Vindhyan Series in the Northwestern and Central Provinces, by F. R. Mallet; Mineral Statistics of India, Coal, by Thomas Oldham; Geology of the Shillong Plateau, by H. R. Medlicott; Part 2, the papers on the Kurhurban and Deoghur Coal-fields, by T. H. Hughes; and Part 3, on the Aden Water Supply, by F. R. Mallet, and on the Karanfura Coal-field, by T. H. Hughes.—The paleontological part, issued in 1871, in 4to, Vol. III, contains as Ser. VI, the Pelecypoda, with a review of all known genera of this class, fossil and recent, by F. Stoliczka, illustrated by numerous plates; and Ser. VII, Kutch Fossils, some Tertiary Crabs, by F. Stoliczka, also illustrated in excellent style.

20. *Geological Survey of Canada*, ALFRED C. SELWYN, F.G.S., Director. *Report of Progress for* 1870–71, 352 pp., 8vo. 1872.—This volume contains Reports, by the Director of the Survey, on the progress of the Survey and on the Gold Fields of Quebec and Nova Scotia; by Messrs. L. W. BAILEY and G. F. MATTHEW on Southern New Brunswick; by Mr. ROBB on Northwestern New Brunswick; by Mr. RICHARDSON on the country north of Lake St. John; by Mr. VENNOR on Frontenac, Leeds and Lanark Cos., Ontario; by Mr. BROOME on Phosphate of Lime and Mica; by Mr. BELL on the region north of Lake Superior. The most elaborate report is that of Messrs. Bailey and Matthew. Their report gives a full account of the geology of the region up to the present time, and of the progress of discovery with reference to it, taking up, in succession, the Laurentian areas and rocks, the Huronian or those regarded as probably of this system, the Primordial under the name of the St. John Group, the Upper Silurian, the Devonian, the Lower Carboniferous, the Carboniferous or Coal Measures, and the Triassic or New Red Sandstone. The geology

of New Brunswick was well posted up by Dr. Dawson, in 1868, in the second edition of his excellent Acadian Geology, and illustrated by a geological map. The authors give a more detailed account of some parts of the subject, together with the results of their recent explorations.

The St. John Group, or Primordial beds, the most interesting of all the formations, first recognized as distinct from the adjoining beds by Mr. Matthew, was first proved to be Primordial by Prof. C. F. Hartt, his discoveries with regard to the fossils, added to those previously obtained, enabling him to announce this conclusion with full confidence, the species of *Lingula*, *Paradoxides*, *Agnostus*, *Conocephalites*, *Obolellæ*, &c., placing it beyond doubt. The formation consists mainly of shales and is stated to be a little over 2000 feet in thickness. It occurs in Southern New Brunswick in the depression extending from the city of St. John by way of Loch Lomond lakes to Hammond river, and in the valley of the Kennebecasis, and St. John rivers, in St. John and Northern Kings Counties, and perhaps also in the Nerepis valley and at some other points. The strata of these regions are particularly described in the report, to which the reader is referred for special information on this and other points in Southern New Brunswick Geology.

21. *Stirlingite, Rœpperite.*—Kenngott, in the February number of the Jahrbuch für Mineralogie, has applied to the chrysolite containing zinc, described by Rœpper in this Journal, II, l, 35, the name *Stirlingite*, and to the *manganesian dolomite*, of the same author and page, the name *Rœpperite.* The former is named Rœpperite by Prof. Brush in the supplement to Dana's Mineralogy, issued a month later in March last. As the silicate is more deserving of a distinct name, it is to be regretted that Rœpper's name cannot be affixed to it.

The Jahrbuch für Mineralogie, Geologie und Palæontologie, of Leonhard and Geinitz (formerly Leonhard and Bronn), published at Stuttgart, is the only journal in which mineralogists will find all the latest mineralogical news. It is an excellent journal also in its other departments, geology and paleontology.

22. *Oligoclase from Wilmington, Delaware.*—N. Teclu gives for the composition of this oligoclase,

$\ddot{\text{S}}\text{i}$ 64·75, alumina 23·56, lime 2·84, soda 9·04. potash 1·11=101·30.

The mineral is described as remarkable for having cleavage parallel to both prismatic faces.

23. *Cryptomorphite in Nevada.*—Professor Davidson mentions the occurrence of this borate in Nevada in nodules from four or six inches to as many feet. A specimen had the appearance of French prepared chalk. It occurs in extensive deposits, but the locality the discoverer declined to disclose. Dr. Blake had found it to be a form of borate of lime.—*Proc. Cal. Acad. Sci.*, iv, 195.

24. *Zeunerite* of A. Weisbach, an *arsenate* of uranium and copper, related to uranite in luster, grass-green color, tetragonal

crystallization and easy basal cleavage. G. = 3·2. Composition according to Winkler,

$\ddot{\bar{\mathrm{A}}}\mathrm{s}$ 15·1, $\dddot{\bar{\mathrm{U}}}$ 55·6, $\dddot{\bar{\mathrm{F}}}\mathrm{e}$ 5·2, $\dot{\mathrm{C}}\mathrm{u}$ 8·7, $\dot{\mathrm{C}}\mathrm{a}$ 1·2, $\dot{\mathrm{H}}$ 14·5=100·3.

From the analysis is deduced the oxygen ratio for the

$\dot{\mathrm{C}}\mathrm{u}$, $\dddot{\bar{\mathrm{U}}}$, $\ddot{\bar{\mathrm{A}}}\mathrm{s}$, $\dot{\mathrm{H}}$, 3 : 18 : 10 : 24.

*Jahrb. Min.*, Feb., 1872, 207.

25. *On a Transparent Garnet from Jordansmühl in Silesia;* by Mr. WEBSKY of Breslau.—This garnet is colorless and has G.= 3·609. The form is the dodecahedron, but with a very obtuse tetrahexahedron $i$-$\frac{64}{63}$. It afforded Websky on analysis,

$\ddot{\mathrm{S}}\mathrm{i}$ 37·88, $\dddot{\bar{\mathrm{A}}}\mathrm{l}$ 21·13, $\dot{\mathrm{F}}\mathrm{e}$ 4·19, $\dot{\mathrm{M}}\mathrm{n}$ 0·45, $\dot{\mathrm{N}}\mathrm{i}$ 0·28, $\dot{\mathrm{M}}\mathrm{g}$ 2·88, $\dot{\mathrm{C}}\mathrm{a}$ 31·28, $\dot{\mathrm{H}}$ 1·08=99.17;

whence it is essentially an alumina-lime garnet.

26. *On the composition of the vapors or gas escaping in the Phlegræan Fields and other places near Vesuvius;* by Mr. GORCEIX.—At the great Solfatara, the gas of the 20th of July consisted of sulphuretted hydrogen 7·0, carbonic acid 88·8, oxygen 0·7, nitrogen 4·5. On the 24th, HS 5·0, $CO^2$ 80·0, O 2·7, N 12·3, the temperature of the gas was 110° to 120°C. On the 25th the gas afforded HS 10·0, $CO^2$ 73·3, O 3·0, N 13·7.

At the Grotto di Zolfo the gas from the entrance consisted of HS 4·7, $CO^2$ 88·2, O 0·7, N with combined gas 6·4; and that from the interior gave for the same ingredients the numbers 5·7, 87·8, 0·7, 5·8. The gas from the baths at Lake Agnano, taken on the 24th of July, afforded in two analyses, $CO^2$ 83·3, 86·9, O 4·1, 2·0, N 12·6, 11·1, with a trace of HS in the second. In other portions there was less of carbonic acid and more of atmospheric acid.

At Torre del Greco gas escaping from fissures in the lavas of the recent eruption afforded HS 20·0, $CO^2$ 91·5, O 0·7, N with combined gas 7·8. [These numbers do not foot up 100, and either that for HS or that for CO must be 20 or less in error]. At Chiatamone the gas consisted of $CO^2$ 82·1, O 1·7, N 16·2.—*Ann. Ch. Phys.*, IV, xxv, 559.

27. *Note on Rhinosaurus;* by O. C. MARSH.—In the June number of this Journal (p. 461), I proposed the name ***Rhinosaurus*** for a new genus of Mosasauroid reptiles. As this name proves to be preöccupied, it may be replaced by ***Tylosaurus***. The name ***Rhamphosaurus***, since suggested by Prof. Cope, cannot be retained, as it was given to a genus of lizards in 1843 by Fitzinger.

## III. ZOOLOGY AND BOTANY.

1. *Note on Intelligence in Monkeys;* by Prof. COPE.—I have two species of *Cebus* in my study, *C. capucinus*, and a half-grown *C. apella*. The former displays the usual traits of monkey ingenuity. He is an admirable catcher, seldom missing anything, from a large brush to a grain, using two hands or one. His cage door is fastened by two hooks, and these are kept in their places by nails driven in behind them. He generally finds means sooner or later to draw out the nails, unhook the hooks and get free. He then occupies himself in breaking up various objects and examin-

ing their interior appearances, no doubt in search of food. To prevent his escape I fastened him by a leather strap to the slats of the cage, but he soon untied the knot, and then relieved himself of the strap by cutting and drawing out the threads which held the flap for the buckle. He then used the strap in a novel way. He was accustomed to catch his food (bread, potatoes, fruit, etc.), with his hands, when thrown to him. Sometimes the pieces fell short three or four feet. One day he seized his strap and began to throw it at the food, retaining his hold of one end. He took pretty correct aim, and finally drew the pieces to within reach of his hand. This performance he constantly repeats, hooking and pulling the articles to him in turns and loops of the strap. Sometimes he loses his hold of the strap. If the poker is handed him, he uses that with some skill, for the recovery of the strap. When this is drawn in, he secures his food as before. Here is an act of intelligence which must have been originated by some monkey, since no lower or ancestral type of Mammals possess the hands necessary for its accomplishment. Whether originated by Jack, or by some ancestor of the forest who used vines for the same purpose, cannot be readily ascertained.

After a punishment, the animal would only exert himself in this way when not watched; as soon as an eye was directed to him, he would cease. In this he displayed distrust. He also usually exhibited the disposition to accumulate to be quite superior to hunger. Thus he always appropriated all the food within reach before beginning to eat. When different pieces were offered to him, he transferred the first to his hind feet to make room for more; then filled his mouth and hands, and concealed portions behind him. With a large piece in his hands, he would pick the hand of his master clean before using his own, which he was sure of.—*Proc. Acad. Nat. Sci.*, April, 1872, p. 40.

2. *Curious Habit of a Snake;* by Mr. Cope.—Mr. Cope made the following remarks:—I had for some time a specimen of *Cyclophis æstivus*, received from Fort Macon, N. C., through the kindness of Dr. Yarrow, living in a wardian case. The slender form of this snake, and its beautiful green and yellow colors, have led to the opinion that it is of arboreal or bush-loving habits. It never exhibited such in confinement, however, and instead of climbing over the Caladia, ferns, etc., lived mostly under ground. It had a curious habit of projecting its head and two or three inches of its body above the ground, and holding them for hours rigidly in a fixed attitude. In this position it resembled very closely a sprout or shoot of some green succulent plant, and might readily be mistaken for such by small animals.—*Ibid.*

3. *Diatoms in Hot Springs.*—Dr. Blake has collected diatoms at a hot spring in Pueblo valley, Humboldt Co., Nevada, the temperature of which was 163° F. More than fifty different species were recognized by him; and they were found to be mostly identical with the species found in beds of infusorial earth in Utah and described by Ehrenberg, showing that the latter must have

been accumulated in a hot lake, of about the same temperature. No other living species were found in the hot waters, excepting red algæ. The deposit was a large one, and in it there were concretions of silica. On making a thin section of one of these concretions, a pair of legs of a coleopterous insect was visible in the quartz; the greater part of the concretion was made up of petrified algæ.

In one of the hot springs at the California geysers, having a temperature of 198° F., he found two kinds of Conferva, one capillary, resembling *Hydrocrocis Bischoffii*, but larger, the other a filament, with globular enlargements at intervals. In another spring, the temperature 174° F., many Oscillariæ were found, which by the interlacement of their delicate fibers formed a semi-gelatinous mass; and also two diatoms. In the water of the creek of Geyser Cañon, 112° F., the algæ formed layers sometimes 3 inches thick covering the bottom of the pools, and the same diatoms were found as in the 174° spring. The waters are acidulated by the presence of free sulphuric acid, and Dr. Blake suggests that this may account for the rarity of diatoms.—*Proc. Cal. Acad. Sci.*, iv, 183, 189, 193, 197.

4. *Life in the Mammoth Cave. The Mammoth Cave and its inhabitants, or descriptions of the Fishes, Insects and Crustaceans found in the Cave; with figures of the various species and an account of allied forms, comprising notes upon their structure, developments and habits, with remarks upon subterranean life in general;* by A. S. Packard, Jr., and F. W. Putnam, Editors of the American Naturalist. 62 pp. 8vo. Salem, 1872.—This excellent and most interesting memoir first appeared in the American Naturalist for December, 1871, and January, 1872. It treats of one of the most curious departments of natural history,—the *subterranean* life of the continent and world, and is illustrated by two plates and many wood-cuts. The work is got up in fine style, and is issued by the Naturalist's Agency at Salem.

5. *Reproduction of Sponges.*—Mr. H. J. Carter has an important article on this subject in the Ann. Mag. Nat. Hist. for June, and also in the same number he describes two new sponges from the Antarctic Sea, and a new species of Tethya from Shetland.

6. *Robert Brown's first Botanical Paper*, "*The Botanical History of Angus*," which was read before the Edinburgh Natural History Society on the 26th of January, 1792, and was found in its MS. records last summer by Dr. Carruthers, has been printed in the Journal of Botany, British and Foreign, for November last. It mainly consists of notes upon the rarer plants which the writer collected in Angusshire in the summer of 1791. He was then in his 18th year. The most interesting article contained in it is that upon *Drosera rotundifolia* which is as follows:

"*Drosera rotundifolia* is a plant not unfrequently to be met with on marshy ground. According to Mr. Lightfoot, the *longifolia* is equally common in Scotland, but this is far from being really the case. It has of late been asserted that the leaves of the *Drosera* have the power, when a small body is applied to their upper surface, of contracting and enclosing the substance so applied, by this means in many cases proving a trap to those insects which happen to light upon them. The examination of this fact is certainly worth the attention of the naturalist. In the second edition of Withering's 'Botanical Arrangement,' it is alleged that this phenomenon was observed immediately to follow the application of the substance. But it appears from works of a late German author, that several hours generally elapsed before the leaf was completely folded together. The same author observes that when an insect is placed upon a leaf it naturally endeavors to escape, but is prevented by the viscid juice which is secreted by the long hairs on its upper surface. In a short time these hairs begin to be bent inward, and gradually clasp the insect, which about this time is found dead, not so much in all probability from the pressure of the hairs, which cannot be great, but rather from the nature of the fluid which they exude. After the hairs have thus enclosed the animal the leaf itself begins to contract, and by very slow degrees at last covers its prey. Although I by no means pretend to deny the fact alleged by Dr. Withering, which was related from the actual experiment, yet I am more inclined to give more credit to the German author's experiments. In a few trials which I made myself no contraction followed after a very considerable time, nor did I at all observe it. But as it must be owned that these were made with a pin instead of an insect, I cannot pretend to contradict the fact, but rather to blame the mode in which the trials were made. For it is well known to every one who has seen this plant in the growing state that many of its leaves are generally folded, and if these are opened there is always found some substance enclosed. If, therefore, the *Drosera* is endowed with such a power (and there is the strongest reason to believe it is), we shall have some difficulty in accounting for it on principles merely mechanical."

The "German author" referred to was probably Roth. In our November number an observation on the folding of the leaf of *D. longifolia* by Mrs. Treat of New Jersey is recorded, and was thought to be wholly new. A. G.

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7. Prantl's memoir upon *Inuline*, an inaugural dissertation, we believe, crowned by Munich University, and printed in Bot. Zeit., 1870, No. 39, is thus noticed:—

"The results obtained by the author of this memoir are in all essential features in accordance with what MM. Nägeli and Sachs have said of inuline. M. Prantl describes this substance as a hydrate of carbon, which differs from starch, cellulose, and licheneine, in never taking on an organic form. Its fixity sufficiently differentiates it from dextrine. It seems to approach most nearly to cane-sugar.

"Inuline is constantly found in plants in the form of a solution of 1 part of inuline to 7 of water. As in artificial solutions, 0·01 gram of inuline saturates 100 cub. centims. of water, we may suppose that when dissolving in the plant it undergoes transformation. It never appears except in subterranean organs.

"This substance is pretty frequently produced in plants of different families, but especially in the Compositæ. The *Dahlia* and certain *Helianthi* contain considerable quantities of it.

"From a physiological point of view, inuline plays exactly the part of one of those nutritive principles which are put in reserve, such as starch, sugar, oils, &c. As we have just said, it exists exclusively in subterranean organs, tubers, or rhizomes. At the moment of growth it is transformed into cane sugar toward the collar of the root, then mounts into the stem in the form of starch, and thus passes to the buds. Subsequently, the starch produced in the leaves descends along the stem in the form of starch itself or of sugar, and it is only on its arrival in the root that it takes on the form of inuline."

The concluding portion of Micheli's revision is devoted to fecundation in phænogamous plants, and the part which insects play in it. Upon this topic the readers of this Journal have been kept in a general way informed. A. G.

8. The *Erysiphei* of the United States. 26 species are enumerated in Seemann's (now Trimen and Baker's) Journal of Botany for Jan., 1872, in an article by Mr. Cooke and C. H. Peck of Albany; and half that number of Schweinitz's species of *Erysiphe*, and one or two of Berkeley and Curtis, are appended as Species inquirendæ. A supplement appears in the June number. A. G.

9. *Kan-sun* is the name of a Chinese culinary vegetable, known under the English name of cane-shoots, upon which Dr. Hance has an article in the May number of the Journal of Botany. He commends it as one of the nicest vegetables known, in flavor nearest the American "green corn," but of a peculiar richness and delicacy. He has ascertained that these cane-shoots are the solid base of a grass, viz. of *Hydropyrum latifolium* of Grisebach, which is so very nearly allied to our American *H. esculentum*, i. e., to *Zizania aquatica*, that, if not the very same, Dr. Hance thinks it probable our wild rice may afford similar esculent roots,—which may be worth attending to. A. G.

10. *Martius, Flora Brasiliensis*, fasc. 55, contains *Violaceæ*, *Sauvagesiaceæ* (restored to a distinct order, with indications of nearer affinities to *Parnassia* and to *Hypericaceæ* than to *Violaceæ*), *Bixaceæ* (well including *Samydaceæ*), *Cistaceæ*, and *Canellaceæ*, the last two each of a single genus and species, by the editor, Prof. Eichler; *Tropæolaceæ* (which we like to see kept separate from *Geraniceæ* proper, but not as an order), *Molluginaceæ*, *Alsinaceæ*, *Silenaceæ*, *Portalaccaceæ*, *Ficoidaceæ*, and *Elatinaceæ* (very small orders in Brazil, but their exposition accompanied by some acute remarks), by the late Dr. Rohrbach, whose early death is much to be regretted. Systematic botanists are few in Germany. A. G.

## IV. ASTRONOMY.

1. *On the Temperature of the Surface of the Sun;* by J. ERICSSON.—It will be recollected that Messrs. M. E. Vicaire and Sainte-Claire Deville read some papers before the Academy of Sciences at Paris last January, showing that the temperature of the solar surface does not exceed that produced by the combustion of organic substances, their reasoning being based on the law of radiant heat established by the investigations of Dulong and Petit. I have in the meantime instituted a series of experiments on a comparatively large scale, in order to test the correctness of the said law. Accordingly, the dynamic energy developed by the radiation of a mass of fused iron weighing 7,000 pounds, raised by "overheating" in the furnace to a temperature of 3,000° F., has been carefully measured.

Sir Isaac Newton assumed that the quantity of heat lost or gained by a body in a given time is proportional to the difference between its temperature and that of the surrounding medium. Some eminent scientists, however, accepting Dulong's conclusions and formula, assert positively that the stated assumption is incorrect. In so doing they apparently overlook the conditions inseparable from the Newtonian doctrine, namely, that the conducting power of the radiating body should be perfect; that at every instant the temperature pervading the interior mass should be transmitted to the surface.* It needs no demonstration to prove that if the conducting power of a body be so perfect that the temperature of the center is at all times the same as that of the surface; in other words, that the fall of temperature at the center, occasioned by radiation, is as rapid as the fall of temperature at the surface, the rate of cooling of such a body will be very different from that observed by Dulong and Petit. The investigation instituted by those experimentalists has in reality established only the degree of conductivity of the radiators employed, under certain conditions, but by no means their true radiant energy at given temperatures. M. E. Vicaire and Sainte-Claire Deville, therefore, commit a serious mistake in assuming that the *quantity* of heat transmitted by the radiation of incandescent bodies at high temperatures has been determined. It may be observed that the relation between the time of cooling and the *quantity* of heat transmitted by radiation which Dulong and Petit established, also

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* The writer has just completed a set of experiments with a spherical radiator, 2·75 in. in diameter, composed of very thin hammered copper, charged with water kept in motion by a wheel applied within the sphere, revolving at a rate of 30 turns per minute, the centrifugal action of which brings the particles of the central portion of the fluid so rapidly in contact with the thin spherical shell, that the apparently absurd condition of perfect conductivity has been practically fulfilled. The result of carefully conducted experiments with this radiator, enclosed in an exhausted vessel kept at a constant temperature, has established that Newton's law relating to radiant heat, up to a differential temperature of 100° Fahr. (beyond which the investigation has not extended), is rigorously correct. The subject will be fully discussed in a future article.

misled Pouillet regarding the temperature of the solar surface, which he computed at 1,461° C., or at most 1,761° C. It will be well to bear in mind that Pouillet had himself ascertained with considerable accuracy the temperature produced by solar radiation on the surface of the earth; and also the retardation suffered during the passage of the rays through the terrestrial atmosphere. He was therefore able to demonstrate that the dynamic energy developed by solar heat amounts to nearly 300,000 thermal units per minute for each square foot of the surface of the sun. Considering the imperfect means employed by Pouillet, his "pyrheliometre," the exactness of his determination of solar energy is remarkable. The truth is, however, that the near approach to exactness was somewhat fortuitous, the eminent physicist having underrated the energy of radiant heat on the surface of the earth, while proportionately over-estimating the retarding influence of the terrestrial atmosphere. The true dynamic energy developed by radiation at the surface of the sun, exclusive of the absorption of the solar atmosphere—no doubt exceedingly small—determined by the solar calorimeter mentioned in a previous article, is 312,500 thermal units per minute upon an area of one square foot. It will be proper to notice that this amount is not a mean result of a number of observations, but the greatest energy developed at any time during observations continued upwards of three years, namely February 28, 1871. It will be proper to add that this result has been withheld from publication until it could be verified by a second observation indicating an equal energy. Fortunately the sky at noon, March 7, 1872, proved to be as clear as on the previous occasion referred to, the indicated energy differing only a few hundred units from that developed February 28, 1871.

Temperature being a true index of molecular and mechanical energy, conclusively established by the exact relation between the degree of heat and the expansive force of permanent gases under constant volume, it is surprising that Pouillet did not perceive that an intensity of 1,461° C. or 1,761° C., could not possibly develop on a single square foot of surface the enormous energy represented by 300,000 thermal units per minute. M. Vicaire, adopting like Pouillet Dulong's formula, states in the paper presented to the French Academy that "an increase of 600° is sufficient to increase the radiation a hundred fold;" and that Pouillet has verified Dulong's law to more than 1,000°. "Supposing," he observes, "that beyond this temperature the law ceases to be true, it cannot be absolutely remote from the truth for the temperatures of from 1,400° to 1,500°, which we deduce by adopting the law." Sainte-Claire Deville concludes his essay on solar temperature thus:—"In accordance with my first estimate I believe that this temperature will not be found far removed from 2,500° to 2,800°, the numbers which result from the experiments of M. Bunsen, and those published long ago by M. Debray and myself." The French *savans* then agree that the temperature of the surface of the sun does not exceed the intensity produced by the combustion of organic substances, their grounds for this assumption being, as we have seen, Dulong's formula relating to the velocity of cooling at high temperatures. But Dulong and Petit did not carry their investigations practically beyond the temperature of boiling mercury; hence their formula relating to high temperatures is mere theory, the soundness of which we have now been enabled to test most effectually by measuring the radiant power of a mass of fused metal raised to a temperature of 3,000° F., 30 inches in depth, presenting an area of 900 square inches.

Before describing the means which have been employed in measuring its radiant power, let us briefly consider the condition of the fused mass during the experiments. In the first place, the temperature has been sufficiently high to produce an intense white light, luminous rays of great brilliancy being emitted by the radiant surface during the trial; (2) the bulk of the fused mass being adequate, the intensity of radiation has been sustained without appreciable diminution during the time required for observation; (3) the temperature being higher than that which the French investigations assign to the surface of the sun, while the bulk, as stated, is sufficient to maintain the temperature of the fused mass, it may be reasonably asked, why an area of one square foot of our experimental radiator should not emit as much heat in a given time as an equal area on the solar surface, if its temperature be that assumed by Pouillet? It may be positively asserted, moreover, that an increase of the dimensions of our radiator to any extent, laterally or vertically, could not augment the intensity or the dynamic energy developed by a given area. Again, Dulong's formula, as applied by scientists, shows that the emissive power of a *metallic* radiator, raised to a temperature of 3,000°, reaches the enormous solar emission computed by Pouillet.

[The description and drawings of the calorimeter used with the account of the method of experimenting are here omitted.—Eds.]

Having thus ascertained practically the amount of dynamic energy developed by the radiation of a metallic body raised to the high temperature of 3,000°, we have only to show in a similar manner the amount of energy developed by a metallic radiator of a low temperature, to be enabled to demonstrate the correctness or fallacy of Dulong's formula. Numerous experiments have been made for this purpose with apparatus of different forms, the results having proved substantially alike. The device most readily described consists of a spherical vessel charged with water, suspended within an exhausted spherical enclosure kept at a constant temperature. Repeated trials show that, when the differential temperature is 65°, the enclosure being maintained at 60°, while the sphere is 125°, the dynamic energy transmitted to the enclosure by a sphere the convex area of which is one square foot amounts to 5·22 thermal units per minute. The accuracy of this determination is confirmed by the fact that, during the summer solstice at noon, when the sun's differential radiant intensity is 65°, the solar calorimeter indicates a dynamic energy of 5·12 units per minute on one square foot of surface.

Our practical investigations, then, show that a differential temperature of 3,000° developes by radiation a dynamic energy of 1,013 thermal units per minute upon an area of one square foot; and that a differential temperature of 65° develops 5·22 units per minute upon an equal area. The ratio of radiant energy at the first mentioned intensity will therefore amount to $\frac{1013}{3000}=0·337$ units for each degree of differential temperature; while for the low intensity it will be $\frac{5·22}{66}=0·080$ unit for each degree of differential temperature. Consequently, the ratio of the radiating energy will be $\frac{0·337}{0·080}=4·21$ times greater at 3,000° than at 65°. Now, M. Vicaire, on the authority of Dulong, states that the ratio will be a hundred fold greater for an increase of only 600°. According to Newton's theory, based on dynamic laws, the proportion between the differential temperature and the radiant energy of bodies is constant; while Dulong and Petit, basing their conclusions upon an erroneous estimate of the time of cooling, assert that the ratio of energy increases several thousand times when the temperature is increased from 65° to 3,000°. Newton, then, as our experiments prove, is incomparably nearer the truth than the French experimenters; and possibly future research will prove that his law, when properly applied, will be found absolutely correct. It should be mentioned that the results of our experiments with the fused metal, compared with the results of other experiments with solid metals at various temperatures, show that the emissive power of cast iron is relatively greater in a state of fusion than when solid, or merely incandescent. This observed increase of emissive power, now being thoroughly investigated, will no doubt account for the deviation from the Newtonian law indicated by the preceding comparison, which, let us recollect, is based upon the difference of radiant energy of fused metal at 3,000°, and solid metal at 65°. Considering this extreme range of temperature, and the totally different conditions of the radiators, the observed discrepancy is not too great to admit of satisfactory explanation.

The fallacy of Dulong's formula relating to high temperatures having been conclusively shown, it will not be necessary to examine the calculations of Messrs. M. E. Vicaire and Sainte-Claire Deville, presented to the Academy of Sciences at Paris. Besides, the question of solar temperature cannot be properly investigated without considering the leading points connected with the propagation of radiant heat through space—a subject of too wide a range to be discussed in this article. It should, however, be mentioned that the result of the measurement of solar intensity March 7, 1872, before referred to, proves the correctness of our previous demonstrations, showing that the temperature of the surface of the sun is at least 4,036,000° F.—*Nature, April* 25.

2. *Aurora of February* 4.—Prof. Gabb writes to one of the editors, from San Domingo, under date of June 18th, that about Feb. 4, and with little doubt on that evening (since it is not probable that there was another there), "We were sitting on the porch, facing the north, when my mother called our attention to a dull red glow on the northern sky extending perhaps 30° high, considerably above the pole star; you know we are in 18° N. At first my impression was that it was the reflection of fires on clouds—some prairie on fire—but there were no clouds; all of the stars shone brightly through it. I cannot say how long it lasted. I saw it occasionally for two hours. We first noticed it between 8 and 9 P. M.—See, for observations in Australia, page 158.

3. *Edinburgh Astronomical Observations.* Vol. XIII, 1860–1870.—A very large and thick volume, containing besides Astronomical Observations of the Royal Observatory of Edinburgh, with the Transit and Mural Circle, and Star Catalogues made during each of the years of the decade 1860 to 1870, occupying 761 pages; also Meteorological tables for Scotland and Scottish towns, accounts of storms, and a long memoir on the Great Pyramid in Egypt, giving measurements of all the Pyramids, and special detailed measurements of the great Pyramid of Jeezeh. The volume closes with the several annual Reports to the Board of Visitors, and a paper on Auroral and other Faint-Light Spectroscopy in 1871. It is illustrated by 56 plates, 37 of which relate to the Pyramids, and two to Faint-Light Spectroscopy.

4. *Astronomical and Meteorological Observations made at the U. S. Naval Observatory during the year* 1869, Commodore B. F. Sands, U. S. N., Superintendent. Published by authority of the Hon. Secretary of the Navy. xliv and 396 pages 4to, with an Appendix of 132, xv and 332 pages. Washington. 1872.—Nearly 400 pages of this Report are devoted to the results of the observations of 1869. Appendix I. contains the Reports on the Solar Eclipse of the year, by Prof. Newcomb, U. S. N., Prof. Asaph Hall, U. S. N., Prof. Wm. Harkness, U. S. N., and Prof. J. R. Eastman, U. S. N., illustrated by several cuts and two plates. Appendix II, running to 332 pages, is devoted to tables pertaining to the catalogue of stars in progress in the observatories with the mural circle. The volume is a record of a great amount of excellent work.

## V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Height of Mt. Rainier and Mt. Baker.*—Officers of the Coast Survey, Prof. Davidson and Mr. Lawson, have determined the height of Mt. Rainier to be 14,444 feet, or 4 feet greater than that of Mt. Shasta. It is situated in latitude 46° 51′ 09″ and longitude 121° 45′ 28″. Prof. Davidson says that there are glaciers on Mt. Rainier. Mt. Baker had previously been found to be 10,760 feet high.—*Proc. Cal. Acad. Sci.*, iv, 1871, 157.

2. *Glaciers on the Mountains of the Pacific Coast.*—Prof. Davidson observes, with respect to the first notice of glaciers on

the Pacific slope of N. America, that Lieutenant (now General) Aug. V. Kautz, U. S. A., attempted to ascend Mt. Rainier in 1856 or 1857, but found his way barred by great glaciers; and that Mr. Coleman, of the Alpine Club, ascended Mt. Baker in 1869, and published that year a description of the glaciers, in Harper's Magazine with illustrations.—*Proc. Cal. Acad. Sci.*, iv, 161, 1871.

3. *Academy of Natural Sciences of Philadelphia.*—With January of 1871, this academy commenced the third series of the Proceedings, *illustrated.* Part III. of 1871 has thirteen excellent plates, part of them colored. One of them contains colored figures of two snakes, *Nothopsis rugosus* Cope, and *Bothrops atrox;* and the rest represent various species of fishes. The charge per year for the illustrated Proceedings is $5.00 to members, and $6.25 to the public. The 2d series of the Proceedings, consisting of 14 volumes, commenced in 1857, and is sold for $42 to members, and $3.75 per volume to the public. The first series of 8 volumes was begun in 1841; price $24 to members, and $30 to the public.

The first series of the Journal of the Academy consists of eight octavo volumes. The new series, in quarto, was commenced in 1847, and seven volumes have been published. The price per volume is $10.

4. *Memorie della Società dei Spettroscopisti Italiani;* edited by Prof. P. Tacchini.—At the close of last year there were no less than five spectroscopes in Italy in the hands of assiduous and skillful observers; two at Rome used by Secchi and Respighi; one at Padua by Lorenzoni; one at Naples under the care of Dr. Gasparis, and one at Palermo by Tacchini. At least one more has since been added, that is at Florence, by Donati

An Italian Society has been formed for the prosecution of spectroscopic observations and their publication, and four numbers of their *Memoirs*, corresponding to the first four months of 1872, have been received. They are published, with extended illustrations, at Palermo, under the supervision of Prof. Tacchini. The following is a list of the articles of the four numbers received:

*January.* 1. New Society of Italian Spectroscopists; account by P. Tacchini. 2. Spectroscopic observations upon the solar limit; by G. Lorenzoni. 3. Tables for converting the angle of position of a point on the sun's limit into corresponding heliographic polar distances; by G. Lorenzoni. 4. New Micrometer for the protuberances; by A. Secchi. (Two plates.)

*February.* Solar protuberances simultaneously observed at Palermo, Rome, and Padua in July and August, 1871; reported by P. Tacchini. (Two plates.)

*March.* 1. Observations on the solar protuberances and their distribution; by A. Secchi. 2. Spectroscopic pictures of sun's limb, made at Palermo, Rome, and Padua, on the 11th and 12th of December, 1871, by Tacchini, Secchi, and Lorenzoni. (One plate).

*April.* 1. On the deviation of the lines of the spectrum due to change of temperature of the prism; by P. Blaserna. 2. Spectroscopic pictures of the sun's limb, made at Palermo and Rome in

August and November, 1871; by P. Tacchini. 3. Spectroscopic observations of solar spots made at Florence; a letter from Prof. Donati to P. Tacchini. (One plate.)

5. *Monthly Record of Results of Observations in Meteorology, Terrestrial Magnetism, etc.* Taken at the Melbourne Observatory, during January, 1872; together with abstracts from meteorological observations obtained at various localities in Victoria. Under the superintendence of R. L. J. Ellery, Government Astronomer. 14 pp. 8vo. Published by authority of her Majesty's government in Victoria.—Besides various meteorological tables, this Report contains the following on the

*Aurora of February*, 1872.—An Aurora Australis was visible shortly after midnight on the 5th until the early morning, coincident with which great magnetic disturbances took place, particularly of the horizontal force, and to a less extent the declination. They commenced at midnight. The maximum disturbance occurred shortly before 3 A. M., when the minimum easterly declination was reached, and continued until 5.15 A. M., when the maximum declination occurred, the extreme range amounting to 1° 18′ of arc; the disturbances then became less violent, and by noon consisted of a succession of slow regular oscillations, gradually getting smaller until toward midnight of the 6th, they ceased. The extreme range in the horizontal force occurred between $1^h$ and $3^h$ A. M., amounting to 0·2940 of the absolute British unit, the maximum occurring at $1^h$ A. M., and the minimum at $3^h$ P. M. The motion of the needle was at times exceedingly rapid, moving once within a few minutes through 0·1737 of the absolute unit.

6. *Hayden's Exploring and Surveying Expedition.*—This expedition, sent out under the authority of the Secretary of the Interior, is again in the Rocky Mountains, continuing its surveys. Prof. F. H. Bradley has joined the expedition as geologist. Congress made an appropriation at its last session of $75,000 for the expenses of the current year.

Professor Bradley says in his letter cited from on page 133, dated July 7th: "Delay has been rather tedious, even in the midst of work; but the rivers have been so full that we could not profitably have gone into the higher mountains earlier, if we had been ready. Dr. Hayden, with his division of the party, is at Fort Ellis, nearly ready to start in. We expect to meet him in the Firehole Basin, in the latter part of August."

7. *Memoirs of the Peabody Academy of Sciences.* Vol. i, No. iii.—*Embryological Studies on Hexapodous Insects;* by ALPHEUS S. PACKARD, Jr. 18 pp., with three beautiful plates.—The species, the development of which Mr. Packard here describes, are *Nematus ventricosus*, *Pulex canis*, *Alletabus Rhois*, *Telephorus Fraxini*, *Chrysomela polygoni*, *Mysia* 13-*punctata* and *Chrysopa oculata*.

8. *Petroleum in San Domingo.*—Mr. A. P. MARVINE writes to one of the editors, in a letter dated Houghton, Michigan, June 16, as follows: "*Apropos* to Mr. Gabb's article 'On the occurrence of Petroleum in the Island of Santo Domingo,' in the June number

of this Journal, I would like to call your attention to some very similar remarks on the same subject in the 'Report of the Commission of Inquiry to Santo Domingo' (pp. 109–110), which was issued by Government about a year ago.

"When at the spot in question I gathered about a quart of the oil from one of the pits, and a small bottle of the gas which bubbles from the well, and still have them well sealed."

9. *American Association.*—The next meeting of the Association will be held at Dubuque, Iowa, instead of San Francisco, commencing August 21st. Dr. J. LAWRENCE SMITH is president for the year.

### OBITUARY.

Dr. WILLIAM STIMPSON.—The news of the death of Dr. William Stimpson, late Secretary of the Academy of Sciences of Chicago, will be received with great regret by his numerous friends and scientific associates. Dr. Stimpson's health has been quite precarious for several years past, making it necessary for him to proceed every winter to the warmer climate of Florida, and the past winter was spent by him in the same region. He was engaged in the earlier part of the season on board the United States Coast Survey steamer *Bache*, in superintending a series of dredgings between Cape San Antonio, Cuba, and the coast of Yucatan; and thence proceeding to Key West, he attempted to prosecute some deep-sea work in the waters between Florida and Cuba. This, however, was prevented by increasing ill health. Returning not long since to the residence of his father-in-law, near Baltimore, he became gradually worse, and died there on the 26th of May.

This is not the occasion for presenting a full account of Dr. Stimpson's life; but of his merits as a naturalist it is proper to say that he occupied the very first rank among American zoölogists, especially in the department of marine invertebrates. For a time a pupil of Professor Agassiz at Cambridge, he made his first mark as a scientific author in 1851, in a work on the shells of New England, which was soon followed by a paper upon the marine invertebrates of Grand Manan, published by the Smithsonian Institution in 1853, and which is still a standard work on the zoölogy of the mouth of the Bay of Fundy.

Shortly afterward he was appointed zoölogist to the North Pacific exploring expedition, first under Captain Cadwallader Ringgold, and subsequently under Captain John Rodgers. He was occupied in this service several years, and then returned and spent a number of years at Washington in the quiet prosecution of his investigations and the publication of their results.

When the late Mr. Robert Kennicott went to Alaska, in 1865, in the service of the Russian telegraph expedition, Dr. Stimpson moved to Chicago to take charge of the general affairs of the Chicago Academy of Sciences, and maintained that connection until his death. During that interval he visited Florida on several occasions, and always obtained numerous interesting collections for the Academy.

As a scientific investigator Dr. Stimpson occupied a very high rank for the thoroughness of his researches, and the clearness and accuracy of his descriptions, in these respects leaving nothing to be desired. No one, with the exception, perhaps, of Prof. Dana, has described so many new species of marine animals as he.

The detailed accounts of his new species, forming a large number of valuable zoölogical monographs, with large numbers of illustrations, and nearly ready for publication, were unfortunately all destroyed by the Chicago fire, together with most of the types of this species—a calamity which of course affected him severely, and in all probability influenced the state of his health. Among these works were synopses of the Mollusca of the east coast of North America, and of the Crustacea of both coasts, to be published by the Smithsonian Institution.—X., in *Harper's Weekly.*

The meeting of the Chicago Academy of June 11th was devoted to addresses in memory of Dr. Stimpson by the President and other members. We cite the following paragraph from the remarks of Mr. E. W. Blatchford:

"I am reminded, Mr. President, that this is the second time in the brief history of our Academy that we have been called upon to mourn the loss of a secretary. They were both men of marked characters, of marked differences. In the scientific world they were typical men—the one of our young West, the other of the maturer East—the one of the undeveloped fields afforded by our lakes, with the territory adjacent, and stretching west and north to the Pacific and Arctic coasts: the other of the more thoroughly investigated Atlantic slope, and foreign field of scientific research. Differing, however, in early opportunities and training, in subsequent associations, and in the fields of their investigations, Kennicott and Stimpson were yet *one*; one in high aims, one in enthusiastic devotion, one in self-forgetfulness, one in persevering and successful labor. And in the faithful performance of duty was each called away. To the one the summons came in the Arctic regions, upon the banks of the Youkan: to the other in the tropics, upon the banks of that wider, deeper stream, the study of whose mysteries had lured him on in spite of pain and weakness. Already are the foundations laid and the walls rising of our new Academy; but the future growth of our collections, and increasing influence, will ever be associated in our minds with our obligations to these men, whose lives have been consecrated to laying here the foundations of scientific truth."

Mr. ROBERT SWIFT, of Philadelphia, died on the 5th of May, in the seventy-seventh year of his age. Mr. Swift from 1852 labored much in West Indian Conchology, and in 1863 published his "Researches of the Virgin Islands." He also contributed, through collections made at his expense on St. Thomas and Porto Rico, to the ornithological collections of the Smithsonian Institution, on which a report was made by Dr. Bryant of Boston.

GEORGE ROBERT GRAY, an eminent British ornithologist, and Assistant Keeper of Zoology in the British Museum, died on the 6th of May last. He was born in July, 1808.

Fig 1

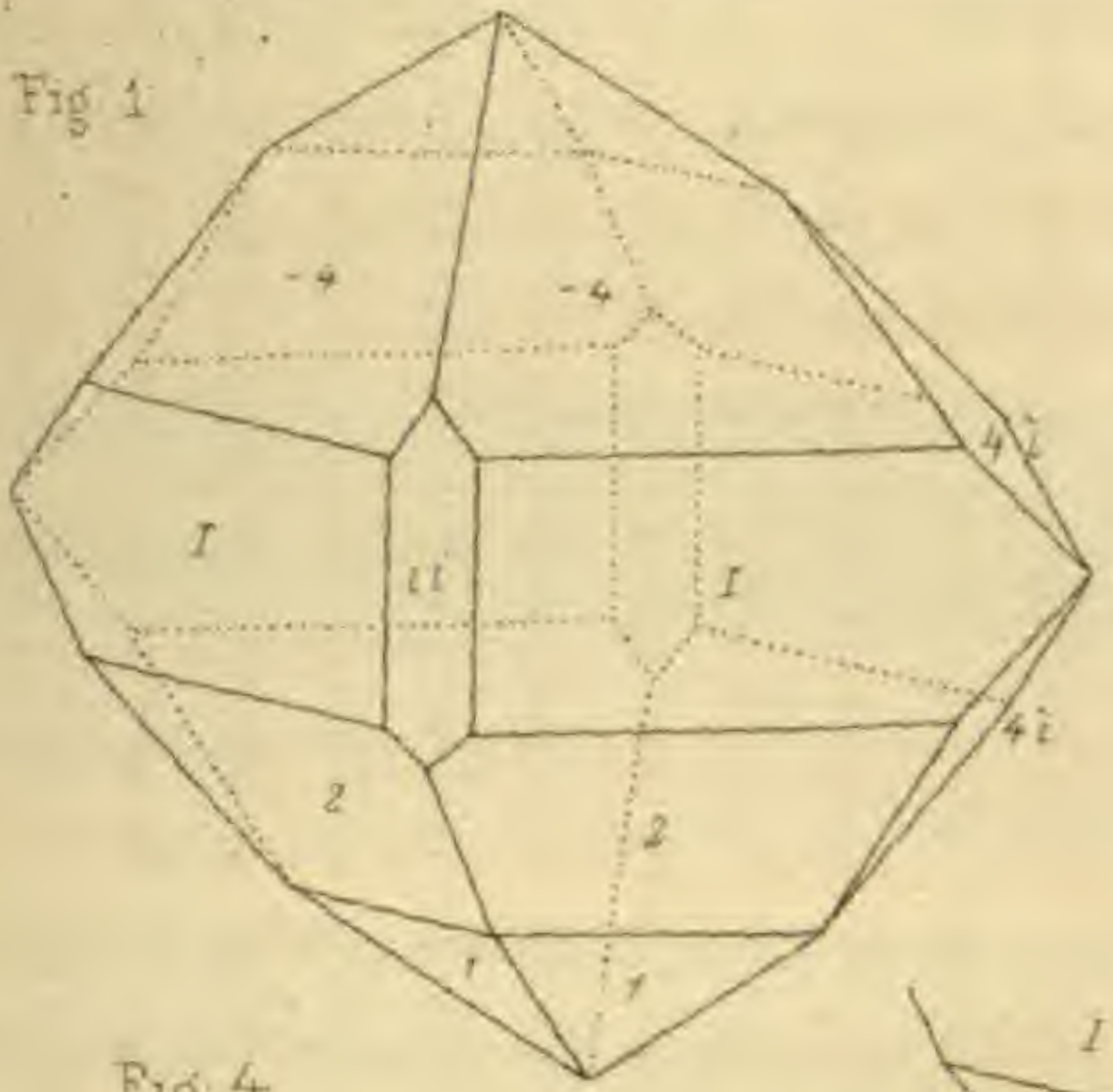

Fig 2

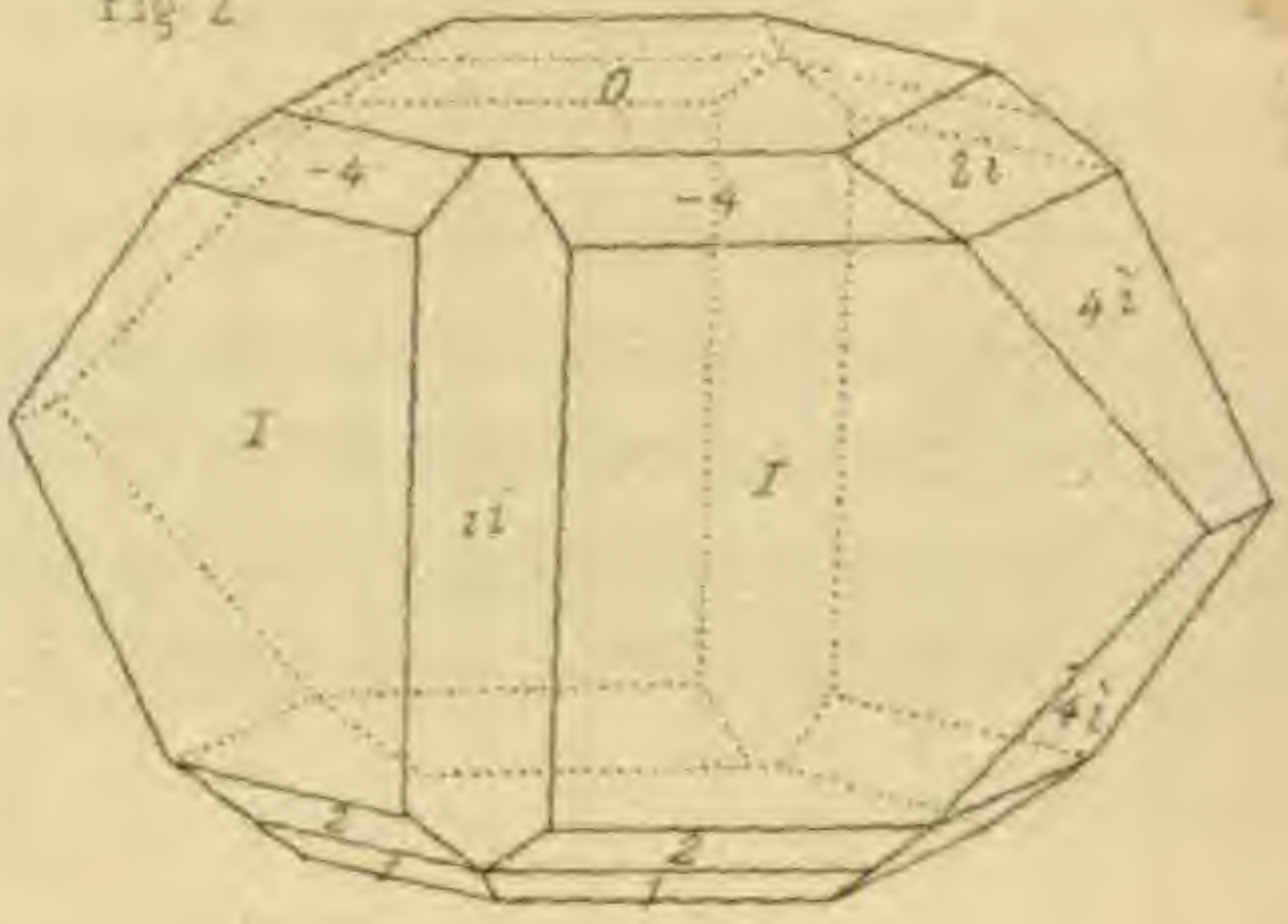

Fig 3

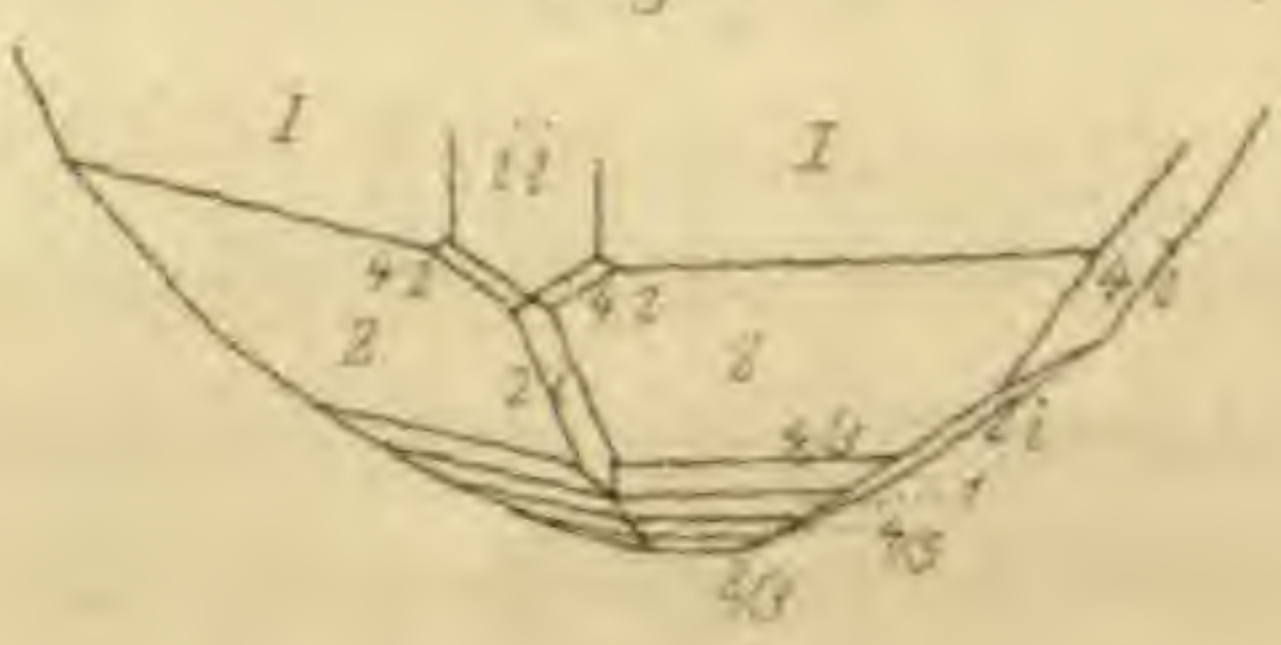

Fig 4

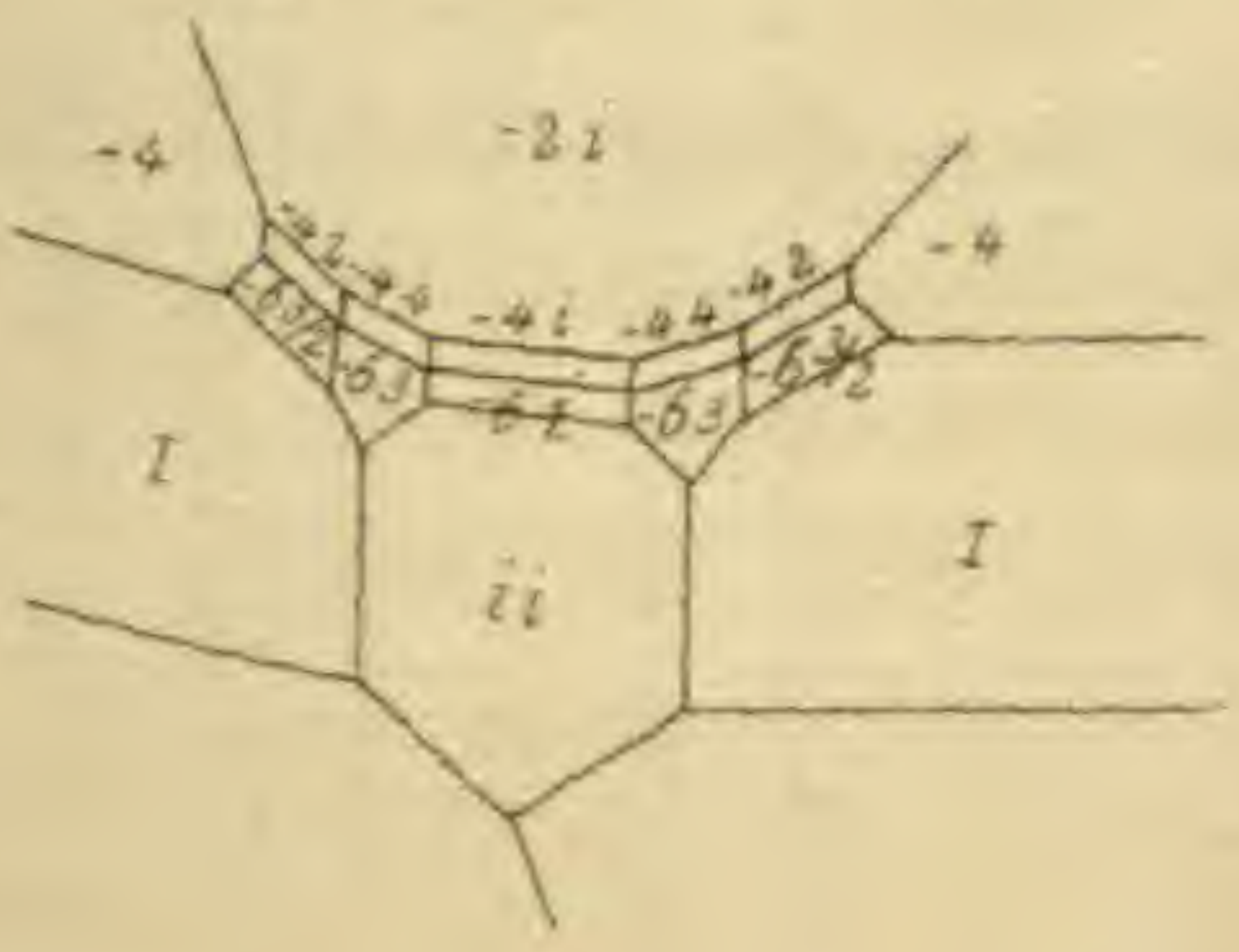

Fig 5

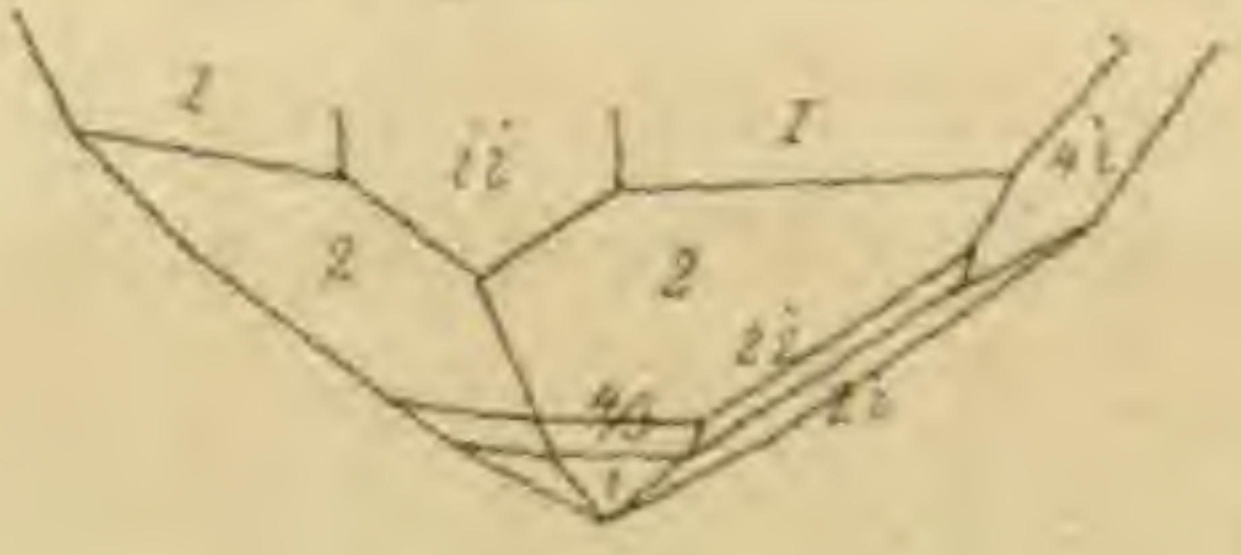

Fig 6

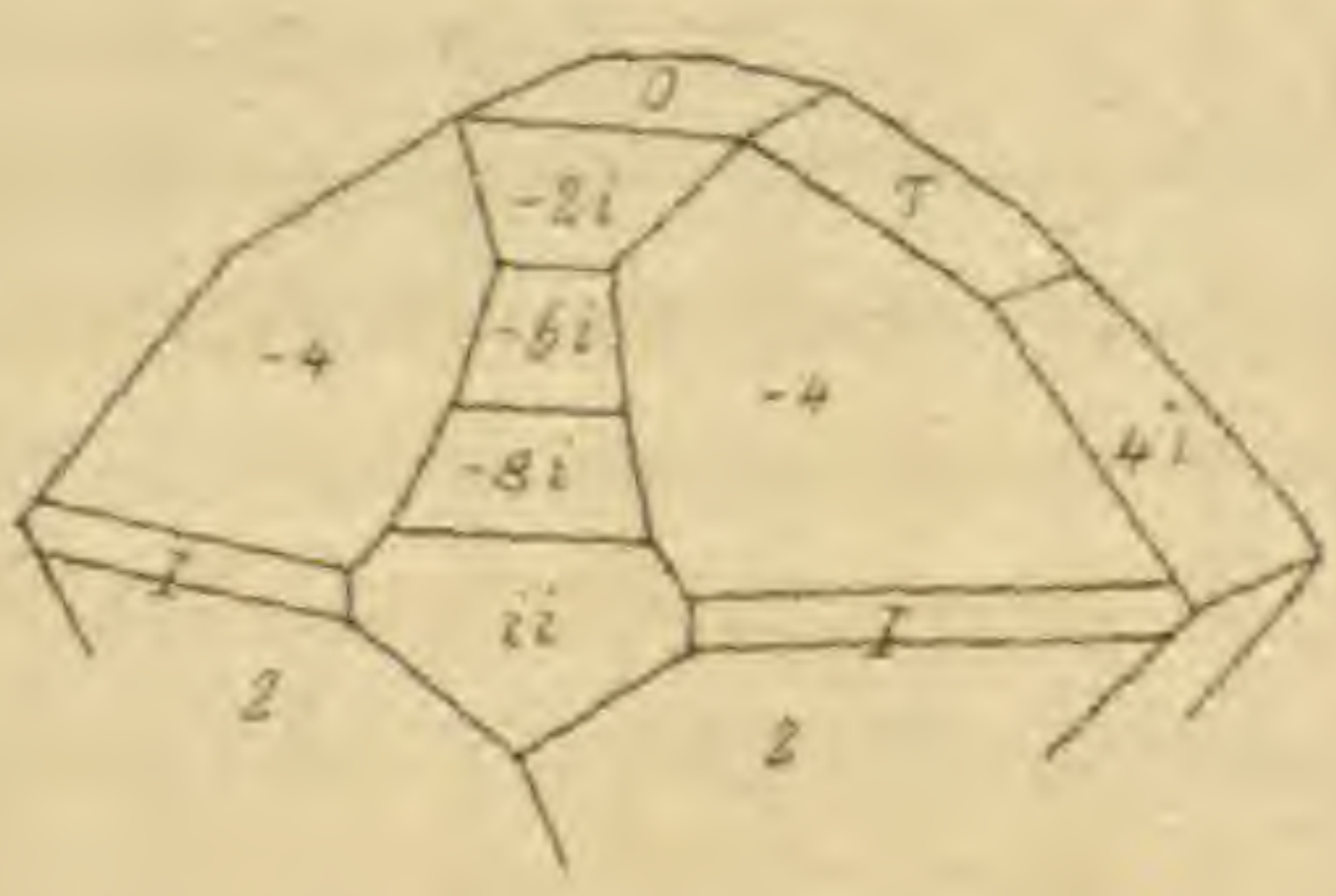

Fig 16

Fig 8

Fig 17

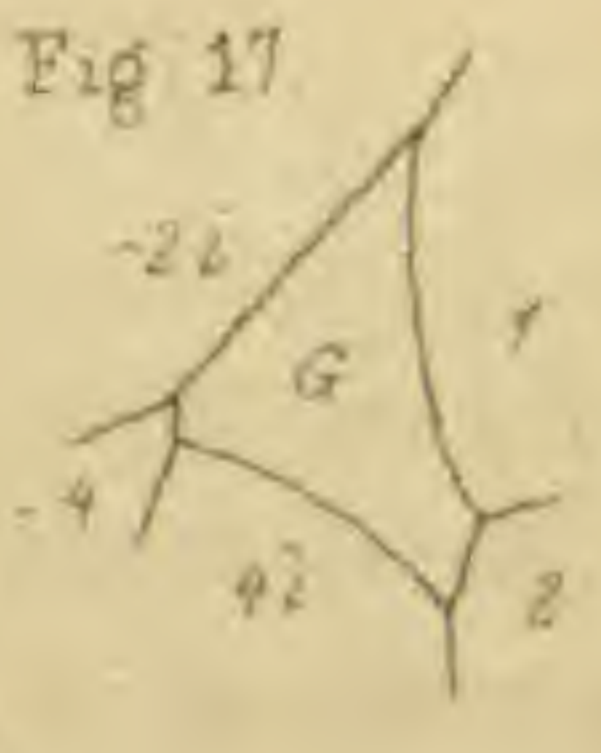

Fig 7

Fig 9

Fig 10.

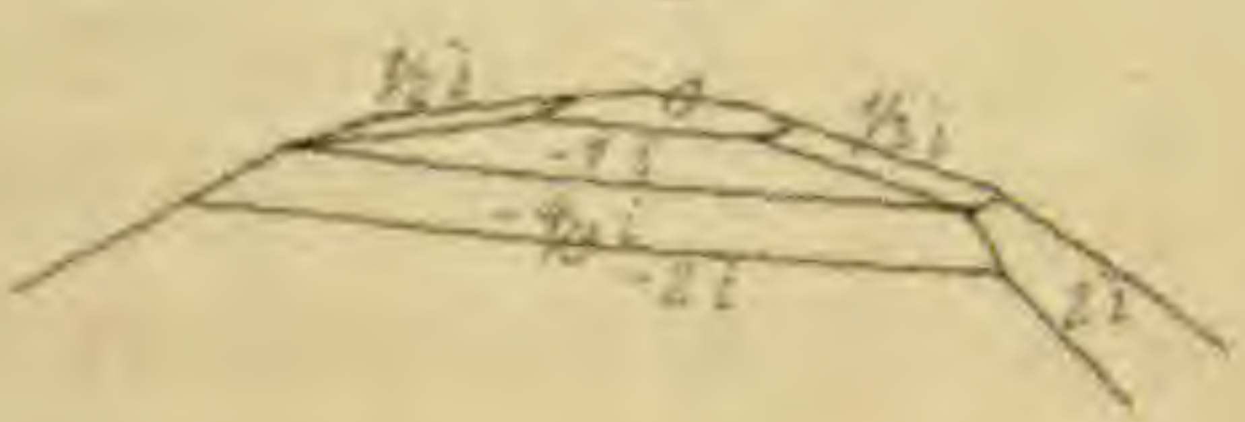

Fig 12

Fig 11.

Fig 13

Fig 14

Fig 15.

Punderson & Crisand, New Haven

DATOLITE OF BERGEN HILL

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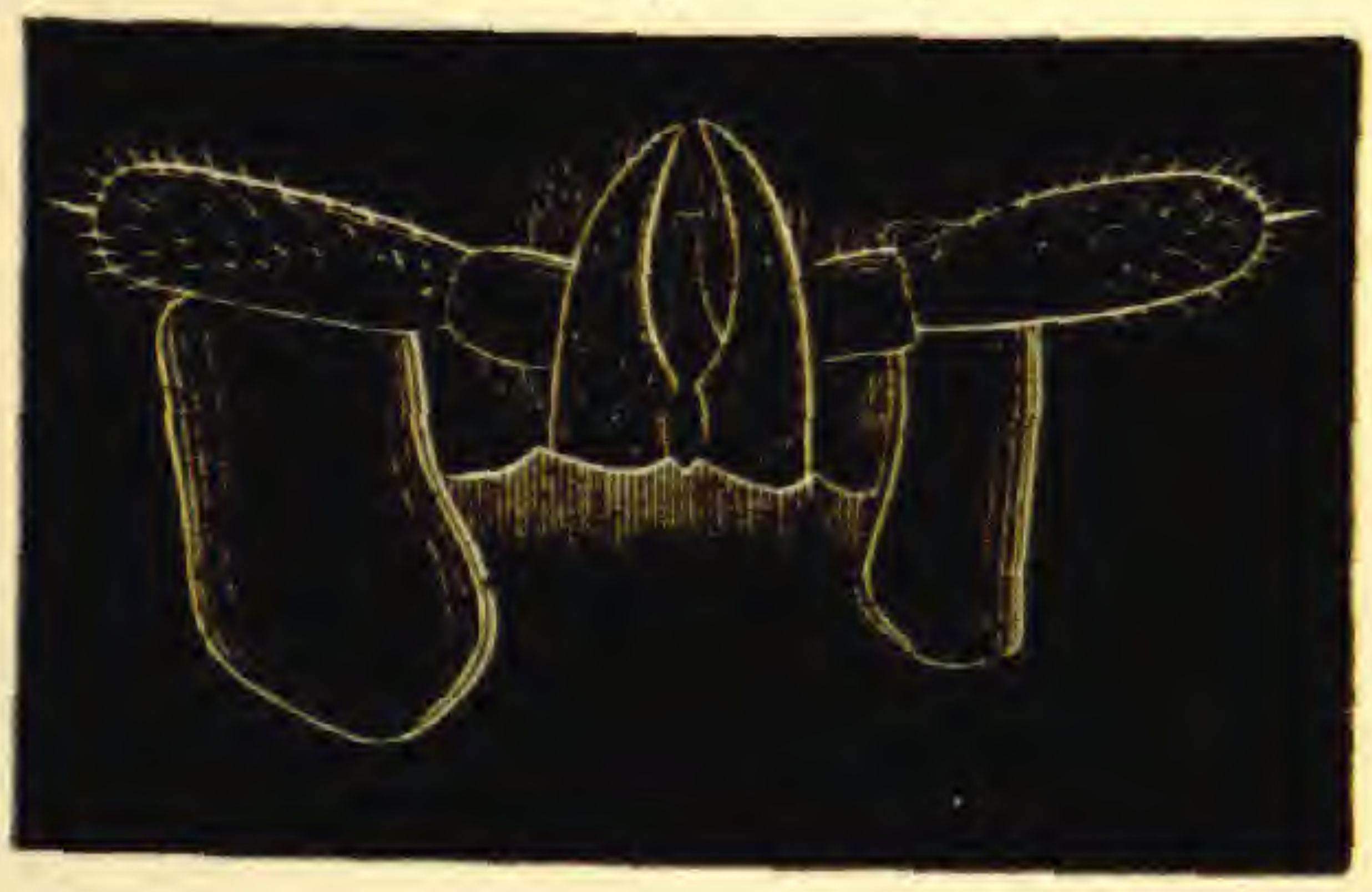

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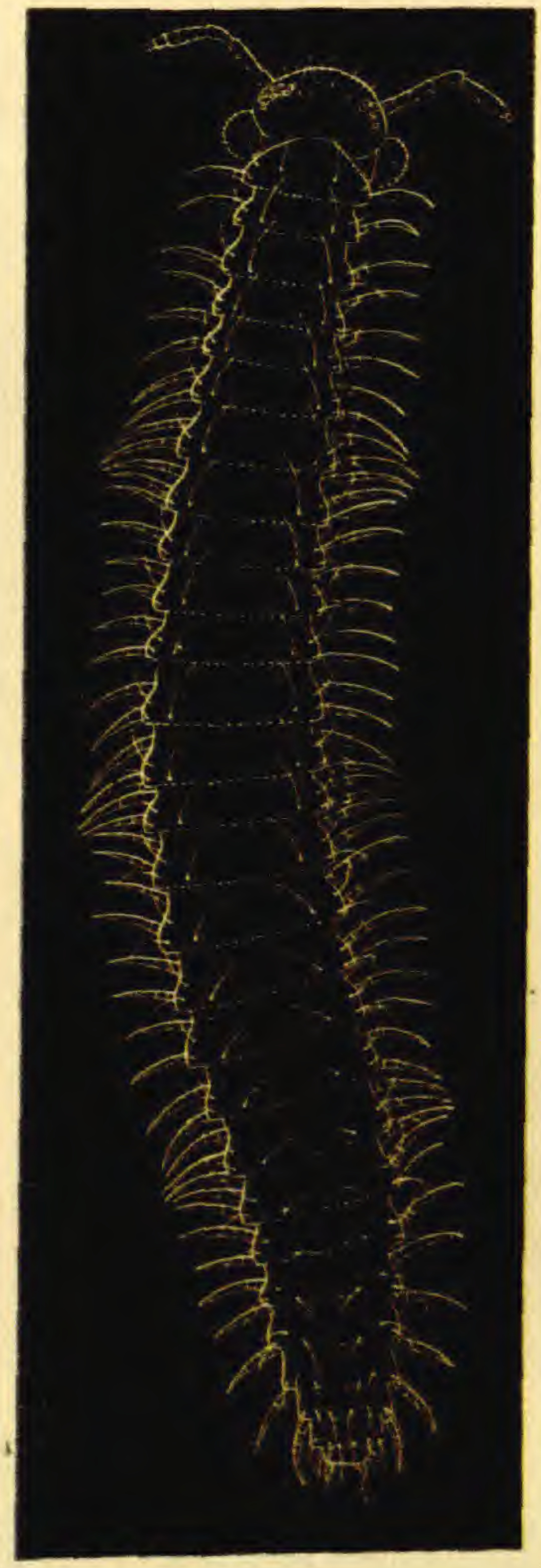

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O. H. from nature and on wood.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

## ART. XXIII.—*Researches in Actino-Chemistry. Memoir First. On the Distribution of Heat in the Spectrum;* by JOHN WILLIAM DRAPER, M.D., LL.D., President of the Faculties of Science and of Medicine in the University of New York.

MANY experimenters at various times have occupied themselves with the problem of the Distribution of Heat in the Spectrum. At first it was supposed that there is a coincidence between the luminous and calorific radiations, and that the maximum of intensity in both occurs at the same point, that is, in the yellow space. This view was abandoned on the publication of the well known experiments of Sir W. Herschel, who showed that in certain cases the maximum is below the red. Subsequently Melloni having discovered the singular heat-transparency of rock-salt, proved that when a prism of that substance is used the maximum in question is as far below the red as the red is below the yellow, but that if the light has passed through flint-glass the maximum approaches the red, if through crown-glass it passes into the red, if through water or alcohol it enters the yellow.

In the case of the sun's spectrum the distribution of heat was more closely examined by Prof. Müller, whose results in a general manner confirmed the views then held, that the invisible radiation below the red greatly exceeds that in the visible spectrum; and still more recently Dr. Tyndall, examining the spectrum of the electric light through rock-salt, showed that the curve indicating the distribution "in the region of the dark rays beneath the red, shoots suddenly upward in a steep and massive peak, a kind of Matterhorn of heat, which quite dwarfs by its magnitude the portion of the diagram representing the visible radiation." These investigations were made under unexceptionable circumstances; the beam of electric light had practically undergone no atmospheric absorption, and the optical refracting train was of rock-salt.

Sir J. Herschel had shown in 1840 that when the sun's rays are dispersed by a flint-glass prism, the distribution of the heat toward the less refrangible region is not continuous, but there are three maximum points. These points, as shown by Dr. Tyndall, do not exist in the spectrum of electric light, the decline of which is continuous; they are therefore to be attributed to the absorptive disturbance which the sun's rays have undergone. Quite recently (1871), M. Lamansky has succeeded in identifying these interruptions by the aid of the thermo-multiplier. In his memoir he states that, with the exception of Foucault and Fizeau, in their well known experiments on the interference of heat, no one has made reference to these lines, and that all experimenters describe the heat-curve as a continuous one (London and Edin. Phil. Mag., April, 1872).

I may therefore be excused for remarking at this point that the three lines in question were not only observed by me nearly thirty years ago, but that an engraving of them was published in the Philosophical Magazine, in a memoir announcing the discovery of fixed lines in the invisible portions of the spectrum (May, 1843). It will be seen, from an inspection of that engraving, that these lines are marked $\alpha$, $\beta$, $\gamma$. They were impressed on daguerreotype plates, by resorting to the well known processes for obtaining photographs of the less refrangible regions of the spectrum.

In view of the preceding statement and others that might be given, it may, I think, be affirmed that the general opinion held at the present day as to the constitution of the spectrum is this, that there exists a heat spectrum in the less refrangible regions, a light spectrum in the intermediate, and a spectrum producing chemical action in the more refrangible regions. An experimental attempt to correct this view, and to introduce a more accurate interpretation, will not be without interest, especially as it is necessarily and directly connected with the important subject of photometry. In this memoir I shall offer some experiments and suggestions respecting the heat of the spectrum, and in another, shortly to be published, shall consider the distribution of the so-called chemical rays. Among the numerous problems of actino-chemistry there are none more important than these.

All the experiments hitherto made on the heat of the spectrum have been conducted on the principle of exposing a thermometer in the differently colored spaces. Such was Sir W.

Herschel's method. Leslie used a differential with small bulbs. Melloni, Müller, Tyndall, a thermo-electric pile, the form preferred being the linear. This was advanced successively through all the radiations, and the deflections of the multiplier noted.

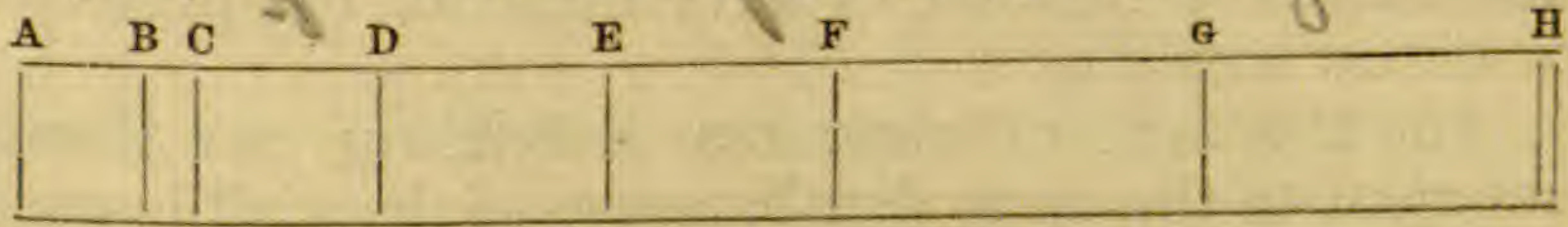
Is not this method essentially defective? Does it not necessarily lead to incorrect results?

"There is an inherent defect in the prismatic spectrum—a defect originating in the very cause which gives rise to that spectrum itself—unequal refrangibility. Of two groups of rays compared together, one taken in the red, the other in the violet region, it is clear that in the same spectrum, from the very circumstance of their greater refrangibility, those in the violet will be relatively more separated from each other than those in the red. The result of this increased separation in the more refrangible regions is to give an apparent dilatation to them, while the less refrangible are concentrated. The relative position of the colors must also vary: the fixed lines must be placed at distances greater than their true distances as the violet end is approached." I am quoting from the 5th chapter of a work "On the Forces which produce the Organization of Plants," published by me in 1844. In this chapter one of the chief points insisted on is the necessity of using wave-lengths in the measurement and discussion of spectrum results, a suggestion which I believe I was the first to make, and which I renewed in a memoir in the Philosophical Magazine (June, 1845).

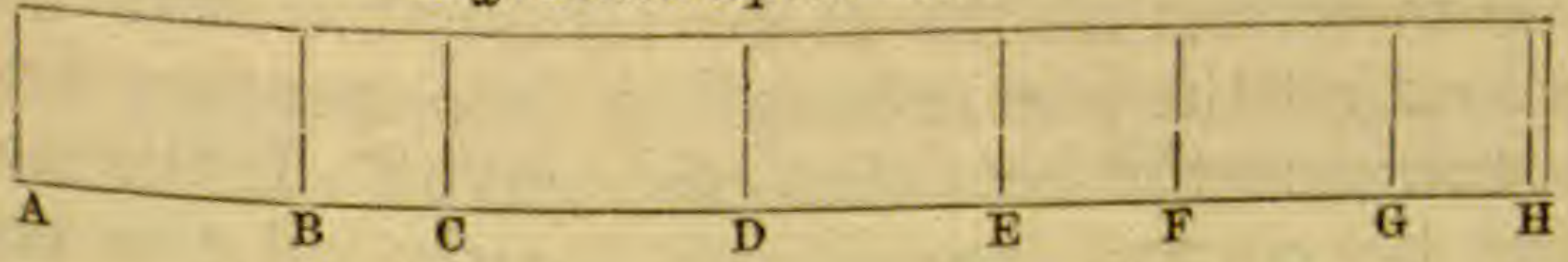
The importance of these remarks respecting the peculiarities of the prismatic or dispersion spectrum, may perhaps be most satisfactorily recognized on examining such a spectrum by the side of a diffraction or interference one. By the aid of fig. 1 this may be done.

FIG. 1.

*Prismatic or Dispersion Spectrum.*



*Diffraction Spectrum.*



Regarding the space between the fixed lines D and E as representing the central region, in each the fixed lines D and E are made coincident. The other lines are laid off in the prismatic as they appear through the flint-glass prism of the spectroscope; those of the diffraction are arranged according to their wave-lengths. It thus appears that in the prismatic, from the fixed line D to A, the yellow, orange, and red regions occupy but little more than half the space they do in the diffraction; while the green, blue, indigo, and violet, from the fixed line E to H, occupy nearly double the space in the prismatic that they do in the diffraction spectrum. The general result is that in the prismatic the less refrangible regions are much compressed, and the more refrangible much dilated. And it is plain that the same will hold good in a still greater degree for any invisible rays that are below the red and above the violet respectively.

Now if a thermometer of any kind were carried in succession from the greatly dilated, more refrangible regions to the greatly condensed, less refrangible, could the measures obtained be accepted as expressing the true distribution? The thermometric surface being invariable, would it not receive in the less refrangible spaces more than its proper amount of heat, and in the more refrangible less than its proper amount?

If we should admit that the distribution of heat in a correctly formed spectrum is uniform, it is plain that measures made by the use of a prism would not substantiate that admission. The concentration to which I have alluded as taking place in the less refrangible region would give an exaggerated, an increased heat for that region; and on the contrary, the dilatation of the more refrangible would give an exaggerated diminution of heat for that space. But if it were possible to make satisfactory heat measures on the diffraction spectrum, in which the colored spaces and fixed lines are arranged according to their wave-lengths, the admission would be substantiated.

In view of these facts I did attempt many years ago to make heat measures on the diffraction spectrum. But so small is the heat that, as may be seen in the Philosophical Magazine (March, 1857), the results were unsatisfactory. More recently I have tried another method of investigation, on principles which I will now explain.

For the sake of clearness, restricting our thoughts for the moment to the more familiar case of the visible spectrum, if we desired to ascertain the true distribution of heat, would not the proper method be to collect all the more refrangible rays into one focal group, and all the less refrangible into another focal group, and then measure the heat that each gave? If the view currently received be correct, would not nearly all the heat observed be found in the latter of these foci, and little, if indeed any, be found in the former? But if all the various regions of the spectrum possess equal heat-giving powers, would not the heat in each of these foci be the same?

Let us give greater precision to this idea. Using Angström's wave-lengths—the length at the line A is 7604, and that at $H^2$ 3933, and these lines are not very far from the less and more refrangible ends of the *visible* spectrum respectively. The middle point of this spectrum is at 5768, which may therefore be called its optical center. This is a little beyond the sodium line D, which is 5892. Now if by suitable means we reunite all the rays between 7604 and 5768 into one focus, and all the rays between 5768 and 3933 into another focus, are we not in a position to determine the true distribution of the heat? Should the heat at these two foci be sensibly the same, must not the conclusion at present held be abandoned?

If in these investigations the rays of the sun be used, it is necessary to restrict the examination to the visible spectrum, excluding the invisible red and invisible violet radiations. On these the earth's atmosphere exerts not only a very powerful but a very variable action, and what is still more, an action the result of which we cannot see, so that we are literally working in the dark. There are days on which, owing to the excessive absorption taking place among the ultra-red rays, a rock-salt train has no advantage over one of glass. But if it be the visible spectrum alone that we are using, and the prisms are of a material *colorless* to the eye, we may be certain that they are exerting no elective absorption on any of the radiations of that spectrum, and that the indications they are giving are reliable.

This variable absorptive action of the atmosphere depends partly on changes in the amount of water vapor, and partly on the altitude of the sun. At midday and at midsummer it is at a minimum. The disturbance is not merely a thermochrose, for both ends of the spectrum are attacked. It is a matter of common observation that the horizontal sun has but little photographic power, owing to atmospheric absorption of the ultra-violet rays, and under the same circumstances his heating power is diminished, owing to the absorption of the ultra-red rays. But if the day be clear and the sun's altitude sufficient, the visible spectrum may be considered as unaffected.

It should be borne in mind that the envelopes of the sun himself exert an absorptive action, which is powerfully felt in the ultra-violet region, as is indicated by the numerous fixed lines crowded together in that region. The force of this remark will be appreciated on examining the plate above referred to, in the Philosophical Magazine for May, 1843.

It seems then that all the conditions necessary for the solution of this problem will be closely approached if we make use of prisms constituted of any substance which is *completely colorless to the eye*, and confine our measures to the *visible spectrum*, collecting all the radiations between the fixed line A and the center of that spectrum just beyond D into one focus, and all the radiations between that center and $H^2$ into another focus, and by the thermopile or any other suitable means measuring the heat of these foci.

Such is the method I have followed in obtaining the measures now to be presented: but before giving them there are certain preparatory facts which I wish to submit to the consideration of the reader.

(1.) In the mode of experiment hitherto adopted, no special care has been taken to ascertain with accuracy the position of the "extreme red," yet that is held to be the point from which on one side we are to estimate the invisible and on the other the visible spectrum. Different persons, perhaps because of a different sensitiveness of their eyes, will estimate that position differently. The red light shades off gradually—it is almost impossible to tell when it really comes to an end. A linear thermopile, such as is commonly used, is liable under these circumstances to give deceptive results, and any error in its indications counts in a double manner. It not only diminishes the value of one spectrum, but it adds that diminution to the value of the other. The force of this remark will be understood by considering the best experiments hitherto made on this subject, those of Dr. Tyndall, as related in his "Heat a Mode of Motion" (London edition, 1870, p. 420, &c.). In the case of the electric light, the result yielded by those experiments was that the heat in the invisible is eight times that of the visible region. But had there been an error in estimating the position of the extreme red by only two millimetres, so much would have been taken from the invisible and added to the visible, that they would have been brought to equality, and then the slightest turn of the screw that carried the pile toward the dark space would have given a preponderance to the visible. It is obvious, therefore, that there cannot be certainty in such measures, unless the fixed lines are resorted to as standard points.

(2.) A ray which has passed through a solution of sulphate of copper and ammonia possesses no insignificant heating power. I took a stratum of a solution of that salt, of such strength that it only permitted waves to pass which are of less length than 4860. Seen in the spectroscope, the colors transmitted through it commenced with a thin green fringe, followed by blue, indigo, violet. It therefore gave rays in which, according to the accepted views, little or no heat should be detected. Yet I found that such rays produced one-ninth of the heat of the solar beam. Does not this indisputably show that the more refrangible rays have a higher calorific power than is commonly imputed to them?

(3.) Again, by the use of the apparatus presently to be described, I found no difficulty in recognizing heat in the violet region. But in the mode of conducting the experiment heretofore resorted to, it could not be detected in rays more refrangible than the blue. It was this result which gave so much weight to the conclusion, that in the more refrangible regions the calorific power is replaced by chemical force, and strengthened the idea commonly entertained that the solar radiations consist of three distinct principles, heat, light and actinism. In the memoir above referred to, as soon to be published, I shall present some facts which apparently make this view indefensible.

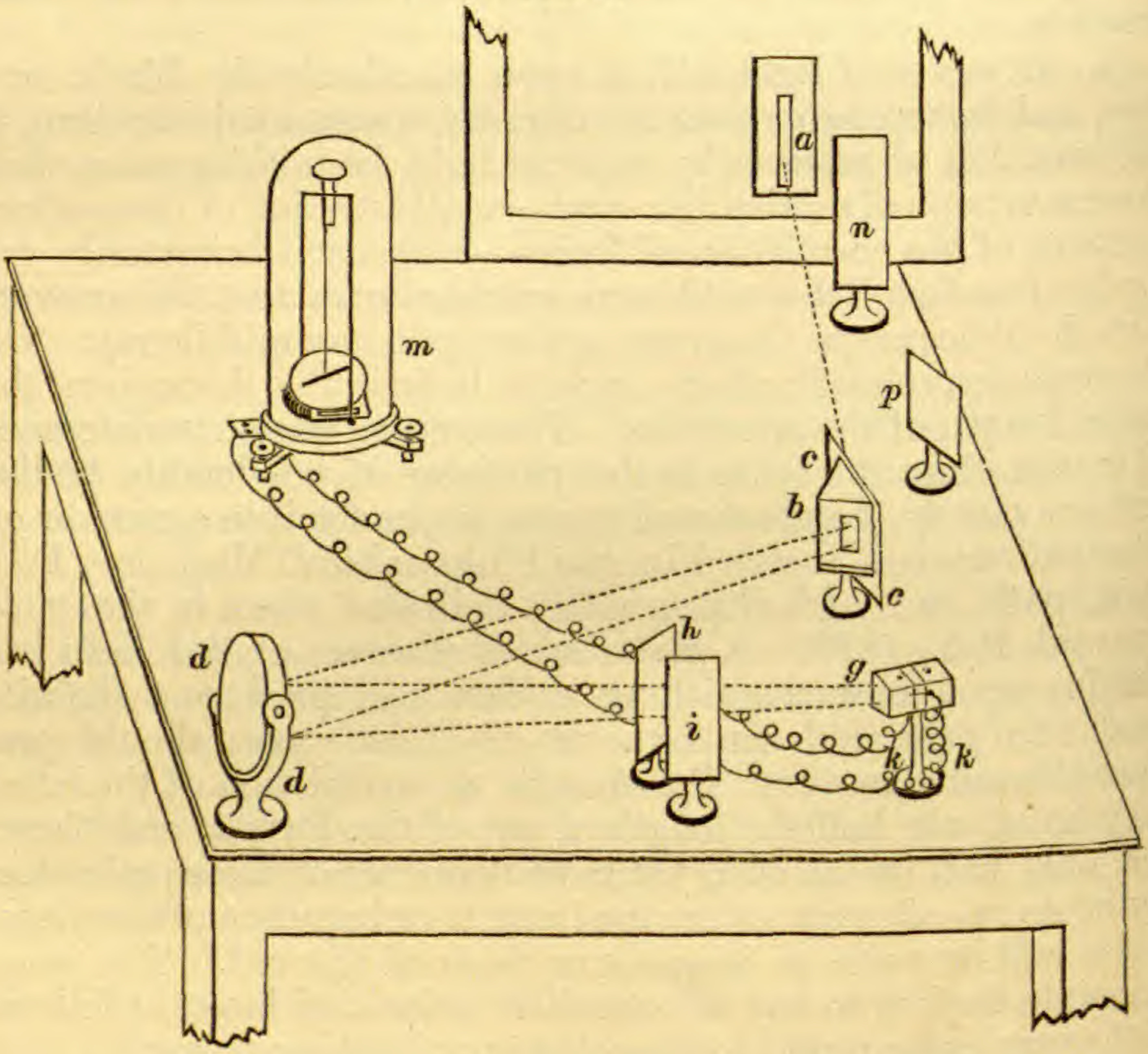
(4.) If waves of light falling upon an absolutely black surface, and becoming extinct thereby, are transmuted into heat, if the warming of surfaces by incident light be nothing more than the conversion of motion into heat—an illustration of the modern doctrine of the correlation of forces—heat itself being only "a mode of motion," it would seem extraordinary that the conversion should cease in the green or blue or in any middle ray. On the contrary, calorific effects ought to be traceable throughout the entire length of the spectrum. These views on the transference of motion from the ether to the particles of ponderable bodies and conversely, I endeavored to explain in detail in a memoir on Phosphorescence, inserted in the Philosophical Magazine, Feb. 1851, p. 98, etc. I had previously indicated them in the same Journal, Feb., 1847. A given series of waves of red light impinging upon an extinguishing surface will produce a definite amount of heat, and similar series of violet waves should produce the same amount. For though an undulation of the latter may have only half the length of one of the former, and therefore only half its *vis viva*, yet in consequence of the equal velocity of waves of every color, the impacts or impulses of the violet series will be twice as frequent as those of the red. The same principle applies to any intermediate color, and hence it follows that every color ought to have the same heating power.

### *Description of the Apparatus employed.*

The optical arrangement I have employed for carrying the foregoing suggestions into practice is represented by fig. 2, and in a horizontal section by fig. 3.

A ray of sunlight reflected by a Silberman's heliostat comes into a dark room through a slit *a* one millimeter wide. It then passes through a prism *b*. On the front face of this prism is a black paper screen *c c*, having a rectangular opening, just sufficient to permit the light of the slit to pass. After refraction the dispersed rays fall as a spectrum on a concave metallic mirror *d d*, nine inches in aperture and eleven in focus for parallel rays. I have sometimes used one of speculum metal, but more frequently one silvered on its front face. In front of this mirror there are therefore three foci. At a distance of eleven inches there is one, *e*, fig. 3, giving a spectrum image of the sun. Still further there is a second, *f*, which is a spectrum image of the slit *a*, in which, if the prism be at its angle of minimum deviation, and the other adjustments be correctly made, will be seen the Fraunhofer lines. Again, still farther off, at *g*, is a focal image of the rectangular opening of the black paper *c c*, on the front face of the prism. This image, arising from the recombination of all the dispersed rays, is consequently white. The second and third foci are at distances from the mirror depending on the distance of the slit *a*, and the black paper *c c*, respectively.

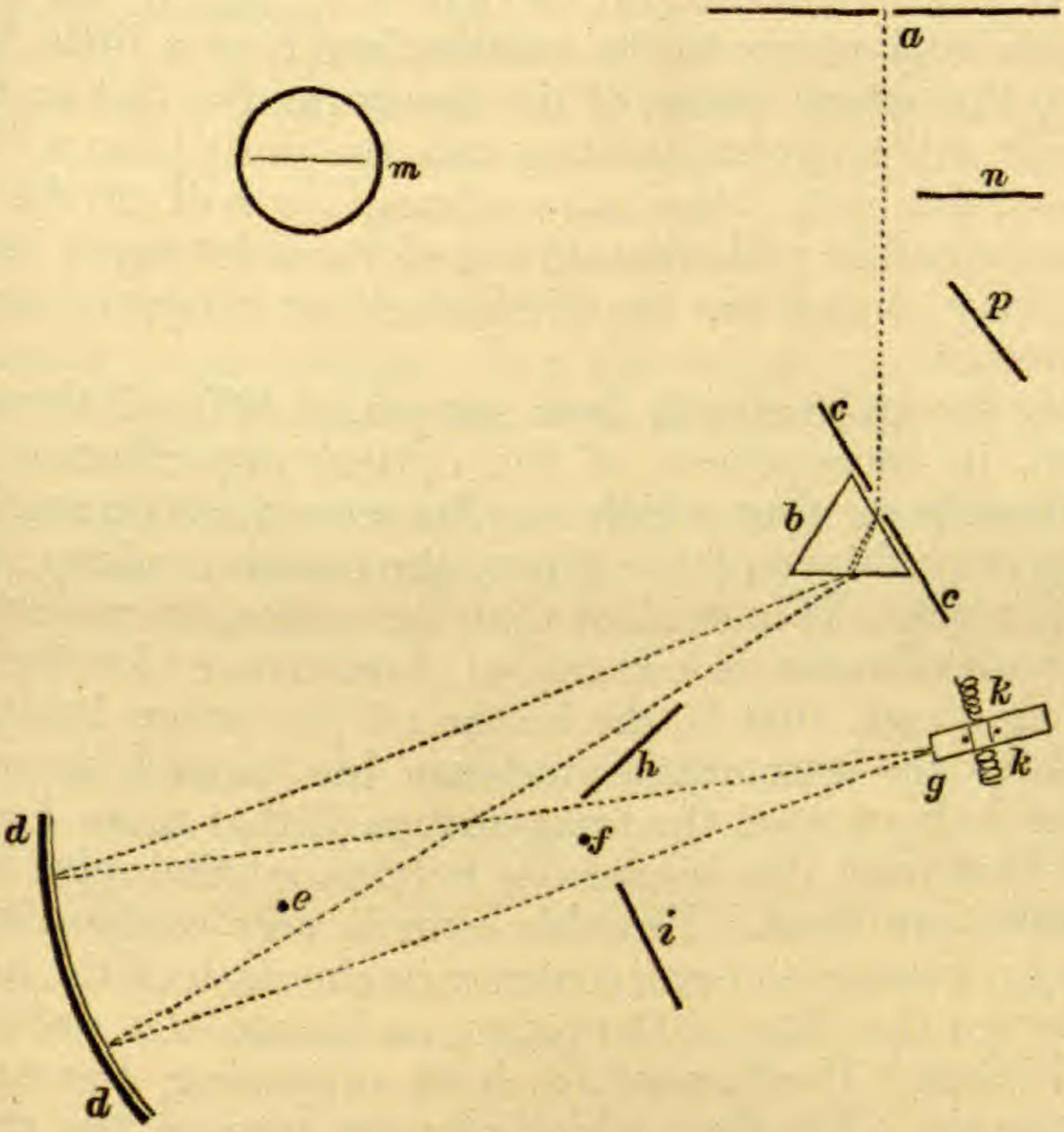
FIG. 2.



With the intention of being certain that the light coming through the slit *a* is falling properly on the rectangular opening in the prism screen *c c*, a small looking-glass is placed at *p*. The experimenter, sitting near the multiplier *m*, can then see distinctly the reflected image of that opening.

At the place where the second focal image with its Fraunhofer lines forms, two screens of white paste-board, *h*, *i*, are arranged. By suitably placing the former of these, *h*, the more refrangible rays may be intercepted, and in like manner by the other, *i*, the less refrangible. In using these screens, and particularly *h*, care must be taken that no rays passing from the prism to the mirror be obstructed, a remark that applies especially to the invisible rays of less refrangibility than the red. For this reason the mirror *d d* must be placed at such an obliquity to its incident rays as to throw the focal images sufficiently on one side. Yet this obliquity must not be greater than is actually necessary for that purpose, or the purity of the second spectrum, with its Fraunhofer lines, will be interfered with.

FIG. 3.



At the place of the third focus, arising from the reunion of the dispersed rays, is the thermopile *g*, connected by its wires *k k* with the multiplier *m*.

Whenever any of the visible rays of the Fraunhofer spectrum are intercepted by advancing either of the screens *h*, *i*, the image on the face of the pile ceases to be white. It becomes of a superb tint, answering to the combination of the non-intercepted rays. A slip of white paper placed for a moment in front of the pile will satisfy the experimenter how magnificent these colors are. It is evident, therefore, that by this arrangement the pile will enable us to measure the heat of any particular ray, or of any selected combination of rays. The screens can be arranged so us tó reach any designated Fraunhofer line.

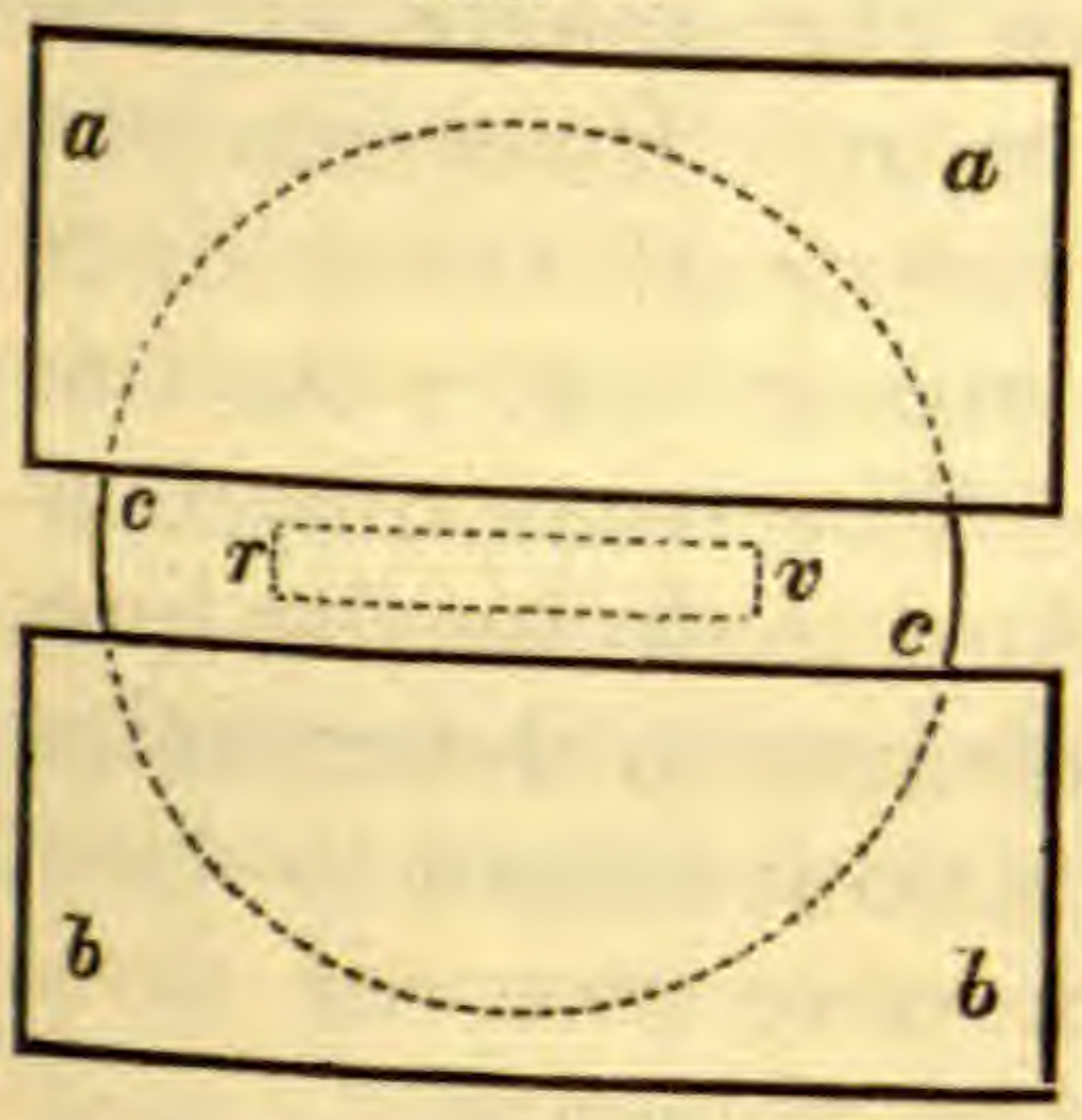
The pile I have used is of the common square form; a linear pile would not answer. The focal image on the pile is of very much greater width than the slit *a*, on account of the obliquity of the front face of the prism.

By removing the screen *h*, and placing the screen *i* so that its edge coincides with the line A of the Fraunhofer spectrum, all the invisible heat radiations of less refrangibility than the red are cut off, except the contaminating ones arising from the general diffusion of light by the substance of the prism. Under these circumstances the image on the pile will be white, and the multiplier will give a deflection representing the heat of the visible and the extra violet regions. If then the screen be advanced still further, until it has intercepted all the less refrangible regions up to the sodium line D or a little beyond, that is, to the optical center of the spectrum, the tint on the face of the pile will be greenish-blue, and the multiplier will give a measure of the heat of the more refrangible half of the visible spectrum, together with that of the ultra-violet rays; the latter portion may, however, be eliminated by properly using the other screen *h*.

Besides the error arising from stray heat diffused through the spectrum, in consequence of the optical imperfection of the prism, there is another which may be recognised on recollecting the relative positions of the prism, the concave mirror, and the face of the pile. It is evident that the prism, considered as a warm or a cool mass, is a source of disturbance, for the mirror reflects its image, that is, the image of the prism itself, to the pile. After the intromitted sunbeam has passed through the prism for a short time, the temperature of that mass has risen, and the heat from this source has become intermingled with the proper spectrum heat. But this error is very easily eliminated. It is only necessary to put a screen *n* in the path of the incoming ray, between the slit and the prism, and note the deflection of the multiplier. Used as we are here supposing, the multiplier has two zeros. The first, which may be termed the magnetic, is the position in which the needles will stand when no current is passing through the coil. The scale of the instrument should be set to this. The other, which may be termed the working zero, is formed by coupling the pile and the multiplier together, and introducing the screen *n* between the intromitting slit and the prism. On doing this it will probably be found that the index will deviate a few divisions. Its position should be accurately marked at the beginning and close of each set of measures, and the proper correction for them made. The disturbing influences of the mass of the prism, of the mirror, and of the pile itself, are thus eliminated. As respects the last, it should not be forgotten that it may be affected by changes in the position of the person of the experimenter himself.

With the intention of diminishing these errors, I have usually covered the upper and lower portions of the concave mirror *d d* with pieces of black paper, so arranged as to leave a band of sufficient width to receive and reflect the entire spectrum. *a a*, fig. 4, is the upper paper, *b b* the lower, *c c* the uncovered reflecting band, receiving the spectrum *r v*. Had the spaces thus covered been permitted to reflect, they would have rendered more intense the image of the prism with its extraneous heat.

Fig. 4.



As regards the multiplier, care must be taken to avoid disturbance from aerial currents. I have one of these instruments of French construction, which could not be used in these delicate researches until proper arrangements were applied. It was covered with a glass shade. The slightest cause occasioned currents in its included air, which perpetually drifted and disturbed the needles. For this reason, and also for more accurate reading, it is best to view the position of the index through a small telescope.

The combination of needles being nearly astatic, attention must be paid to their magnetic perturbations, whether arising from local or other causes; and, since the vibrations are very slow, ample time must be given before the reading is ascertained.

The condition of the face of the pile is of importance. It must be such as to extinguish as completely as possible all the incident rays. To paint it with lamp-black, mixed with gum, will not answer—the surface so produced is too glossy and reflecting. The plan I have found best is to take a glass tube half an inch in diameter and six inches long, open at both ends. and use it as a chimney. A piece of camphor being set on fire at the lower end, and the face of the pile to be blackened being held for a moment at the upper, it is covered with a dense black film, without any risk of injury to the pile. Even at the best, when this has been done, there is an unavoidable source of error in the want of perfect blackness of the lamp-black. It is sufficient to inspect the face of the pile when receiving rays from the concave mirror to be satisfied how large a portion of light is reflected. The experiments of Dr. Tyndall show that this substance also transmits a considerable percentage of the heat falling on it. Its quality of transmitting light is well known to every one who has looked at the sun through a smoked glass.

The galvanometer I have used is calibrated according to the usual method. The numbers given in this memoir do not represent the angles of deflection, but their corresponding forces.

The proper position of the intercepting screens *h*, *i*, can often be verified with precision by looking through blue cobalt glass. This glass insulates a definite red, an orange, and a yellow ray in the less refrangible regions, and then commencing with the green, gives a continuous band to the end of the violet. Its red ray begins at the less refrangible end of the spectrum, and ends near C; it includes the fixed lines A, B, C. Its orange ray lies wholly between C and D, including neither of those lines. Its yellow ray begins near 5894, and ends about 5581; the line D is therefore near its point of beginning. The remaining continuous band begins about 5425; it therefore includes the lines E, *b*, F, G, H. I have found this glass of much use in determining how far the screen *i* has been pushed. It is convenient to select a light kind of it, and by looking through one, two, or three pieces, the depth of color can be regulated at pleasure.

The optical train which has acted on the sunbeam under examination is therefore (1) the sun's atmosphere; (2) the earth's atmosphere; (3) the heliostat mirror of speculum metal; (4) the prism; (5) the concave mirror of silvered glass; (6) the blackened face of the thermopile.

### *Results obtained by the Apparatus.*

We are now ready to examine the results which this optical apparatus yields, it having been of course previously ascertained that the reflecting band of the concave mirror *d d* is sufficient to receive all the radiations coming from the prism, and that none are escaping past its edges.

The operations required are as follows:

The heliostat is to be set, and its reflected ray brought into the proper position. The optical train is adjusted, the prism being at its minimum deviation, and the concave mirror giving a white image on the face of the pile.

The screen *h* is then to be placed so that, without intercepting any rays coming from the prism to the mirror, it cuts off all the Fraunhofer spectrum above $H^2$.

The screen *i* is so placed as to cut off all rays less refrangible than the sodium line D. More correctly, the screen should be a little beyond D. The light on the face of the pile will now be greenish-blue.

The screen *n* is then placed so as to intercept the intromitted beam. When the needles of the multiplier come to rest they give the working zero, which must be noted.

The intromitting screen *n* being now removed, the multiplier will indicate all the heat of the more refrangible rays, that is from a little beyond D up to $H^2$. The force corrected for the working zero is to be noted.

The screen *i* is then removed to the line A, so as to give all the radiations between the lines A and $H^2$. The light on the face of the pile is white, and the multiplier gives the whole heat of the visible spectrum. By subtracting the foregoing measure from this, we have the heat of the less refrangible region, that is from A to the centre of the spectrum.

As a matter of curiosity, the experimenter may now, if he pleases, remove the screens *h*, *i*; the light on the face of the pile will still be white, and the multiplier will give the force of the entire radiations, except so far as they are disturbed by the thermochrose of the media. These measures, as not bearing upon the problem under consideration, I do not give in the following tables.

Instead of advancing the screen *i* from the less toward the more refrangible regions, I have very frequently moved *h* from the more refrangible to the less. When it is brought down from $H^2$ to the centre of the spectrum, the light on the face of the pile is of an intense orange-red—it might perhaps be called a bromine-red. I need not give further details of this mode of experimentation, as I did not find that its results differed in any important degree from those obtained as just described.

The variation in different experiments may generally be traced to errors in placing the screen *i* with exactness on the centre of the spectrum and on the line A.

For the sake of more convenient comparison, I have reduced all the different sets of experiments to the standard of 100 for the whole visible spectrum.

I have made use of four prisms: (1) rock salt; (2) flint glass; (3) bisulphide of carbon; (4) quartz, cut out of the crystal so as to give a single image.

All the observations here recorded were made on days when there was a cloudless sky.

TABLE I.—*Distribution of heat by rock-salt.*

| | Series I. | Series II. |
|---|---|---|
| (1) Heat of the whole visible spectrum,.......... | 100 | 100 |
| (2) " more refrangible region,........ | 53 | 51 |
| (3) " less " " ........ | 47 | 49 |

In this table the column marked Series I. gives the mean of four sets of measures, and that marked II. of three. At the beginning of each set the rock-salt was repolished.

TABLE II.—*Distribution of heat by flint-glass.*

| | Series I. | Series II. |
|---|---|---|
| (1) Heat of the whole visible spectrum,.......... | 100 | 100 |
| (2) " more refrangible region,........ | 49 | 52 |
| (3) " less " " ........ | 51 | 48 |

Series I. gives the mean of ten sets of measures, Series II. of eight.

TABLE III.—*Distribution of heat by bisulphide of carbon.*

| | | Series I. | Series II. |
|---|---|---|---|
| (1) | Heat of the whole visible spectrum, | 100 | 100 |
| (2) | " more refrangible region, | 52 | 49 |
| (3) | " less " " | 48 | 51 |

The sulphide employed was devoid of any yellowish tinge; it was quite clear. Series I. is the mean of eight experiments, Series II. of ten.

TABLE IV.—*Distribution of heat by quartz.*

| | | Series I. | Series II. |
|---|---|---|---|
| (1) | Heat of the whole visible spectrum, | 100 | 100 |
| (2) | " more refrangible region, | 49 | 53 |
| (3) | " less " " | 51 | 47 |

Series I. represents twenty-seven experiments, Series II. twelve. In the former two quartz prisms were used to increase the dispersion; in the latter only one was employed.

Perhaps it may not be unnecessary for me to say that I have repeated these experiments many hundred times during a period of several months, including the winter and the summer, varying the conditions as to the hour of the day, arrangement of the apparatus, &c., as much as I could, and present the foregoing tables as fair examples of the results. Apprehending that the heliostat mirror, which was of speculum metal, might exert some disturbing influence on account of its faint reddish tinge, I replaced it with one of glass silvered on the front face, but could not detect any substantial difference in the results.

The important fact clearly brought into view by these experiments is, that if the visible spectrum be divided into two equal portions, the ray having a wave-length of 5768 being considered as the optical center of such a spectrum, these portions will present heating powers so nearly equal that we may impute the differences to errors of experimentation. Assuming this as true, it necessarily follows that in the spectrum any two series of undulations will have the same heating power, no matter what their wave-lengths may be.

But this conclusion leads unavoidably to a most important modification of the views now universally held as regards the constitution of the spectrum. When a ray falls on an extinguishing surface heat is produced, but that heat did not preexist in the ray. It arose from the stoppage of ether waves, and is a pure instance of the conversion of motion into heat—an illustration of the modern doctrines of the conservation and transmutation of force.

From this point of view the conception that there exists in an incident ray various principles disappears altogether. We have to consider an incident ray as consisting solely of etherial vibrations, which, when they are checked by an extinguishing substance, lose their *vis viva*. The effect that ensues depends on the quality of the substance. The vibrations imparted to it may be manifested by the production of heat, as in the case of lamp-black, or by chemical changes, as in the case of many of the salts of silver. In the parallel instance of acoustics clear views have long ago been attained, and are firmly held. No one supposes that sound is one of the ingredients of the atmosphere, and it would not be more incorrect to assert that it is something emitted by the sounding body, than it is to affirm that light or heat, or actinism, are emitted by the sun.

The progress of actino-chemistry would be greatly accelerated if there could be steadfastly maintained a clear conception of the distinction between the mechanism of a ray and the effects to which that ray may give rise. The evolution of heat, the sensation of light, the production of chemical changes, are merely effects—manifestations of the motions imparted to ponderable atoms,—and these in their turn can give rise to converse results, as when we gradually raise the temperature of a substance the oscillating movements of its molecules are imparted to the ether, and waves of less and less length are successively engendered.

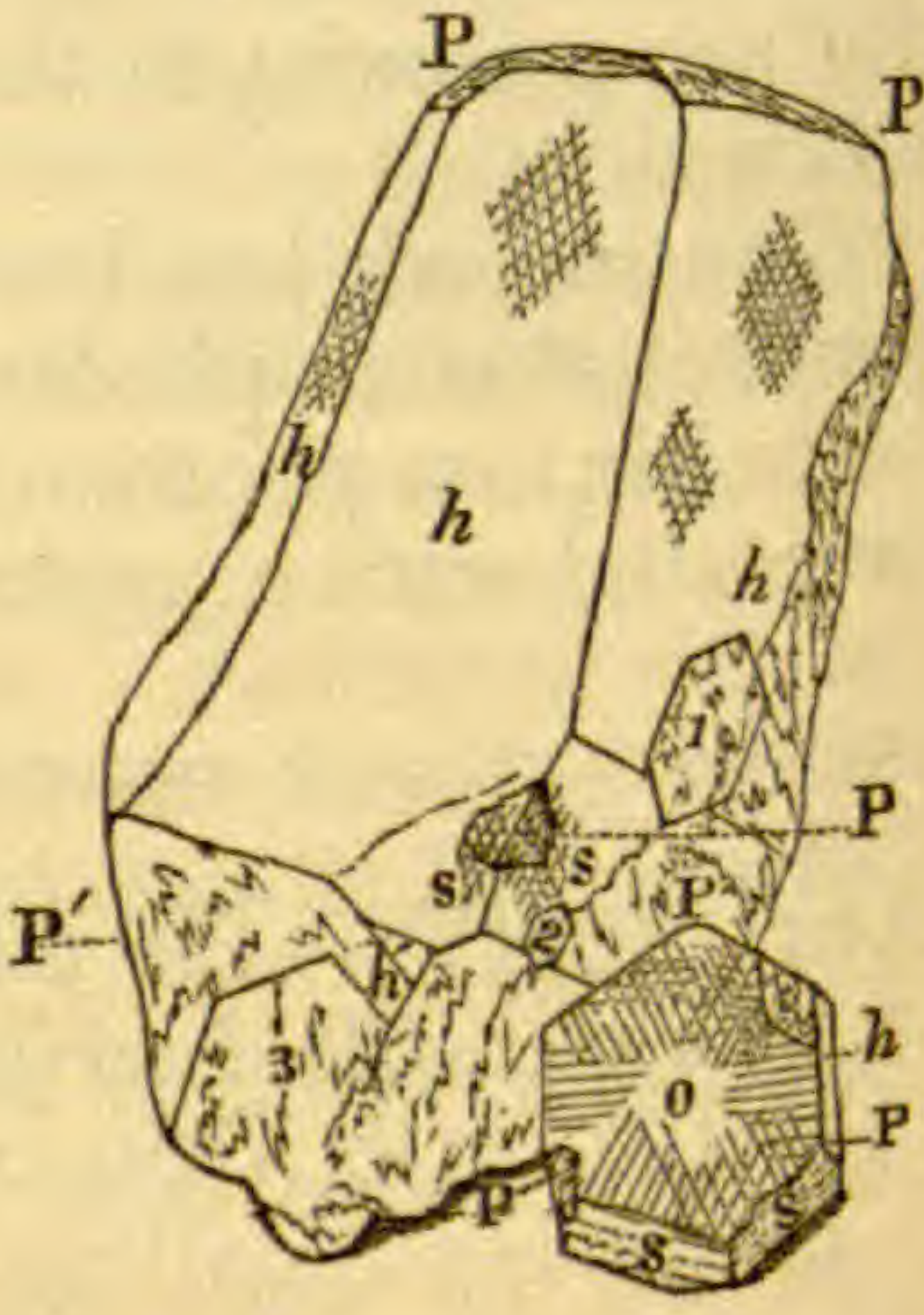
In the title of this memoir I have employed the phrase "Distribution of Heat," in accordance with general usage; but if the conclusion arrived at be true, it is plain that this should be exchanged for "Production of Heat." The heat observed did not pre-exist in the incident rays: it is the result of their extinction.

The remark has been made that these results are essentially connected with photometry. In fact, any thermometer is converted into a photometer, if its ball or other receiving surface be coated with a perfectly opaque non-reflecting substance.

## ART. XXIV.—*On the Corundum region of North Carolina and Georgia, with descriptions of two gigantic crystals of that species;* by CHARLES UPHAM SHEPARD, Sr., Prof. of Natural History in Amherst College, Mass.

(Concluded from page 114.)

IT remains to speak of the corundum itself. This may be said to be eminently crystalline throughout, often in tolerably perfect crystals of considerable size, in a few instances, gigantic. Their form, as usual, is that of six-sided prisms or pyramids, sometimes the two combined; and exhibiting occasional triangular faces belonging to the primary rhombohedron. Whether massive or crystallized however, it is readily cleavable; and the crystals are remarkable for showing cleavage lines, whereby their faces are transversely ruled off into lozenge-shaped areas, often in a very beautiful manner. The prevailing colors are blue and red, the latter often of a deep tint and handsome. The blue is intense only in small patches, and shades off into gray or pale yellowish gray. Thus far I have seen no single crystal, however small, sufficiently free from a tendency to cleavage, to answer the purposes of jewelry; though some of the larger crystals of the blue variety might perhaps afford portions pure enough for ring-stones. In a few cases I have seen pale yellow and apple-green colors. The crystals are generally rather remarkable for their translucency and internal regularity. Their unfitness for cutting, therefore, would appear to result from their too easy cleavage, rather than from other causes. The same crystal often combines the red and blue shades of color; the latter tint, if the form is pyramidal being the deepest at the base, and evincing a tendency to traverse the center of the crystal nearly to its apex, where the ruby color wholly replaces it, and sometimes here presents itself with much intensity. The faces differ considerably in smoothness and luster. Those belonging to the prism, the primary rhombohedron, and the face perpendicular to the axis, being the most perfect; while those of the pyramids are the most deficient in finish. In size, the crystals vary from a quarter of an ounce up to a pound in weight, though the latter are rare; while two have been found of very extraordinary magnitude. The largest of these weighs 312 pounds, the smaller 11¾ pounds. The annexed wood-cut, sketched from a photograph, will give some idea of their comparative dimensions. It represents them at about one-tenth the natural size.



The largest of the two is red at the surface, but within of a bluish-gray. This was found by Col. Jenks last autumn at the Culsagee mine, Macon Co., N. C.; and occurred in a layer of soft, almost pulverulent, vermiculite, within four feet of the surface of the ground. We undoubtedly owe the very perfect preservation of its form to the soft material in which it was reposited. Had it occurred at a greater depth in the stratum, where the gangue is an unaltered ripidolite, its extrication except in fragments, would have been impossible. The general figure is pyramidal, showing, however, scarcely more than a single six-sided pyramid, whose summit is terminated by rather an uneven and somewhat undefined hexagonal plane. Two opposite faces of this pyramid are produced to nearly double the width of its remaining four, which imparts a wedge-shaped or flattened aspect to the crystal. Its length is 22 inches, its greatest breadth 18, its thickness 12. The angles as approximately determined are, $h$ on $h = 124°$, $h$ on $h' = 139°$, $s$ on $s = 120°$, P (cleavage plane) on $s = 137°$. The letters P refer to cleavage planes: figures 1, 2, and 3 to hexagonal crystals projecting from the sides of the main crystal. The first of these (1) is red only near its surface; within, it is bluish-gray, as indeed the large crystal itself seems to be, since such is the color of the entire central region of the broad cleavage face P′. The lower part of the crystal, as well as a large part of the broad right hand face $h$, is largely covered by implanted crystals and laminæ of deep green ripidolite, some of which are an inch or more in diameter. The adjoining pyramidal face $h$ also displays here and there small imbedded hexagonal plates of the same species. The color of the four narrow planes is a pale pink or rose-red, that of the two broader planes is a dark purple. The surfaces of the first-mentioned set are somewhat rough, as if from erosion or a partial disintegration, resulting perhaps from the very easy cleavage of the external layers; but the broad faces retain their original smoothness and polish. On both sets, however, the lines of cleavage are very striking. An attempt to represent them in the figure is made at a few places by trellised lines, which may be taken as a picture of the whole surface of the crystal, excepting the region indicated by other shadings, where the ripidolite abounds. The small triangular space between $s$ and $s$ refers to a cleavage face parallel with one of the primary planes.

The smaller of the crystals represented in the wood-cut is a regular hexagonal prism, well terminated at one of its extremities, the other being drusy and incomplete. One of its lateral planes is reduced one-half in its dimensions, through the presence of the pyramidal face $h$. The letters P denote the existence of incipient cleavage planes, parallel with the primary rhombohedron; while the crossing lines on $o$ are to show the lines of cleavage, coincident with the upper edges of the laminæ. The meeting of these lines at the center impart a stellular aspect to this terminal plane. The crystal is incomplete, as will appear from the circular shading at one of its angles. This defect extends, however, to less than one inch in depth, where the surface becomes very uneven, from being coated by a brown vermiculite, or altered ripidolite. Some of the lateral planes are coated in patches with a white pearly margarite. The general color of the crystal is a grayish-blue, though there are spots, particularly near the angles, where it is of a pale sapphire tint. Its greatest breadth is six inches, and its length rather above five. This specimen also was found by Col. Jenks, while exploring for corundum at a new locality in Rabun Co., Ga., at a spot about 16 miles west from Walhalla (S. C.), a little north of the proposed route of the Rabun Gap railroad. The associated minerals here are described by Col. Jenks as similar to those at the Culsagee mine.

It may not be out of place to append to the foregoing a few observations upon the minerals associated with corundum at other localities. The most important of these in the United States is that at Chester, Mass., already widely known as the emery mine, a deposit forming a well defined vein or stratum, several feet in thickness, and running without interruption a distance of several miles. Emery, as is well known, puts on an appearance very different from corundum, though from its great hardness and intimate admixture with pure corundum, it has always been regarded as a variety of this species. Its peculiarity arises from its being an aggregate of corundum and magnetite. This fact is well seen at the Chester mine, the proportion of the two species here varying indefinitely from the merest sprinklings of almost invisible octahedral crystals of magnetite through the grains and crystals of corundum, up to absolutely pure massive magnetite, which itself sometimes fills almost the entire vein for a distance of rods, to the apparent exclusion of all traces of corundum. There occur again considerable stretches of the vein, consisting of almost pure corundum, or at least of this species only blended with the chloritic gangue.

In noting the associated rocks and minerals, we here find a hornblendic gneiss (with considerable epidote) on one side of the vein (rarely on both sides), and talcose slate or steatitic "pseudo-serpentine" on the other, in both of which rocks minute grains of corundum are found, at least for a short distance from the vein. In one place this hard gray serpentine has a dolomitic character; and the emery vein itself often contains narrow seams, a few inches long, of pure white and sparry dolomite, in which small crystals and grains of sapphire may frequently be detected. Rutile and corundophilite usually accompany this dolomite. Margarite in veins and balls, from half an inch to two inches in thickness, made up of broad rose-colored laminæ, placed at right angles to the sides of such veins, are frequent in the emery. In a somewhat similar manner, though much more rarely, the diaspore is here found. Epidote in slender radiating crystals is another characteristic species of the locality. In one portion of the vein a blackish-brown tourmaline is abundant; and not far distant the walls and joints of the vein are coated by a greenish-black chloritoid. Very thin laminæ of ilmenite have also been met with in connection with the margarite and other imbedded minerals. The amphodolite or indianite variety of anorthite also occurs at a few places. These are all the minerals thus far distinguished as proper to the vein. But of these it should be stated, that the magnetite constitutes at least one-third of its contents; next in abundance comes the chloritic mineral, corundophilite, forming, perhaps, one-eighth, while all the other species constitute in their entire aggregate less than one hundredth.

The North Carolina and Georgia regions chiefly differ from the Chester, then, in the absence of magnetite (and accordingly of emery), rutile and diaspore; and in having zoisite instead of epidote, ripidolite in place of corundophilite, and albite for anorthite. Besides, both the soda hornblende (arfvedsonite), so frequent in N. C., and the spinel, are wholly absent at Chester. But the two formations agree in the contiguity very largely of magnesian minerals, in the total absence of quartz and mica, and, in general, in the low proportion of silicic acid.

The corundum region of Franklin, Newton and Vernon of New Jersey and its prolongation at Warwick, N. Y., is characterized throughout by closely agreeing features. The gangue is a dolomitic limestone, in which it exists in isolated masses or pockets. It is generally crystallized, and either of a sapphire or ruby color. Its associates are spinel (which is either red or gray), rutile, biotite (phlogopite), a brown hornblende, a grass-green arfvedsonite, a peculiar feldspar, and more rarely, margarite and chondrodite (the latter sometimes changed into serpentine). Thus far, neither magnetite nor diaspore have been detected as occurring in the aggregate at these places.

Biotite here seems to take the place of the chloritic minerals found so often with corundum elsewhere, and spinel that of magnetite. But in the absence of quartz and the prevalence of magnesia, we discover a marked similarity of conditions with the N. C. and Chester localities.

Concerning the localities in Delaware and Chester counties, Pa., I am unable to speak definitely. But all the specimens of corundum I have examined from that region evince the absence of quartz; and seem to show that tourmaline, margarite and albite are its constant associates. At Clubb Mountain, Lincoln county, N. C., an entirely distinct region from that in the Blue ridge first described, the corundum is mixed with margarite and rutile. The specimens of blue corundum found within a few years at Pelham, Mass.,* were in fragments associated with biotite, and also distributed through vermiculite. The corundum found in balls of cyanite at Litchfield, Conn., is constantly associated with talc and diaspore; while the small sapphire

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* See this Journal, vol. xi, ix, p. 271.

crystals occurring at Norwich, Conn., were completely surrounded by the allied species, fibrolite.

Dolomitic limestone constitutes the chief repository of corundum in other countries, as at Campo Longo, St. Gothard; and of the emery, according to Dr. J. Lawrence Smith, in the Turkish dominions, where he has pointed out the margarite, diaspore and chloritoid as its distinguishing attendants.

The examples from Mozzo, Piedmont, show a gangue of some compact species of feldspar; but, it is noticeable, with the exclusion of all adhering quartz. The precious corundum (sapphire and ruby) coming from Ceylon in crystals, mingled with red spinel, as the produce of river washings, may fairly be presumed to have originated in dolomitic limestone; while the larger crystals and cleavable masses from Ava, Hindostan, Thibet and China, as well as those from the Urals, were probably afforded by a region in some respects similar to that of the mountainous district of Georgia and North Carolina; but of the minerals immediately associated with it in those countries we possess no reliable information.

Amherst College, June 8, 1872.

## ART. XXV.—*Notice of some of the works of J. Barrande, with extracts from his remarks with reference to the mode of origin of Paleozoic species.*[*]

1. Trilobites. Extrait du Supplément au vol. i. du Systême Silurien du centre de la Bohême. Prague, 1871. 8vo, 282 pp.
2. Systême Silurien du centre de la Bohême: vol. ii, Céphalopodes; $4^{me}$ Series, Pl. 351 à 460. 4to. Prague, 1870.
3. Syst. Sil. etc.; Céphalopodes; $4^{me}$ Series; Distribution horizontale et verticale des Céphalopodes, dans les Contrées Siluriennes. 4to. Prague, 1870.

THE receipt of the above-named volumes, which we owe to the kind courtesy of the author, Mr. Joachim Barrande, of Prague, Bohemia, gives us a favorable opportunity for noticing the extended and valuable labors of this accomplished naturalist.

For forty years or more, he has made the study of the Silurian strata of Bohemia and of their fossils his principal object. His first publication was a brief "Preliminary Notice of the Silurian System and the Trilobites of Bohemia," issued in Leipzig in 1846. This was followed, in 1847 and 1848, by a brief notice of the Bohemian Brachiopoda, published in two parts, at

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[*] Prepared for this place by Professor Frank H. Bradley of Knoxville, Tennessee.

Vienna, and, in 1850, by a paper on the Graptolites of Bohemia. In 1852, he commenced the publication of the large quarto series of detailed descriptions of Bohemian Silurian fossils, with abundant and excellent illustrations, of which two volumes are named above. The first issue was the volume of Trilobites, in two parts, 935 pages of text and 52 plates, published in 1852. Of vol. ii, which includes the Cephalopods, the first livraison, including 107 plates, appeared in 1865, followed, in 1866, by the second, which includes plates 108 to 244. The first livraison of text, consisting of 712 pages, appeared in 1867, and was followed, in 1868, by the third series of plates, including Nos. 245–350. The fourth and final series of plates, including Nos. 351–460, which is named at the head of our article, appeared in 1870, accompanied by a volume of 263 pages of supplementary text, treating especially of the distribution of Silurian Cephalopods. Volume iii. of the series, including the Pteropods of the Bohemian Silurian, and consisting of 179 pages of text and 16 plates, appeared in 1867. We still look for the supplement to the volume on Trilobites, for the remaining livraison of the text of vol. ii, which is to include the descriptions of the species of *Orthoceras*; for the volumes which are to include the Gasteropods, Conchifers, Brachiopods and Radiates; and for the final one which is to give us all the details of the stratigraphical and lithological geology of the Bohemian basin. As a whole, these volumes will constitute one of the grandest monuments ever erected to the energy, skill, patience and industry of one man, as well as to the constant liberality of his munificent patron, the Count de Chambord.

Besides this quarto series, Mr. Barrande has issued, for more general distribution, a series of octavo pamphlets, including the general conclusions announced in his larger works, but not repeating specific descriptions, although they have been issued generally in advance of the appearance of the volumes which they represent, thus showing that his object has been purely the spread of information rather than personal credit for priority. Of this series, we name one at the head of our article. He has also issued four pamphlets, entitled "Défense des Colonies," of which we propose to speak further on another occasion.

Of the volumes now before us, we can truly say that they are worthy successors to those previously published. The plates most fully and elegantly illustrate the remaining species of the Cephalopoda, and include many additional figures of species already illustrated in the earlier plates.

The supplementary text of the Cephalopods includes a thorough study and full summary of the general and detailed facts of their distribution, at least so far as Silurian forms are concerned, together with many later ones. This comprises so much interesting matter, that we feel justified in making considerable extracts, especially from the closing *Résumé général:—*

"I. *Relative importance of Cephalopods.* As regards organization, this order [Cephalopoda] is the first among Mollusks. It can also be considered as occupying the first rank among the orders represented in the Silurian faunas, if we except the Fishes, which appeared sporadically, at some isolated points, toward the end of this great period. We ought, however, to refrain from judging absolutely as to the relative preëminence of organization in the Cephalopods and the Trilobites, because the elements from which this preëminence could be determined are forever withdrawn from the comparative observations of science. We know, moreover, that zoölogy possesses no incontestable rule for exactly measuring and weighing the grade of organization of animals which belong to different classes or subkingdoms. As regards the power or physical force indicated by the dimensions of the shells of many species, there is no doubt that the Cephalopods enjoyed great superiority among the beings which animated the Silurian seas, at least during the continuance of the second and third faunas. They were, then, during these long ages, the veritable rulers of the ocean. This predominance cannot be disputed, except toward the end of the third fauna, the epoch of the appearance of the first Fishes and of the powerful Crustaceans of the genus *Pterygotus*, belonging to the family of *Eurypteridæ.* As regards prolific power, *i. e.*, the frequency of individuals, which is also indispensable for exercising domination in the sea, it would be impossible for us to decide whether the Cephalopods surpassed the Trilobites, since, in these two faunas [second and third] the remains of individuals of the two groups occur in myriads, and are equally innumerable.

Thus far, the Cephalopods may be considered as occupying or disputing the first rank * *, but, in other respects, we must perceive that the preëminence belongs to the tribe of Trilobites. They possess, in the first place, an incontestable and well-marked preëminence over the Cephalopods as regards priority. We know, in fact, that this tribe of Crustaceans constitutes by itself almost the whole of the Primordial Silurian fauna. The number of genera and species by which it is represented in this fauna is already very considerable, and we see that it tends to increase constantly, especially in England and America. * * No authentic trace of Cephalopods has yet been recognized in the same formations. The great prolific power of the Mollusks of this order in the second and third faunas authorizes us to think that, if they had existed under various generic and specific forms in the Primordial fauna, we should find their remains as frequent as those of the Trilobites in the formations which inclose this fauna. And as the first appearance of each order or of each important family is habitually announced, in the geological series, by some sporadic or prophetic forms, we long looked forward to the discovery of similar precursors of the Cephalopods in the Primordial fauna; but our expectation has not yet been realized. In any case, we cannot deny that the Trilobites had the privilege of being called into existence long before the Cephalopods. As regards the number of generic types of Trilobites, * * we judge that it may well amount to triple the number of the types of Cephalopods, which is 25, according to our classification. The advantage of the Trilobites, in this respect, is then very decided. Still, it would be notably reduced, if we deduct the types which exist only in the Primordial fauna. As regards the multiplicity of specific forms, the Trilobites are far from possessing so marked a predominance. * * Although the Trilobites maintain *some* numerical superiority, as regards species, in all the three faunas, it has not been very noticeable. But, if we consider only the second and third faunas, the predominance of the Cephalopods becomes, on the contrary, very great. On the whole, in spite of the privileges which seem to assure the first rank to the Trilobites, in the whole of the Silurian faunas, the Cephalopods possess exclusively certain advantages, which assured their domination during the continuance of the second and third faunas. The total number of Silurian species enumerated in the "Thesaurus Siluricus," in 1868, reached 9,030. Adding about 800 Bohemian species, the names of which are not yet published, and the new species announced in Canada and elsewhere, the sum total of forms known in the Silurian formation may be accounted as 10,000, in round numbers. The Trilobites contribute to this sum in the proportion of 0·167. and the Cephalopods in that of 0·162. The difference is hardly sensible.

II. *First appearance of Cephalopods.*—What is most inexplicable to us is the relative abruptness with which the Cephalopods seem to manifest themselves at the same time in many countries, under very various generic and specific forms, toward the beginning of the second fauna, while their remains have not been found in the Primordial fauna. Considering the Silurian countries as a whole, the types which show themselves in these first appearances are 12 in number, and so represent very nearly half the total number of types of this order, which is 25. * * These 12 types, as a whole, present the principal forms of the shell * * from the straight form of *Orthoceras* to the completely rolled-up forms of *Nautilus* and *Trochoceras.* While the greater part of them show the simple opening which characterizes our first series, one of them, *Gomphoceras*, represents, by its completely contracted aperture, the more ancient form corresponding with our second series. Still, we observe one important deficiency, namely, the total absence of the more simple forms of the order, *i. e.*, the *Ascoceratidæ.* The number of species derived from these 12 primitive types is about 165. * * This number represents about one-third of the 478 forms which characterize the second fauna in all Silurian countries. Thus, the order shows itself already largely developed, in generic types and specific forms, upon the horizons where we observe the most ancient traces of its existence. We ought also to note the important fact that, during the first epoch, the number of migrating species, or those common to many countries upon the grand northern zone of Europe and America, does not constitute one-fourth of the sum total of existing forms. There is, moreover, no form common to these northern regions and to the grand central zone of Europe. Consequently, the autochthonous species, *i. e.*, those belonging exclusively to a single country, represent more than three-fourths of the sum indicated, viz : 165. This localization of the great majority of the species, at a time so remote as the beginning of the second fauna, is so much the more worthy of attention, since the Cephalopods, as pelagic Mollusks, are supposed to enjoy great powers of locomotion. Moreover, science being brought to admit the existence of a very nearly uniform temperature, over the globe, at this epoch, one of the great obstacles to the general diffusion of these Mollusks can not be invoked to explain their distribution in so many centers distinct in their local faunas. The apparent abruptness in their first development, and the localization in their horizontal distribution, do not constitute exceptions which are anomalous and peculiar to this order. On the contrary, it seems to us that these are habitual and normal phenomena, which manifest themselves in the mode of appearance of the orders or of the principal families composing the Paleozoic fauna.

We possess another striking example in the Silurian Crustaceans. In all countries where the Primordial fauna has been observed, numerous generic and specific forms of Trilobites start together. While some of the first genera of this tribe are found in most countries, each of these, and especially each of the great zones, possesses many contemporaneous types which exclusively belong to it. But it is especially the distribution of specific forms which offers us one of the most remarkable examples of localization. In fact, among more than 240 species already known in the whole of the Primordial fauna properly so called, the number of those which are common to two countries geographically separated is very small. Thus the circumstances which seemed the most inexplicable, in the first appearance of the

Cephalopods, were already previously manifested in the first appearance of the Trilobites. They seem to have been even more exaggerated in what concerns this tribe of Crustaceans. We are far from believing that these appearances are due solely to the disappearance of lost faunas. On the contrary, we have demonstrated that this hypothetical conception can by no means be applied to the Cephalopods. We would here call attention to a very important fact, and one which shows a new harmony between the appearance of the Cephalopods and that of the Trilobites. We proved, in 1852, that all the genera of Trilobites appeared during the Silurian, excepting a doubtful form, called *Griffithides.* We now perceive, also, that all the generic types of the [Paleozoic] Cephalopods, *i. e.*, of the Goniatidæ, the Nautilidæ and the Ascoceratidæ, have had their rise during the same age. As to the exclusively Devonian species, named *Clymenia*, they merely represent the endogastric form of the type, of which the *Goniatiles* constitute the corresponding exogastric form. Thus the first appearances of all the district types of Trilobites and Cephalopods, with which we are dealing, have been alike concentrated in the Silurian and have been accompanied by like circumstances. These circumstances, then, seem to us to be normal.

III. *Evolution of Cephalopods.*—The evolution of Cephalopods, during the continuance of the second and third Silurian fauna, is very irregular, in each of the countries, in each of the grand zones, and also in the entire Silurian world. * * * To the absolute maximum of 665 species, produced by 10 types, in our band $e^2$, succeeds immediately the absolute minimum of 2 genera and the small number of 31 species, in our band $f^1$. By contrast, to the absolute minimum of 12 species, produced by 3 genera, in our band $g^2$, succeeds immediately the absolute maximum of 11 types, represented only by 86 species, in our band $g^3$. These two facts, well ascertained, suffice to show us that we should not attribute the evolution of the Cephalopods to the simple theoretical law of descent (*filiation*) and of slow transformation. It is important to remark that, in certain regions, the existence of Cephalopods seems to be interrupted by some total intermissions, *i. e.*, by the absence of every vestige of this order, in more or less considerable vertical thicknesses of the formations. We have noted such total intermissions in different countries; viz: 2 in Bohemia, 1 in Canada. 3 in New York, 1 in Wisconsin and 1 in Illinois. We have also shown, at the same time, that the local repeopling can not be attributed solely to immigrations, and that it consists principally of new species, peculiar to each country. * * *

The appearances of the 25 types of this order are mainly concentrated in three principal epochs, which correspond to the beginning of the second fauna, the beginning of the third fauna, and the end of the third fauna. We observe, also, that these three epochs are at the same time distinguished by the co-existence of greater numbers of genera, *i. e.*, that they present three nearly equal *maxima*, viz: 12 types to each of the first two epochs and only 11 to the third. On the contrary, the epoch which corresponds with the end of the second fauna presents only 8 co-existing types; and again we find only 8 about the middle of the second fauna; while about the middle of the third fauna we can count only 4 or 5. We do not know any reason to be assigned for these fluctuations. They are particularly well marked in the basin of Bohemia, doubtless on account of its great richness. * * About the beginning of the third fauna, the 12 existing types show a development of specific forms which constitute the absolute maximum of all the Silurian, viz, about 1000 species. This is principally due to the contribution furnished by Bohemia, viz: 746 species.

IV. *Parallel between the chronological and zoölogical evolution of Cephalopods.*—Concordance between geological and zoölogical evolution should be plainly shown, if the more simple forms of zoölogical evolution had appeared first, and if, on the other hand, the more complicated forms had appeared last, in the series of Silurian epochs. Now, observations of facts shows us that precisely the contrary has occurred. In fact, according to existing documents, the more simple forms, viz: the *Ascoceratidæ*, appeared only toward the end of the second fauna in Canada, and at the beginning of the third fauna in Bohemia. On the other hand, the more complex forms, such as *Nautilus* and *Trochoceras*, are manifested from the beginning of the second fauna, in America. * * These facts suffice to show us the irreconcilable discordance which exists between the zoölogical and the chronological evolution of Silurian Cephalopods.

But we have also noted, in the course of our studies, other facts, which confirm this discordance, and which are inexplicable by the transformation theory. The principal ones are as follows: 1. The almost abrupt appearance of a great number of generic types of all shapes, disseminated through different Silurian regions, about the beginning of the second fauna, does not accord with the idea of their slow and successive derivation from any earlier prototype whatever. In fact we have shown that the total absence of Cephalopods from the Primordial Silurian fauna forbids our supposing that these types were developed during the existence of a series of ante-primordial faunas, of which no trace remains. 2. The slow transformations, conceived of theoretically, would not account for the extraordinary accumulation of specific forms which is found in Bohemia, within a very narrow horizontal area, and the vertical thickness of a few calcareous layers of our formation E. These distinct forms reach the number of 746, representing the proportion of 0·46, *i. e.*, nearly half, of the 1622 species of Cephalopods to-day known in the Silurian world. 3. If the transformations and divergence of specific forms results, as theory indicates, in the production of new types, we ought especially to observe traces of this phenomenon in Bohemia, as a consequence of the coëxistence of 10 generic types and 665 specific forms, during the deposition of our limestone bed $e^2$. Now, we have before shown that the sum of all the variations which one might have expected here, according to theory, has *really* resulted in the *disappearance* of 8 types and 644 species, while 2 types and 21 species are continued in the equally calcareous overlying band $f^1$. Lest anyone should wish to consider this as constituting a purely local exception, resulting from accidental causes which acted in spite of time and place, we will mention another of the same character, and free from any similar interpretation. We have before stated that, among the families of Cephalopods which are the object of our present study, there has arisen no new type, either cosmopolite or local, during all the continuance of the Devonian, Carboniferous and Permian faunas. * * Still, as a whole, these faunas present a number of species of Cephalopods at least as considerable as that of the second Silurian fauna, during which there appeared 17 generic types. In this case, neither time, nor space, nor the number of specific forms, failed to favor the production of some new type.

It is then the very power of variation or transformation that has itself been wanting. If this pretended force really has a continuous action, and one inherent to the nature of organized beings, why was it so suddenly and for so long a time paralyzed as to its effect upon the propagation of the families which have from the beginning constituted the most powerful order of Mollusks? This same paralysis has been prolonged up to the present time in the *Nautili*, which have enjoyed the rare privilege of an existence without any other limits than those of the geological ages and the terrestrial surface. In fact, they have maintained their primitive generic characters, from the Silurian period, without producing a single collateral and distinct type, in spite of the number of their specific forms, in every country and in each of the extinct faunas. If the type of Cephalopods * * had been gradually constituted by transformations, up to their normal form, the intermediate transitions would be represented by a multitude of individuals, whose number might well surpass that of the examples showing the exact form which we find. Now, these transition forms are nowhere found in Silurian countries. On the contrary, wherever we observe the first appearance of a type, the conformation of the shell offers to us all the characters which distinguish it from other types of this order. No country seems to us to have been more favorable than Bohemia for the preservation of transition forms between the 20 types which it possesses; for many of them are represented by myriads of individuals, among which we do not discover any intermediate form. Of certain species, * * we have been able to collect series of individuals representing all ages of the shell, from the embryo to the adult. Since these forms, really intermediate in the development of a species, are so well preserved, why have the hypothetically intermediate forms between the types invariably disappeared, in the same basin and the same limestone layers?

In view of these facts, which are reproduced in an analogous manner in all countries, it is probable that no scientist would risk putting forth the assertion that the formations including the forms intermediate between our 25 types of Cephalopods have been everywhere invariably destroyed by denudations. It would then be superfluous to refute such a supposition. Still, we would observe that the very rocks which have been removed by denuding agents, have left vestiges of their existence and their faunas. Thus, the *diluvium* of the north of Germany has already furnished 47 species of Cephalopods * * which comes from the denuded rocks of Scandinavia and Russia. No one of these species presents to us a form intermediate between the types of these countries. Further, the pebbles of Budleigh-Salterton, in Devonshire, have already yielded, according to a recent private communication from Mr. Thos. Davidson, more than 80 species, of different orders, derived in part from still unknown outcrops of Silurian and Devonian rocks, without the appearance among them of any forms transitional between established types. So, in any case, the disappearance of deposits inclosing forms intermediate between the 25 normal types of Cephalopods would be completely inadmissible in science. We could not then invoke it in order to account for the invariable absence of these forms in the Silurian formations. On the contrary, all observations concur in indicating to us that these forms have never existed. We ought further to remark that the small number of forms which can be considered as showing an apparent transition, between any two normal types whatever, have hitherto been reported only upon horizons when their existence constitutes the gravest anachronism as regards supposed transformations. * * * These intermediate forms are posterior to the normal type, while, according to theory, they ought to be anterior to it. * * The *Endocerata* noted in our studies upon the evolution of the Cephalopods, as offering ideally an intermediate type between

*Ascoceras* and *Orthoceras*, appeared toward the origin of the second fauna, while *Ascoceras* is known only toward the end of this fauna. If we admit the reality of the transition between these forms, it will be necessary also to admit that this has taken place from the more composite forms, *Orthoceras*, to the more simple, *Ascoceras*, *i. e.*, following an order diametrically opposed to that which theory supposes.

In short, not only is the chronological evolution of Cephalopods in full discordance with their zoölogical evolution, but, also, the various considerations set forth concur in demonstrating that the suppositions relative to transformations vanish before observed facts, in all countries. On the contrary, these facts all tend * * to inspire the belief that the types of Cephalopods have appeared [originally] under the characteristic and normal forms by which we distinguish them. They seem, then, to owe their origin to a special cause and one totally distinct from descent from anterior forms.

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VII. *Extinction and gradual renovation of specific forms.*—All geologists have remarked that most of the species which characterize a formation disappear between its vertical limits, and are replaced by new forms which similarly characterize the following formation. * * * It is clear that species which cannot be attributed to either of these three sources [1, vertical propagation of identical species; 2, descent from anterior species; 3, immigration of foreign species] are entirely new, and represent the effect of gradual renewal. These three sources united have furnished only about 34 per cent. of the species of Cephalopods in any fauna whatever. * * There remain, then, about 66 per cent. of new species, representing the influence of gradual renewal in each distinct fauna. In every case, renovation or the gradual and successive appearance of new forms seems to have itself contributed at least as much as, and probably much more than, all other apparent sources, to furnish the elements of the successive faunas of Cephalopods, during the Silurian age.

It remains for us to call the attention of our scientific readers to the harmony which exists between the results of this study upon the gradual renewal of species and the results of the parallel established above between the zoölogical and the chronological evolution of Cephalopods. In considering the chronological order of the appearance of types, this parallel has brought us to recognize that the generic and specific forms of the Silurian Cephalopods cannot be regarded as gradually derived from each other by an insensible passage from the more simple to the more complicated forms. Consequently, the successive evolution of the Cephalopods cannot be attributed to a power of variation inherent in their nature and only controlled by the influence of the surrounding medium. According to this conclusion, it is indispensable to recur to other causes, in order to account for the successive appearance of the forms of this order. Now, in our study upon gradual renovation, in analyzing the elements of diverse origin which constitute any fauna whatever of Cephalopods, we have shown that at each of the epochs characterized by a distinct fauna, some new and independent forms, representing at least half of the coëxisting species, are made manifest without appreciable cause, and as if by the effect of a special creation, in each of the Silurian countries. This conclusion, immediately deduced from the whole of the facts observed in the Silurian world, confirms, in a manifest manner, our preceding conclusion, derived from the parallel between the zoölogical and the chronological evolution of Cephalopods. Both contribute equally to show us how far teachings founded upon positive facts determined by science are in discordance with the spontaneous intuitions of any theories whatever.

Mr. Barrande enumerates in his list of Silurian Cephalopods 25 genera and 1622 species.

In the volume on Trilobites, mentioned at the head of our article, he gives us the results of similar studies upon the Trilobites, and from this point reaches conclusions in accordance with those drawn from the study of the Cephalopods. We have room for merely his closing summary.

"Upon one of the earlier pages of this volume, we have recalled the fact that direct observation has marvelously confirmed the previsions of astronomical theories on the subject of the planet Neptune. These theories are then in harmony with the truth. By way of contrast, we should state, as the final result of our studies, that direct observation radically contradicts all the previsions of paleontological theories on the subject of the composition of the earlier phases of the Primordial Silurian fauna. In short, the special study of each of the zoölogical elements which constitute these phases has demonstrated to us that the theoretical previsions are in complete discord with the observed facts of paleontology. These discordances are so numerous and so well marked, that the composition of the real fauna would seem to have been calculated for the express purpose of contradicting all that the theories teach us regarding the first appearance and the primitive evolution of the forms of animal life upon the globe. So, the paleontological theories are completely invalidated by reality, whose test they cannot sustain. It is still to be ascertained whether the demonstrated discordances ought to be imputed solely to the essential principle of the theories of descent and transformation, or whether they are derived, to some extent, from their point of departure in paleontology, *i. e.*, from the supposed animal nature of *Eozoon.* This is a question whose solutions we leave to those whom it concerns. For our own part, we persist in thinking that science ought to keep strictly within the sphere of observed facts, and remain completely independent of every theory which may tend to lead it into the sphere of imagination."

## ART. XXVI.—*On the Red Oxide of Zinc of New Jersey;* by AUG. A. HAYES, A.A.S.

THIS mineral, discovered and analysed by the late Dr. Bruce of New York, was subsequently examined by M. Berthier. In his "Traité des Essais par la voie Sèche," 1834, are his results, in which no allusion to the cause of the rich red color of this mineral is made, but the remark, "le manganèse y est probablement à l'état de deutoxide," closes the description.

In the year 1845 I made some analyses of this beautiful mineral for my late friend, Mr. Frank Alger, who was then compiling a treatise on mineralogy. Impressed by its interesting relations to light, the experiments and observations were multiplied, and resulted in the expression of the opinion, that the red color is due to the presence in the mineral of extremely brilliant scales of specular iron, which, transmitting a red-brown color, and reflecting lighter tints, acted as a most powerful coloring matter would do. It was also demonstrated that no oxide of manganese higher than protoxide existed in various samples. This opinion in relation to the cause of the coloration was adopted by Mr. Alger, and expressed in his published work, and subsequently Prof. Dana quoted the analysis and opinion in his standard work on Mineralogy. In the years, since passed, the subject has been several times discussed by scientific friends; and when doubts of the sufficiency of the cause have been expressed, a resort to ocular proofs at the moment has been deemed convincing and satisfactory.

On turning to the description of this mineral in the 5th Edition of the admirable system of Mineralogy by Prof. Dana, I was surprised by the statement that the distinguished and accurate author had found, by "means of a high magnifying power, that this ore is free from foreign scales of red oxide of iron."

Dr. Lewis Feuchtwanger had kindly sent me a large cabinet specimen of a finely colored mass of this mineral engaged in calcite. At some points of junction the red oxide seemed to have stained the white calcite, as a coloring matter would have done. This,—with various specimens of my own, including part of the most distinctly crystallized form yet found, and ranging in color from deep red garnet to reddish-orange—were selected as the subjects enabling me to criticise my early results, with the light afforded by improved methods of research. To those who turn over these pages, it may sum a trivial matter whether the color of this mineral be due to "red oxide of manganese" or another kind of matter, so long as a body foreign to the basis oxide of zinc is present. I do not accept this conclusion.

If we are to seek a complete knowledge of a mineral presenting color or altering white light—the cause of this color, or action on light, becomes an important part of it. Color may arise from modification of structure, by the intrusion of unallied minerals; by quantity of added matter or solely by the action of a minute weight of a body so framed as to decompose light. These are proper subjects for study, and the results become, not only an advance in science, but afford to the student the delight arising from positive increase of knowledge, and an introduction to a large untrodden field—open to research.

The color of the amethyst is due to little flaws and cavities filled with a highly refracting coloring matter. This fact has been known for about a century. In a quartz crystal examined a part of the length was colorless; then a section of amethyst was followed by colorless quartz—the summit of the crystal was amethyst—and no preceptible disturbance of the crystal-forming process could be detected.

In this case, the beauty, value and interest in the colored part of the same crystal were greatly increased by a minute amount of added matter, and the suggestions arising from states of altered conditions. The "Oriental" amethyst of the dealers is distinguished from other kinds only by the number and dispositions of these color flaws, a fact which has surprised and interested every lapidary and collector, when the proofs were offered. The remark applies to colored gems—as we see commonly in emerald when compared with the same mineral as aqua-marine, and in all collections of lapidaries, museums and of private owners in Europe: it is easy to convince one's self that the more valued qualities in colored gems are always due to defects of a certain magnitude in the crystal, which have been filled and changed by the intrusion of foreign mineral bodies, conferring more and new beauty, and enhancing the intrinsic value of the gem.

The observations which follow apply to the mineral which has been freed from carbonates and crenates, and dried at 100° C.

This mineral presents a crystalline form in lamina of varied thickness, sometimes distinctly divided by thin layers of specular iron scales. These scales when abundant reflect an iron-black color from the surface, while the cross fracture is ruby-colored. As the depth and beauty of the color diminishes, the scales of specular iron, whether observed in the mass, or separated by solution, become less abundant, and Prof. Blake has described a yellow specimen free from iron scales. The powder is of some shade of orange-yellow: the semi-transparent and rich red fragments of this mineral when looked *upon* become opaque when placed between the eye and some source of light. The rays of light enter the mass, and absorption of part takes place, while parts are being refracted and reflected. Entire transmission of red rays does not occur; in thin splinters only a little altered light passes. We thus get an indication of unequally diffused foreign matter, not pertaining to, or necessary in, the structure of the mineral. In using either the Nachet or chemical form of microscope, the aid of the lowest or lower power is best; and when both reflected and transmitted daylight illuminates the assay, we observe that a splinter of the mineral transmits an olive-green or brownish-yellow light, varying with the thickness. Thick portions reflect a portion of reddish light, in shadow, olive to bluish-gray, becoming silvery in slant rays. At about 20° inclination a splinter in partly reflected light becomes crossed by lines of a dark color. Artificial light makes the assay more yellow; a nice adjustment of the incident ray allows a red reflection from the lines, now become surfaces of reflection. If the assay be cubical, the rays transmitted *through* the lamina are arrested and dispersed, when the ends or sides of the cubes are observed. In these directions dark-colored scales may be seen engaged in the brownish-yellow or olive-tinted masses. This observation is important in explanation of diversity of conclusions among observers. The scales generally lie parallel with the laminæ of the assay, so that the passing rays are transmitted or slightly disturbed, and the red color of the mineral is replaced by that color, which the scales transmit in a colorless medium. If a concentrated fluid sulphate of the ore be observed, the naked eye takes in the same tint.

The tendency of fracture is in the direction of the laminæ, and in preparing the assays the better way is to crush the mineral between steel surfaces to the diameter of pin wire, and select the more opaque grains. These show the scales as distinctly as the bars of a window are seen.

Fragments of larger size may be covered with acetic hydrate (No. 8 of the shops). The assay dissolves as quietly as would sugar, and the line of contact of assay with acid never shows a dilution of color. Points of scales, having right-lined forms, appear, and can be moved so as to transmit or reflect colored light. But the most beautiful exhibition occurs later. The strength of the acid is such as to form a saturated solution, and hence it acts most rapidly on the more basic elements of the ore, and in so doing allows the foreign bodies only slightly altered to come in view. From the scales, little clouds arise and float away, often carrying the specular iron scales, and decomposing light. In this way, beautiful hues accompany the chemical and mechanical actions, and the latter show the constitution and compound nature of these scales.

Sulphuric hydrate, so diluted as to form the brilliant transparent rhombic prisms of zinc-sulphate, in acting on the assay in a saturated solution, transfers the scales in the assay to the colorless crystals forming. We get the scales of specular iron arranged in the axes of the prisms of little crystals; but the charms of color, luster and action on light are so far lost, that the ordinary exhibition of translucent scales engaged in a transparent medium alone is given.

The facts, that the red zinc ore always contains particles of specular iron of scale thickness, bounded by right lines, and that these scales are covered by or connected with a highly lustrous mineral, decomposed by the feeblest acids, become evident. That the color of this ore is not produced by any inhering coloring matter, but results from the action of intruding minerals absorbing some, reflecting and dispersing other rays of light, can be proved by the aid of the microscope.

In passing to the chemical analyses of this mineral, it becomes my duty to meet some suppositions formed by others respecting the cause of color; to prove, so far as is possible for me, the *negative* in relation to any other cause than that which a course of experiments demonstrates as existing. The sufficiency of this cause in producing the effect may not be convincing alike to differently constituted minds. It was a casual remark of Berthier,—a most distinguished and accurate chemist—that the oxide of manganese in this mineral is probably in the state of deutoxide. This *probability* has been seized upon to account for the color of the mineral. There is no red compound of deutoxide of manganese known which could exist in this mineral. Compounds with sulphuric hydrate and chlorine exist, but I make use of both these re-agents as most sensitive and trustworthy in proving the absence of any oxide of manganese higher than protoxide.

Composition. Retaining the numbers obtained in 1845, after most carefully criticising their accuracy, as representing the composition of the more perfect red specimens, we have

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| Zinc protoxide | 93·48 |
| Manganese protoxide | 5·50 |
| Scales of specular iron | 0·44 |
| Ferric deutoxide | 0·36 |
| Loss | 0·22 |

The loss is excessive, and was then supposed to arise from a body undetermined. Resort to the more precise mode of independent separation did not diminish this loss, and samples reserved have enabled me to account for it in part, and add silica, calcium oxide and another metal to the number of those before known to be present. In resuming this subject, the ore has been subjected to a careful search for all other known bodies which can be detected without the aid of the spectroscope. The red samples of this ore may be considered as a mixture of pure anhydrous zinc and manganous oxides in ninety-nine parts, and foreign intrusive bodies one part in one hundred parts, and in some cases the latter do not form half this weight; making the one hundredth part scales of specular iron, ferric deutoxide, silica, molybdous oxide, calcic oxide and HO, which have been determined.

The absence of the higher oxides of manganese was proved by the fact that the clean mineral of more than four pounds weight, when tested in pure chlorhydric acid, did not produce chlorine.* When the ore, chlorhydric acid and slips of copper are inclosed, the acid dissolves only so much copper as the deutoxide of iron present would do. When coarse powder of the ore is added to strong chlorhydric acid, no color appears near the particles,—a most sensitive test. Avoiding any reaction of the mass of zinc oxide—the latter separated from the residue by cold diluted acids, such as acetic hydrate and sulphuric hydrate, which do not dissolve higher oxides of manganese—these residues do not afford any traces of the higher oxides, where, if they existed in this ore, they would be concentrated. If the mineral has been heated in the steps of analysis, or weathered by exposure, then deutoxide of manganese appears, and hence doubtless the supposition of its occurrence has arisen.

If we suspend large fragments of prepared ore in diluted sulphuric hydrate contained in a tall jar, the descending striæ of the solution forming contain scales of specular iron, from which the brilliant investing matter has been dissolved. Removing and drying the fragments, after partial solution, the saline

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* When the preparation of pure chlorhydric acid was going on, it was observed that chlorine and sulphurous acid may co-exist for weeks in the stronger solutions of this acid without re-action.

matter incrusts the surface of an olive-yellow hue. The numerous points projecting permit us to see the arrangement of the altered scales, and in a beam of light to catch the absorption, reflections and dispersion caused by the unaltered mineral within the fragments. This observation is suggestive in several ways. In the instances of the lightest colored specimens being used in the trials, it was necessary to view the striæ through a lens, in order to see the scales descending, although the collection at the bottom of the jar proved their presence. This led me to inclose the clean fragments of ore in four, six and eight thicknesses of fine cambric, which had been so treated as to leave nearly pure cellulose. The cylindrical rolls were suspended in tall jars of acetic and sulphuric hydrate—largely diluted—so as to be just below the line of surface. Descending striæ indicated the chemical action as before, and after two or three days the rolls were removed, soaked in successive portions of water, and opened. The inner surface of the roll was covered with a lustrous film, composed of transparent scaly particles of a general light brown color, highly reflective and in part transparent. The siliceous skeleton and partly decomposed mineral connected with the iron scales was thus obtained. When the solutions from the rolls had reposed for eight days, a ray of light showed myriads of diffused and nacreous scales in the solution, proving their excessive fineness and tenuity.

This ore is so eminently basic that it dissolves in all the solvents of even zinc hydrate, and in the act often develops heat. In my endeavors to select a solvent which, removing most of the zinc and manganous oxides, would leave the intruded mineral free, all the solvents which suggested themselves have been tried in this connection without success. The crystallizing process has also been tried, and even the beautiful and neatly crystallizing double zinc and manganous and ammonium sulphate retained portions of the mineral, after graduated crystallization. The best and most instructive mode of action adopted is the following:

Both ammonium chloride and hydrate dissolve the ore, and in the air the manganous deutoxide forms and separates. If we pass hydrogen gas though a solution of the ore, from which scales of specular iron and silica have been separated, contained in a two-tubulated flask, ammonia chloride and hydrate in excess being added, the oxides first precipitated dissolve. After repose, the clear solution may be removed by pressure of the gas, leaving a mere flock of ferric deutoxide. If silica is present it dissolves in the zinc solution with the other bodies, and it may be stated here that the substances sought for are so small a part of the whole weight that, in presence of the mass of zinc oxide, re-agents generally fail in separating them. By placing fragments of ore of one-third of an inch cube in cambric cellulose as above stated, and using as a solvent the ammonia chloride and hydrate solution in the jars, the ore dissolves freely, while the surface of the solution, and later the outer folds of the rolls become covered with the manganic deutoxide, formed from the protoxide as it is withdrawn from the protoxide solution below. This action alone would settle the condition of the manganese existing in the ore; but it also removes both zinc and manganous oxides from the mineral. As oxygen cannot penetrate the folds through a manganous solution, after the action has ceased we have left on the inner fold the intrusive mineral, brilliant, satin-like in luster, and in the most perfect state it has presented. It has, however, been partially decomposed, and is a skeleton or silhouette of the original, greatly expanded, excepting just where it is engaged in undissolved portions of the fragments.

Thus obtained, its general hue is brown, by reflected light, as we see it by the microscope in the mineral; but its micaceous films transmit white and yellowish rays, excepting when engaging scales of specular iron; then reddish-brown to orange-red rays are transmitted, and the reflections become lustrous in a high degree.*

The production of color by this intrusive mineral, which may be considered as a silicate of zinc, calcium and ferric deutoxide, seems to depend on its mica-like structure and compound composition, including the specular iron scales. We have seen that the ore loses its red color when a ray of light is transmitted, or only the brown color of these scales comes in view, and where partial absorption succeeds red color results.

The experiments demonstrating its constant presence in the colored specimens of ore, and its proportions following the depth of color always, would not be conclusive, if they failed to throw light on the state of division naturally presented. Here the value of the observations of the solution of the mineral in close textures rests. Even when we use nearly pure cellulose paper-filters of finest texture, and many folds, the mineral passes, and we reach that state of division, where mordants dye solid colors, or the mystic boundary separating solution proper from suspension. The apparent staining of the calcite by the ore alluded to is here explained, and the necessity for the existence of much ponderable matter to produce color, even in the powder, negatived. Indeed, in some reddish-colored samples, the whole foreign matter separated did not exceed $\frac{44}{100}$th part, and only a small part of this was color-producing material.

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* It is not, perhaps, generally known that the splendid peacock hues of the anthracite coal of Pennsylvania, in the upper layers, are due to the decomposition of light by the films of calcite, formed from calcium crenate as in Newtonian rings.

Although multiplied trials have failed in separating the intrusive mineral, in a pure state, we have the evidence of its extraordinary power of action on light, in its partially decomposed condition, and in some trials we can see it naturally engaged in the mass, and coloring it, while its appearance at the surface under chemical action removes all doubt of its being the cause of color. It was deemed important that the feature of sufficiency of this cause of color should be supported by analogy, and I have sought in several directions for facts of this kind. I submit one of these.

The mineral carnallite presents in some specimens a full rich red color. By fracturing and selection samples were made for analysis.

1200 parts in pure water afforded one part of matter remaining from a colorless, clear solution. This one part afforded $\frac{40}{100}$th part of specular iron scales tolerably pure, while the brilliant satin-like investing mineral, in proportion too small for weighing, gave to the compound a resemblance to the luster color, and action on light, seen in the intruding mineral of the red oxide of zinc. This is the most significant proof I have met with, and the work was done before the interesting experiments of Prof. G. Rose were published in this country.

In numerous trials no feature favoring the presence of other causes of color has been observed, and this ore must take its place with other minerals whose colors are due to foreign minerals crystallizing with or intruding into them.

The detection of molybdenum in this ore is easily effected by evaporating a neutral chlorhydric solution to a syrupy consistence on a hasp formed of zinc and platinum. The platinum becomes black and covered with a coating, which dissolves in potassium bisulphate, or phosphoric hydrate as altered by heat, in a state fitted for testing.

**Brookline, Mass., 10th July, 1872.**

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## ART. XXVII.—*Remarks on Dr. R. Radau's paper in Dr. Carl's "Repertorium"* (vol. viii., no. 1), *entitled "Remarks on the influence of a motion of Translation of a Sounding Body on the Pitch of the Sound;"* by ALFRED M. MAYER, Ph.D.

IN the last number of *Carl's Repertorium*, Dr. R. Radau, of Paris, writes an article, bearing the above heading, of which the following is the opening paragraph:

"The simplest means of showing the influence of a motion of the source of sound on the apparent pitch, an effect first suspected by Doppler, is, perhaps, the application of two tuning forks of almost equal pitch. This is already mentioned in König's Catalogue of Acoustic Apparatus, 1865; also in Pisko's Latest Acoustical Apparatus, page 224; and in my Popular Acoustics (page 298 of the German edition), as also in other places. Still one Mr. A. M. Mayer has communicated the same method to the Paris Academy of Sciences, 11th March, as something altogether new. The only difference is this: that König counts the beats which are gained or lost to the ear by a motion of the tuning fork, whereby the change of pitch is measured, while Mr. Mayer, an American, only shows by a little cork ball, resting against the stationary fork, whether it vibrates or not with the moving one."

As Dr. Radau does not give König's experiments and mine, so that they may be compared, but quickly disposes of us both to hurry to tell of his own, I deem it but just to König and to myself that our experiments should be set forth in our own words, so that a just inference may be drawn from their comparison, while, at the same time, they will serve one to form a proper estimate of M. Radau's claim to experiments subsequently explained in his paper.

From König's *Catalogue des Appareils d'Acoustique*, Paris, 1865, p. 16: "73. Deux diapasons $ut_4$, montés sur leurs caisses de résonnances et accordés pour donner exactement quatre battements par seconde.

On peut varier l'expérience de plusieurs manières. Voici la plus simple.

On met les deux diapasons l'un à côté de l'autre, à quelque distance de l'oreille; puis, ayant constaté d'abord qu'ils donnent bien les quatre battements par seconde, on rapproche le plus grave des deux de l'oreille, d'environ 60 centimètres, tout en continuant de compter les battements. L'oreille reçoit alors de ce diapason une vibration double de plus, pendant le temps employé à le déplacer, et l'on constate alors la perte d'un battement dans le même temps. Si c'est le plus aigu des deux diapasons que l'on rapproche de l'oreille, on obtient un battement de plus.

Si on tient l'un des diapasons à la main, les yeux fixés sur un pendule qui bat les secondes, on arrive sans peine à lui donner un mouvement de va-et-vient, tel qu'on entende toujours alternativement trois et cinq battements par seconde. J'ai enfin fait l'expérience en mettant les deux diapasons à une certaine distance l'un de l'autre, et en promenant entre eux, soit l'oreille elle-même, soit ce qui est de beaucoup préférable, un résonnateur $ut_4$, mis en communication avec l'oreille par un tube en caoutchouc.

Je ferai encore observer qu'on arrive, par le même procéde, à déterminer approximativement la longueur d'onde d'un son et sa hauteur."

From the *Comptes Rendus*, 11 Mars, 1872: "*Expériences acoustiques tendant à démontrer que la translation d'un corps en vibration donne lieu à une onde d'une longueur différente de celle que produit le même corps vibrant dans une position fixe.* Note de M. A. M. Mayer, présentée par M. Delaunay.

### L'APPAREIL.

"Après m'être procuré quatre diapasons à fourchette appuyés sur des caisses résonnantes et donnant la note $ut_3=256$ vibrations complètes par seconde, je les ai designés par les nos. 1, 2, 3, 4. J'ai mis à l'unisson parfait les nos. 1 et 2 d'après un procédé que j'indiquerai plus tard. No. 1 fut placé devant une lanterne magique; une petite balle de bon liége (6 millimètres de diamètre), suspendue par un filament de soie, affleurait une de ses branches; l'image du diapason et de la balle de liége fut projetée sur un écran. No. 3 avait l'extrémité d'une de ses branches chargée de cire, de manière à donner deux battements par seconde avec no. 1 ou no. 2.

No. 4 avait les extrémités de ses branches limées et donnait aussi deux battements par seconde avec no. 1 ou no. 2; ainsi no. 4 faisait deux vibrations par seconde de plus que no. 1, tandis que no. 3 faisait deux vibrations par seconde de moins que no. 1.

### LES EXPÉRIENCES.

"Dans les expériences 1 à 7 inclusivement, le diapason no. 1 reste devant la lanterne, la balle de liége affleurant une de ses branches.

Exp. 1.—Diapason no. 2, attaché à sa caisse et tenu à la main, est mis en vibration à une distance de 30 et 60 pieds du no. 1; la balle est écartée de la branche du diapason no. 1 qui vibre à l'unisson avec no. 2.

Exp. 2.—Je me suis placé à une distance de 30 pieds du no. 1, tenant le diapason no. 2 détaché dans une main et sa caisse dans l'autre. Alors, j'ai fait vibrer le diapason et je me suis dirigé rapidement vers no. 1. Lorsque mon movement fut devenu uniforme, je posai le diapason sur sa caisse, et l'ôtai avant de m'arrêter. Bien que je n'aie été éloigné du diapason no. 1 que d'un pied à peu près, la balle de liége resta en contact avec la branche du diapason.

Exp. 3.—Je me suis approché de nouveau du diapason no. 1 comme dans l'expérience 2, mais sans ôter le diapason de sa caisse après l'avoir attaché. La balle ne bougea pas jusqu'au moment où je m'arrêtai; mais à ce moment même mon assistant, qui tenait l'oreille près de la caisse tandis qu'il observait l'écran, entendit vibrer le diapason no. 1 et vit sauter la balle de liége.

EXP. 4 et 5.—Je me suis éloigné du diapason no. 1 au lieu de m'en approcher. Le résultat a été le même que dans les exp. 2 et 3.

EXP. 6.—J'ai fait vibrer, comme dans l'exp. 1, le diapason no. 3, qui faisait 254 vibrations par seconde. La balle ne bougea point. Alors j'ai détaché le diapason de sa caisse, et, me mettant à une distance de 30 pieds du diapason no. 1, j'ai balancé la caisse dans la main vers no. 1, mettant no. 3 dessus quand elle approchait no. 1 avec la vitesse convenable (8—9 pieds par seconde). La balle fut subitement rejetée de no. 1. Si l'on ralentit ou accélère considérablement le mouvement de va-et-vient de la caisse, les vibrations de no. 3 ne produiront aucun effet sur no. 1.

EXP. 7.—Le diapason no. 4, qui fait deux vibrations par seconde de plus que no. 1, fut substitué à celui employé dans l'exp. 6, mais placé sur la caisse en mouvement, quand celle-ci s'éloignait de no. 1. Le résultat de ce mouvement et des changements effectués dans la vitesse fut le même que dans l'exp. 6.

EXP. 8.—J'ai placé le diapason no. 3 devant la lanterne et balancé le no. 1 comme dans l'exp. 7, avec le même résultat.

EXP. 9.—J'ai placé le diapason no. 4 devant la lanterne et balancé le no. 1 comme dans l'exp. 6. Le résultat fut le même que dans l'exp. 6."

It is thus seen that König's method is founded on *the phenomenon of beats*, and his experiments are adapted only for exhibition before a small auditory; while my method is founded on *the phenomenon of the communication of vibrations*, and as the images of the fork and cork-ball are projected in greatly magnified proportions on a screen, they have been witnessed, with entire satisfaction, by an audience of nearly one thousand persons. In other words, König's are, we may say, *subjective* in their character, and the alteration of wave-length is inferred from the change in the frequency of the beats; while mine are eminently *objective*, and are directly intelligible from the visible mechanical actions produced by the forks.

It may be asked, why did I not mention Mr. König's beautiful experiments as a proper preface to my own? Before publishing my results, I examined into the literature of the subject as far as the journals and transactions of societies allowed, and I found nothing that interfered with my claim to the use of the forks, as well as the above application of the principle of the communication of vibrations, and the exhibition of the same to a large audience by means of the lantern. It was only after my communication to the Paris Academy had been published, that my friend Professor Rood, of Columbia College, showed me in König's catalogue the account of his experiments, as above quoted. Had I sooner met with them I would have prefaced my paper with a minute account of his work. As it is, I do all I now can, and here publicly render to M. König the *amende honorable.*

Let us now proceed to examine another paragraph of Dr. Radau's paper, after having perused the account of my experiments from No. 2 to No. 9 inclusive, M. Radau says, "If the forks are mounted on resonators, the change of pitch can also be observed by imparted vibrations. One fork is left upon a table; the other, tuned *unisono*, is strongly vibrated and approached to or moved from the other. If the second fork is in contact with the resonator *only during this motion*, the *first* is *not* put into vibration because the exciting sound is put out of tune by the motion. The imparted vibrations can, however, be obtained by first putting the exciting fork out of concord, by proper means (for instance with wax)."

I have carefully examined into the history of this subject, and have read all the articles referred to by M. Radau in his paper, including his own "*l'Acoustique*," and I feel justified in saying, that surely I could not have desired a clearer or more concise description of the real essential principle of my communication to the Paris Academy of Sciences than is contained in the above quoted paragraph; and after having M. Radau to tell us in his opening paragraph that I have done nothing new, it is in truth very cool in him thus, in a subsequent paragraph, so complacently to appropriate my work.

Thus one M. Radau, a Frenchman, treats the "one Mr. A. M. Mayer, an American."

July 5th, 1872.

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## ART. XXVIII.—*Preliminary Description of New Tertiary Mammals;* by O. C. MARSH. Part II.

THE present communication is a continuation of the article in the preceding number of the Journal (p. 122), in which were described some of the new mammalian remains discovered by the Yale College expeditions to the Rocky Mountain region. Among the animals briefly described in the present paper are several which appear to be Marsupials, the first fossil species of the group detected in this country, and hence of much interest.

### *Limnofelis ferox*, gen. et sp. nov.

A gigantic carnivore, nearly as large as a lion, is represented in our collections by portions of a skull, a fragment of a lower jaw containing the sectorial molar, and by some vertebræ and other less important parts of the skeleton of the same individual. The remains show that the animal was a typical carnivore, one of the largest and most powerful yet discovered. The tooth preserved resembles the corresponding one of the lion in its general shape, but is proportionally broader anteriorly, the base of the crown being subtrilateral in outline, with the inner side the longest. This tooth was in close contact with the one in front. The zygomatic process of the squamosal is proportionally more massive than in the lion, although similar in form.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of lower sectorial molar, | 24·5 mm. |
| Greatest transverse diameter, | 11·75 |
| Height of tooth above jaw, | 14· |
| Greatest vertical diameter of zygomatic process of squamosal, | 32· |

This interesting specimen was found, in September last, by Mr. J. F. Page, of the Yale party, near Henry's Fork, Wyoming. The geological horizon was Eocene, or Lower Miocene.

### *Limnofelis latidens*, sp. nov.

A second very large carnivore, but inferior to the preceding in size, is indicated by a last upper premolar, and probably by some other fragmentary remains. This premolar is unusually broad, and is remarkable for its large posterior tubercle, which is two-thirds the size of the main cusp. The anterior tubercle is very small. On the outer face there is a well-marked basal ridge. The crown is 16·5$^{\text{mm}}$ in longitudinal diameter, 11·$^{\text{mm}}$ in transverse diameter, and 15·$^{\text{mm}}$ in height. Another specimen, apparently of this species, is the left lower jaw of a young individual. It contains the canine, and three molars, the last of which is still nearly enclosed in the jaw. The space occupied by the three molars is 46$^{\text{mm}}$.

The only known remains of the species were discovered by Mr. G. M. Keasbey and the writer, last autumn, in the Tertiary beds of Grizzly Buttes, near Fort Bridger, Wyoming.

### *Limnocyon riparius*, sp. nov.

A new species of *Limnocyon*, about the size of a fox, is represented by both lower jaws and a single upper molar from the same animal. The lower jaws are long and massive, and throughout the space occupied by the premolars and molars they maintain nearly the same width and depth. The symphysis is elongated, and the rami were but slightly coössified. There were six teeth behind the canine, all close together and each with two fangs. The last two are tubercular. The canine was large, and near the symphysis.

*Measurements.*

| | |
|---|---|
| Space occupied by lower premolars and molars, | 47· mm. |
| Space occupied by last three molars, | 26· |
| Antero-posterior diameter of penultimate lower molar, | 9· |
| Transverse diameter, | 5· |
| Depth of jaw below this molar, | 12·5 |

The specimens on which this description is based were found, in August last, by Mr. O. Harger, at the same locality as the preceding species.

### *Limnocyon agilis*, sp. nov.

A still smaller species, apparently of the same genus, is well represented by the greater portion of a skull with teeth, and the more important parts of the skeleton of the same individual. In the lower jaws, the premolars are separated from each other and from the canine, and the first premolar has but a single fang. The first upper premolar is separated nearly its own longitudinal diameter from the canine. The penultimate upper molar has its elevated pair of cusps more closely united than in the last species. The present animal had a long tail, and claws resembling those of a fox.

*Measurements.*

| | |
|---|---|
| Space occupied by three lower premolars, | 27·6 mm. |
| Depth of jaw below third lower premolar, | 12· |
| Antero-posterior diameter of penultimate lower molar, | 7·75 |
| Transverse diameter, | 3·6 |
| Length of astragalus, | 21· |
| Transverse diameter of distal end of humerus, | 27· |

The above remains were found by the writer, in September last, in the Tertiary shale of Grizzly Buttes, Wyoming.

### *Thinocyon velox*, gen. et sp. nov.

A small carnivore, about as large as a cat, is represented by a nearly perfect lower jaw with several teeth, and perhaps by some less characteristic remains of other individuals. The jaw somewhat resembles in its proportions that of *Lymnocyon*, but it is more elongated, and the symphysis is more nearly horizontal. The angle of the lower jaw is inflected, thus probably indicating the marsupial affinities of the species. The condyle, also, was evidently but little elevated. The number of teeth in each lower jaw was nine, divided as follows: Incisors 2, canine 1, premolars and molars 6. The incisors are small and compressed. The canine is large, and nearly round at the base. The last two molars are tubercular; and the four anterior teeth compressed, and each has two fangs.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of lower premolars and molars, | 30·5 mm. |
| Extent of last three teeth, | 16· |
| Depth of jaw below last molar, | 7·5 |
| Length of symphysis, | 17· |
| Longitudinal diameter of canine at base, | 3·75 |
| Space occupied by two left incisors, | 2· |

The specimen on which the present description is based was found, in September last, by the writer, during the explorations of the Yale party in Grizzly Buttes, Wyoming.

### *Viverravus* (?) *nitidus*, sp. nov.

A diminutive mammal, about the size of a weasel, is clearly indicated by a perfect penultimate lower molar, which agrees so nearly with the corresponding tooth of *Viverravus gracilis* Marsh, that the species it represents may for the present be referred to that genus. Other remains in our collections probably pertain to the same species. The crown of this molar is composed of a posterior tubercle, which has its summit near the outer side; next a pair of elevated, pointed cusps, of equal size, the exterior being slightly in advance of the other; and in front a small, slightly bifid tubercle.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of penultimate lower molar, | 4· mm. |
| Transverse diameter in front, | 1·5 |
| Transverse diameter behind, | 2· |
| Height above jaw of central tubercles, | 3· |

The above specimen was discovered last autumn, by G. G. Lobdell, Jr., in the Tertiary shale near Henry's Fork, Wyoming.

### *Thinolestes anceps*, gen. et sp. nov.

The collections made by the Yale party include the remains of a number of small carnivorous mammals, which are apparently very unlike any hitherto known. In dentition, they somewhat resemble several extinct species, supposed to be of suilline affinities, but their carnivorous characters appear unmistakable. All apparently had the angle of the lower jaws inflected, and present other marsupial characters, although in general structure they are very different from any known form of that group. The teeth in the present genus are similar to those of *Limnotherium*, and the two genera are evidently nearly related. In the complete description, the characters and affinities of this peculiar group, which may be called *Limnotheridæ*, will be fully discussed.

The lower jaws in this species are short and stout. The teeth agree in number and general form with those of *Limno-*

*therium tyrannus* Marsh, and may be divided as follows: Incisors 2–2, canines 1–1, molar series 7–7. There are seven teeth, also, in the upper jaw behind the canine. The first premolar above and below has only a single fang. The upper molars have an external pair of pointed cusps, and on the inner side the first and second true molars have a pair of nearly confluent cones, of which the anterior is much the larger. The last upper molar has but one inner cone. The lower jaws are coössified at the symphysis, but the suture is visible externally. The head of the humerus is much like that of the opossum, and the distal end has a similar supra-condilar foramen. The astragalus resembles that of the raccoon. The animal had a long slender tail, and was nearly as large as an opossum. Its food was probably, in part, insects.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of upper molar series, | 28· mm. |
| Extent of three upper true molars, | 15·5 |
| Extent of three lower true molars, | 18· |
| Antero-posterior diameter of last lower molar, | 6·6 |
| Transverse diameter, | 4· |
| Depth of jaw on posterior face below last lower molar, | 10· |

This species is represented in the Yale Museum by the more important part of several skeletons, which were found, last autumn, by Mr. J. F. Quigley, Mr. G. G. Lobdell, Jr., and the writer, in the Tertiary deposits of Western Wyoming.

### *Telmalestes crassus*, gen. et sp. nov.

This genus, which closely resembles *Thinolestes* in the dentition of the lower jaws, may be readily distinguished from it by the upper molars, which have the inner pair of cones of the first and second true molars separate, and of nearly equal size. The last upper molar has but one inner cusp. So far as is now known, the dental formulæ of the two genera are the same, and in the proportions of the jaws they are very similar. The present species was about as large as a raccoon, but the lower jaws are much stouter, and are ankylosed at the symphysis.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of lower molar series, | 37· mm. |
| Extent of three lower true molars, | 23·5 |
| Antero-posterior diameter of last lower molar, | 9·5 |
| Transverse diameter, | 5·5 |
| Extent of last three upper molars, | 18· |
| Transverse diameter of last upper molar, | 7·8 |

The specimen on which the above description is mainly based was discovered, last September, near Henry's Fork, Wyoming, by Mr. O. Harger, of the Yale party.

### *Limnotherium affine*, sp. nov.

A species of *Limnotherium*, somewhat smaller than *L. tyrannus* Marsh, is well represented in the Yale Museum by portions of a skull with teeth, both lower jaws, and a considerable part of the skeleton of the same animal. The lower jaws are much more slender than in *L. tyrannus*. The lower molars have their crowns more compressed, and the canine is but little larger at the base than the first lower premolar. This and the following premolar have each but one fang. The upper true molars closely resemble those of *Thinolestes anceps*.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of lower molar series, | 32· mm. |
| Extent of last three lower molars, | 17· |
| Antero-posterior diameter of last lower molar, | 7· |
| Transverse diameter, | 4· |
| Depth of jaw on posterior face below last lower molar, | 9· |
| Antero-posterior diameter of lower canine at base, | 2·3 |

The type specimen of this species was found, last September, at Grizzly Buttes, by Mr. J. F. Quigley, of the Yale party.

### *Orohippus pumilus*, gen. et sp. nov.

The remains on which the following description is principally based consist of two separate series of upper molar teeth, four of each. They indicate a new genus of small solipeds, nearly allied to *Anchitherium*, and which possibly may include the species described by the writer as *A. gracile*. The crowns of the upper true molars are composed of a pair of external cusps similar to those of *Anchitherium*. There are two corresponding inner tubercles, from which ridges extend obliquely to the anterior inner margin of the outer cusps, but the anterior ridge is divided so as to form an intermediate anterior tubercle. All the teeth preserved have a distinct basal ridge. The species was about the size of *Anchitherium gracile*, and appears to have had a long slender tail.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of four upper posterior molars, | 27· mm. |
| Antero-posterior diameter of last upper molar, | 7· |
| Transverse diameter, | 8· |
| Antero-posterior diameter of penultimate upper molar, | 7·5 |
| Transverse diameter, | 8·5 |

The specimens here described were found, in August last, at Grizzly Buttes, Wyoming, by Mr. G. M. Keasbey and the writer.

### *Helohyus plicodon*, gen. et sp. nov.

An interesting genus of small pachyderms, nearly related to *Hyracotherium*, is indicated by an upper molar tooth in perfect preservation. An isolated third upper premolar, and probably some other fragmentary specimens from the same deposits, may belong to the same species. The molar tooth is apparently the last of the upper series. The crown is composed mainly of four cones of nearly equal size. On the outer side, there is a pair of regularly conical tubercles, quite similar in size and shape. At the inner anterior corner, there is another cone, and behind, and partly inside of this, is the inner posterior cone. Between the anterior pair of cones, there is a smaller tubercle. The crown is surrounded by a strong basal ridge, and the entire surface of the enamel is delicately wrinkled. The species was about the same size as *Hyracotherium leporinum* Owen, from the London Clay.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of last upper molar, | 9· mm. |
| Transverse diameter, | 10·2 |
| Distance between summits of external pair of cones, | 4· |
| Distance between summits of anterior pair of cones, | 5·3 |

The known remains of this species were found by Mr. H. D. Ziegler and Mr. G. G. Lobdell, Jr., last summer, at Grizzly Buttes, near Fort Bridger, Wyoming.

### *Thinotherium validum*, gen. et. sp. nov.

A portion of a lower jaw containing the last true molar, and two isolated lower molars, which were found near, and may pertain to the same animal, indicate a new ungulate mammal, about as large as the preceding species. The ultimate molar resembles in its form and structure of crown the corresponding tooth of *Elotherium lentum* Marsh. The four principal tubercles are similar, and have the same relative position, but at the posterior extremity of the crown there is but a single small cusp, and the antero-interior cone was but slightly, if at all, divided. There is a marked lateral construction of the crown between the anterior and central pair of cones, and no basal ridge on the sides.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of last lower molar, | 11· mm. |
| Transverse diameter in front, | 7· |
| Transverse diameter through central pair of cones, | 6· |
| Distance between summits of central cones, | 3·1 |
| Antero-posterior diameter of first lower true molar, | 8·4 |

The only known specimens of this species were discovered last autumn, near Henry's Fork, by Mr. G. G. Lobdell, Jr.

### *Passalacodon litoralis*, gen. et sp. nov.

Several small mammals, evidently insectivores, about the size of the European hedge-hog, were among the interesting discoveries of the Yale party last year. One of these is at present known by a lower jaw with the last two molars perfect, and by a few other fragmentary remains. The jaw was rather slender and compressed, and much prolonged backward at the angle. The two molars preserved have their crowns composed of an anterior pair of high cusps, and a posterior pair less elevated. At the anterior and posterior margin of each molar, there is a small intermediate tubercle which projects beyond the border, especially in the penultimate molar. The rami were apparently not coössified. The inner incisor is large and near the symphysis.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of last lower molar, | 5· mm. |
| Transverse diameter, | 3· |
| Antero-posterior diameter of penultimate lower molar, | 4·8 |
| Transverse diameter, | 3·1 |
| Depth of jaw below first lower premolar, | 6· |

This unique specimen was found, in September last, near Henry's Fork, by Mr. J. F. Quigley, of the Yale party.

### *Anisacodon elegans*, gen. et sp. nov.

Another genus, nearly allied to the preceding one, may be established on a lower jaw with teeth, which belonged to an animal about the same size as the one last described. In the present specimen, the last lower molar is smaller than the penultimate. In both, the cavities between the cusps are much more deeply excavated than in the same molars of *Passalacodon*, and the small intermedial tubercles are less prominent.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of last lower molar, | 4· mm. |
| Transverse diameter, | 3·9 |
| Antero-posterior diameter of penultimate lower molar, | 4·6 |
| Transverse diameter, | 3·1 |
| Depth of lower jaw on posterior face below last molar, | 6·5 |

The only known specimen of this species was found, last autumn, by the writer, near Henry's Fork, Wyoming.

### *Centetodon pulcher*, gen. et sp. nov.

A small insectivorous mammal, not larger than a mole, is likewise indicated by part of a lower jaw, with the last true molar well preserved. Several other specimens in our collections probably should be referred to the same species. The lower molar is quite different from that of the recent small insectivores, and resembles somewhat the corresponding tooth in *Centetes*, although probably there is little affinity between the two genera. The posterior part of the crown is formed by a low tubercle, which is separated by a deep notch from the anterior elevated portion. The latter is composed of three pointed cones, the front one being the highest.

*Measurements.*

| | |
|---|---|
| Space occupied by last two lower molars, | 3·6 mm. |
| Antero-posterior diameter of last lower molar, | 1·8 |
| Transverse diameter, | 1·1 |
| Depth of jaw below last lower molar, | 3·5 |

The specimen here described was found, last September, by the writer, near Henry's Fork, Wyoming.

## PART III.

Nearly all the remains briefly described in this section of the present communication belonged to quite small animals, many of them insectivorous, and several evidently marsupials. In the complete description, now in course of preparation, these various species will be fully described, and their more exact affinities determined.

### *Stenacodon rarus*, gen. et sp. nov.

A new genus of very small mammals, apparently related distantly to *Hyopsodus*, may be established on a single last lower molar, in good preservation, which is one of the rarities of our collections. The crown of the tooth is remarkably narrow. It is composed essentially of four main cusps, nearly of the same size, and a larger posterior tubercle. The main cusps are arranged in two transverse pairs, and the posterior pair are the highest. There is no basal ridge. The species was somewhat smaller than *Hyopsodus paulus* Leidy.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of last lower molar, | 6·5 mm. |
| Transverse diameter through anterior pair of cones, | 2·9 |
| Transverse diameter through posterior tubercle, | 2· |
| Height of posterior pair of cones above jaw, | 2·7 |

The above specimen was found by the writer, last autumn, near Henry's Fork.

### *Antiacodon venustus*, gen. et sp. nov.

This species, which is about the same size as *Homacodon vagans*, is at present represented only by part of a lower jaw, with the characteristic lower molar, so often alone preserved. The crown of the present tooth has a similar composition to that of the same molar in *Homacodon*. The four principal cones stand in nearly opposite pairs, but the posterior tubercle is less widely separated from the central pair of cones, and the inner anterior cusp has its summit distinctly cleft. The crown, also, is proportionally shorter longitudinally. There is a distinct basal ridge on the front and outer sides of the crown.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of last lower molar, | 6·2 mm. |
| Transverse diameter through anterior pair of cones, | 3·6 |
| Transverse diameter through central pair of cones, | 3·6 |
| Height of anterior inner cusp above jaw, | 3·7 |

The only known specimen of this species was found, last September, by the writer, near Henry's Fork, Wyoming.

### *Bathrodon typus*, gen. et sp. nov.

In this genus, the first and second lower true molars have crowns with a similar composition to those of *Limnotherium*, but the anterior pair of cusps are more elevated, and the posterior pair are nearly equal in size. The last lower molar is quite different from the corresponding tooth in that genus. It is more like the preceding molar with the addition of a posterior tubercle, which is near the inner margin. The present species is based mainly on a portion of a lower jaw containing the last three molars. They indicate an animal about as large as *Limnotherium elegans* Marsh, and one probably allied to that species.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of last three lower molars, | 12·5 mm. |
| Antero-posterior diameter of last lower molar, | 5· |
| Transverse diameter, | 3·5 |
| Height of penultimate lower molar above jaw, | 2·6 |

The only known remains of this species were found by Mr. F. Mead, Jr., at Grizzly Buttes, near Fort Bridger, Wyoming, in September last.

### *Bathrodon annectens*, sp. nov.

Another species, about as large as a rabbit, is indicated by a fragment of a lower jaw with the last lower molar perfect. In its composition, and the position of its cusps, this tooth resembles the corresponding molar of *Anisacodon elegans*, but differs from it in having the anterior half of the crown narrower than the posterior portion. The former is elevated, and has its inner cusp the highest. There is no distinct basal ridge.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of last lower molar, | 5·8 mm. |
| Transverse diameter in front, | 3·4 |
| Transverse diameter through posterior half, | 3·5 |
| Depth of jaw on posterior face below last lower molar, | 10·6 |

This unique specimen was discovered, in September of last year, near Henry's Fork, by Mr. F. Mead, Jr., of the Yale party.

### *Mesacodon speciosus*, gen. et sp. nov.

This species and genus is based essentially on a nearly perfect lower jaw, with most of the teeth in good preservation. In its general features, the jaw resembles that of *Limnotherium*, and the molars are similar in composition to those of *L. elegans*, although considerably narrower. The teeth form a continuous series. The canine is large and compressed, and almost in contact with the symphysis. There are three premolars, and three true molars. The first premolar had but a single fang. The second is compressed, and the third is very similar to the next true molar. The last lower molar is narrower than the penultimate. The jaw is short, twisted longitudinally, and was not coössified with its fellow. The lower border was produced posteriorly, and the angle inflected. The remains indicate an animal about the size of the preceding species, and probably insectivorous.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of six lower molars, | 20·2 mm. |
| Extent of three lower true molars, | 12·2 |
| Antero-posterior diameter of last lower molar, | 4·3 |
| Transverse diameter, | 3· |
| Depth of jaw on posterior face below last lower molar, | 7· |

The jaw described above was found by the writer, last September, at Grizzly Buttes, Wyoming.

### *Hemiacodon gracilis*, gen. et sp. nov.

A genus of small mammals, apparently insectivorous, with molar teeth resembling those in *Mesacodon*, is well represented by portions of several lower jaws, and possibly by other characteristic remains. All appear to belong to the same species, which was somewhat smaller than that last described. The lower jaws preserved are rather slender and compressed. The teeth form a continuous series, and the dental formula appears to be as follows: Incisors 2, canine 1, premolars 3, and molars 3. There were evidently two small incisors, and the canine was but little larger. The first premolar has but a single fang. The two following are compressed, subtriangular at the base, and both quite different from the first true molar. There was an external basal ridge on the crowns of the lower molars. The jaws were not coössified at the symphysis. The lower margin was produced posteriorly, and the angle inflected.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of nine lower teeth, | 20·5 mm. |
| Extent of premolar and molar series, | 17·2 |
| Extent of true molars, | 11· |

| | |
|---|---|
| Antero-posterior diameter of last lower molar, | 4· mm. |
| Transverse diameter, | 2·4 |
| Depth of jaw below last lower molar, | 6·3 |

The type specimen of this species was discovered, last autumn, near Henry's Fork, Wyoming, by Mr. G. G. Lobdell, Jr. Other specimens were found in the same region by Mr. O. Harger, Mr. J. J. DuBois, and Mr. T. G. Peck, of the Yale party.

### *Hemiacodon nanus*, sp. nov.

A much smaller species, of the same genus apparently, is indicated by a right lower jaw, with the last four molars in perfect preservation. The teeth in this specimen have crowns with the same composition as in the preceding species, and there is the same marked difference between the last premolar and the first true molar. The teeth preserved have a distinct basal ridge. The jaw is less compressed than in the larger species. It indicates an animal about the size of a weasel, whose food was probably insects.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of last four lower teeth, | 11· mm. |
| Extent of three lower molars, | 9· |
| Antero-posterior diameter of last lower molar, | 3.5 |
| Transverse diameter, | 2· |
| Depth of jaw below first lower true molar, | 4·6 |

The specimen on which the above description is based was discovered by Mr. O. Harger, in September last, near Henry's Fork, Wyoming.

### *Hemiacodon pucillus*, sp. nov.

A still smaller species, not larger than a mole, is indicated by a fragment of a lower jaw containing the penultimate molar in good condition. The jaw was proportionally more compressed than in the specimen last described, but the tooth preserved agrees closely in the composition of its crown with the corresponding molar of that species. On the outer side of the crown, there is a distinct basal ridge.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of penultimate lower molar, | 2·3 mm. |
| Transverse diameter, | 2· |
| Height above jaw of anterior tubercles, | 1·7 |
| Depth of jaw below this molar, | 3·7 |

This specimen, the only known remains of the species, was found in September, 1870, by Mr. H. B. Sargent, of the Yale party. The locality was at Grizzly Buttes.

### *Centetodon altidens*, sp. nov.

A fragment of a lower jaw in our collections contains a penultimate molar, which agrees so nearly in its main features with the last lower molar of *Centetodon pulcher*, that the species it represents may provisionally be referred to that genus, although the species differ widely in size and other respects. The tooth preserved is remarkable for its great height above the jaw. The notch between the low posterior tubercle and the elevated anterior trifid portion of the crown is not so deep as in the small species, and more like that in the genus *Centetes*. There is a faint basal ridge around the crown, except on the inner side.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of last two lower molars, | 5·5 mm. |
| Antero-posterior diameter of penultimate lower molar, | 3· |
| Transverse diameter, | 2·3 |
| Height above jaw, | 3·1 |
| Depth of jaw below penultimate molar, | 6· |

This specimen was found by the writer, last autumn, near Henry's Fork.

### *Entomodon comptus*, gen. et sp. nov.

Another genus of insectivores is represented by several isolated teeth, one of the most characteristic of which is a last lower molar, in excellent preservation. This tooth is very narrow, and its crown is composed of a low posterior tubercle slightly bifid; a pair of elevated central cones, the outer being the highest, and slightly in advance of the other; and a small compressed anterior tubercle. Behind the inner central cone is a deep depression. There is no basal ridge. The tooth indicates an animal about as large as a weasel. It is probable that the small mammal referred provisionally by the writer to the genus *Viverravus*, under the name *V. nitidus*, may belong to the present genus. The species is rather smaller than the one here described.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of last lower molar, | 5·3 mm. |
| Transverse diameter through central cones, | 2·5 |
| Height of outer central cone above jaw, | 4·4 |
| Height of anterior tubercle above jaw, | 3·4 |

The type specimen of this species was found near Henry's Fork, last September, by Mr. G. M. Keasbey.

### *Entomacodon minutus*, gen. et sp. nov.

A small insectivorous mammal, about as large as a mouse, is indicated by a fragment of a lower jaw with the last molar perfect, and probably by some other equally characteristic remains. The crown of the lower molar has its main cusps very similar to those of *Entomodon*, but all are pointed. The outer of the three anterior points is the highest, and the posterior cusp is trifid. There is no basal ridge. An upper true molar found near this specimen, and perhaps belonging to the same animal, resembles in the composition of its crown the first upper true molar in the genus *Erinaceus*.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of last lower molar, | 2·2 mm. |
| Transverse diameter, | 1· |
| Height above jaw, | 2· |
| Depth of jaw below last lower molar, | 2·5 |

The specimens now representing this species were found, last autumn, near Henry's Fork, by Mr. O. Harger.

### *Centracodon delicatus*, gen. et sp. nov.

One of the treasures of our collections is a small, nearly perfect lower jaw, containing seven teeth, most of them in good preservation. The specimen clearly represents an insectivore, about as large as a mole, and apparently a marsupial. The jaw is slender, and its lower border regularly curved longitudinally. There were three true molars with pointed cusps, and four premolars more or less compressed. The first and second premolars are inclined forward, and separated from each other. The last molar has a low, sharp, posterior tubercle, and in front a high pointed external cusp, with two small inner tubercles.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of seven posterior lower teeth, | 12·8 mm. |
| Extent of last three molars, | 6· |
| Antero-posterior diameter of last lower molar, | 2· |
| Transverse diameter, | 1·2 |
| Depth of jaw below last lower molar, | 3· |

The above specimen was discovered, in September of last year, near Henry's Fork, by Mr. F. Mead, Jr.

### *Nyctilestes serotinus*, gen. et sp. nov.

An interesting genus of very small bats is indicated by part of a lower jaw with the last three molars perfect. On the outer side, these molars resemble those of *Nyctitherium*, but differ essentially from the same teeth in that genus in having the pair of pointed anterior tubercles of equal height. The jaw is slender, and proportionally less deep than in *Nyctitherium*. The present species was somewhat smaller than *N. velox*.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of last three lower molars, | 4·2 mm. |
| Antero-posterior diameter of last lower molar, | 1·3 |
| Transverse diameter, | 1· |
| Depth of jaw below last lower molar, | 2· |

This specimen, the only known remains of the species, was found by the writer, last September, at Grizzly Buttes, Wyoming.

### *Ziphacodon rugatus*, gen. et sp. nov.

A new carnivore, about the size of a civet cat, is represented in our Wyoming collections by the anterior part of a lower jaw, and probably by other less important remains. The premolars in this specimen have their main cusps peculiarly sharp and effective. Their anterior and posterior tubercles, also, are pointed, and placed near the base of the crown. The first premolar is large, and near the canine. The latter was of medium size, and inserted in the jaw more nearly vertically than in most carnivores. The adjoining incisor was large. The premolars have the enamel of the crown coarsely wrinkled.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of canine and five next lower teeth, | 30· mm. |
| Extent of four premolars, | 19·2 |
| Antero-posterior diameter of third premolar, | 6· |
| Transverse diameter, | 3· |
| Depth of jaw below fourth premolar, | 8·5 |

The type specimen of this species was found by Mr. J. J. DuBois. The locality was near Henry's Fork.

### *Harpalodon sylvestris*, gen. et sp. nov.

Another new genus of carnivores, apparently allied to *Viverravus*, is indicated, also, by part of a lower jaw containing the last two premolars, and the following tubercular molar. The former differ from the corresponding teeth in *Viverravus*, in the absence of the upper posterior tubercle on the third lower premolar. On the fourth premolar, this tubercle is rudimentary. The premolars preserved are much compressed, and have the anterior tubercle well developed. The present species was somewhat larger than *V. gracilis* Marsh.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of last two premolars and first true lower molar, | 16·2 mm. |
| Antero-posterior diameter of first true molar, | 5·3 |
| Antero-posterior diameter of last premolar, | 5·8 |
| Transverse diameter, | 2·7 |
| Height above jaw, | 4·6 |

The above specimen was found by Mr. O. Harger, near Henry's Fork.

### *Harpalodon vulpinus*, sp. nov.

A larger species, apparently of the same genus as the preceding, may be based on part of a lower jaw containing the last premolar. Some other isolated specimens probably belong to the same species. The premolar has the posterior tubercle proportionally more developed than in *H. sylvestris*, and is consequently broader at this part of the crown. The main cusp is less elevated.

*Measurements.*

| | |
|---|---|
| Space occupied by four lower premolars, | 22· mm. |
| Antero-posterior diameter of last premolar, | 7· |
| Transverse diameter, | 2·3 |
| Height above jaw, | 4·3 |
| Depth of jaw below last premolar, | 8· |

The known remains of this species were found by Mr. T. G. Peck, in September last, near Henry's Fork, Wyoming.

### *Orotherium Uintanum*, gen. et sp. nov.

This genus is nearly allied, apparently, to *Lophiotherium*, but differs from the known remains of that genus in having, on the second lower premolar, a prominent posterior tubercle. In the true lower molars, also, the anterior inner cone is slightly bifid, a character not indicated in the figures given of the corresponding teeth of *Lophiotherium*. The present species is based on a nearly entire lower jaw, with the last six teeth in perfect preservation. The first true lower molar is very similar to the fourth premolar, but is rather broader anteriorly. The second premolar is narrow, with the anterior cusp compressed and separated from the posterior tubercle by a wide notch. The lower teeth resemble those of *Lophiotherium sylvaticum* Leidy, as well as those of the smaller species, *L. Ballardi* Marsh, and both these species should doubtless be placed in the genus *Orotherium*. The relations of this genus to *Orohippus* cannot at present be fully determined.

*Measurements.*

| | |
|---|---|
| Longitudinal extent of last six lower teeth, | 47· mm. |
| Extent of last three lower molars, | 26·5 |
| Antero-posterior diameter of last lower molar, | 11·5 |
| Transverse diameter, | 5· |
| Antero-posterior diameter of fourth lower premolar, | 7· |
| Transverse diameter, | 5· |

This specimen was found near Henry's Fork, Wyoming, by the writer, in September last.

### *Helaletes boops*, gen. et sp. nov.

Among the Tapiroid mammals in the Green River Tertiary deposits, there are two distinct genera which have been referred to *Lophiodon.* Dr. Leidy has given the name *Hyrachyus* to one of these, which embraces the larger species, and the other may be called *Helaletes.* In this genus the last lower molar has a third, posterior lobe. The upper molars resemble those of *Lophiodon.* The astragalus differs widely from the Tapiroid type, and in its narrow oblique condyles is very similar to that of the Equidæ. Other characters of the genus will be given in the full description.

The present species is based upon the greater portion of a skull with teeth, and the more important parts of the skeleton of the same individual. The teeth agree in size with those of *Lophiodon nanus* Marsh, but the last upper molar has a small tubercle on the outer margin between the cusps, which appears to be wanting in the type specimen of the latter species. There are also other differences of importance. Both species doubtless belong to the same genus.

*Measurements.*

| | mm. |
|---|---|
| Longitudinal extent of last three lower molars, | 33· |
| Antero-posterior diameter of last lower molar, | 12·2 |
| Depth of jaw below last lower molar, | 21· |
| Antero-posterior diameter of last upper molar, | 11·1 |
| Transverse diameter, | 11·5 |
| Length of astragalus, | 27· |
| Width between condylar ridges, | 11·5 |

The type specimen of this species was discovered, last autumn, at Grizzly Buttes, Wyoming, by Mr. G. G. Lobdell, Jr.

## PART IV.

### *Paramys robustus*, sp. nov.

A new rodent, with lower molar teeth similar to those of *Paramys*, but a much larger animal than the known species of that genus, is represented by two lower molars, and by other less important remains. An isolated incisor probably belongs to the same species. The lower molars preserved are proportionally broader than those in *Paramys delicatus* Leidy, and the penultimate molar is more constricted medially, the notch on the inner side being nearly as deep as the external depression. The known remains of this species indicate an animal somewhat larger than the common woodchuck (*Arctomys monax* Gm.).

*Measurements.*

| | mm. |
|---|---|
| Antero-posterior diameter of last lower molar, | 6·5 |
| Transverse diameter, | 5·5 |

| | mm. |
|---|---|
| Antero-posterior diameter of penultimate lower molar, | 6· |
| Transverse diameter, | 6·4 |
| Transverse diameter of incisor, | 4· |

The specimens above described were found by Mr. G. G. Lobdell, Jr., and Mr. G. M. Keasbey, in the lower Tertiary deposits of Grizzly Buttes and Henry's Fork, Wyoming.

### *Tillomys senex*, gen. et sp. nov.

A small rodent, about the size of a rat, is represented in our Wyoming collections by a fragment of a lower jaw with the second molar in place, and apparently by some other uncharacteristic remains. The crown of this molar is somewhat worn, but was composed essentially of an anterior transverse crest, and a pair of posterior tubercles, the outer and larger one being connected by a small intermediate tubercle with the outer posterior angle of the anterior lobe. The jaw in the present species is slender, and the tubercle at the anterior margin of the masseteric fossa is under the center of the second molar.

*Measurements.*

| | mm. |
|---|---|
| Space occupied by four lower molars, | 11· |
| Antero-posterior diameter of second lower molar, | 2· |
| Transverse diameter, | 1·8 |
| Depth of jaw below second molar, | 5· |

The known remains of this species were found, in September last, by the writer, near Henry's Fork.

### *Tillomys parvus*, sp. nov.

A smaller species, which appears to agree generically with the specimen last described, may be established on a lower jaw, also containing the second molar. In this tooth, the longitudinal connecting ridge, or tubercle, between the anterior and posterior parts of the crown, is more nearly central. The masseteric fossa, moreover, terminates in front just at the anterior margin of the last lower molar. The species was but little larger than a mouse.

*Measurements.*

| | mm. |
|---|---|
| Length of lower molar series, | 7·1 |
| Antero-posterior diameter of second lower molar, | 1·9 |
| Transverse diameter, | 1·5 |
| Space between incisor and first lower molar, | 3· |

This specimen was found by Mr. O. Harger, at Grizzly Buttes, Wyoming, in September last.

### *Taxymys lucaris*, gen. et sp. nov.

The existence of another small rodent, evidently belonging to the Sciuridæ, is clearly proved by a fragment of an upper jaw, with the first two molars in position. A vertebra found with this specimen probably belonged to the same animal, which was rather smaller than the common flying squirrel (*Pteromys volucella* Desm.). The first upper molar is nearly round, and diminutive, as in many of the recent squirrels. The second upper molar has its crown composed of two main transverse ridges, which start from the outer side, and meet in a prominent inner and slightly bifid tubercle. There are two low ridges outside of the main pair, which meet in the same tubercle.

*Measurements.*

| | |
|---|---|
| Space occupied by first two upper molars, | 2·5 mm. |
| Antero-posterior diameter of second upper molar, | 1·7 |
| Transverse diameter, | 2· |
| Transverse diameter of first upper molar, | 1· |

The above remains were found, last autumn, by the writer, near Henry's Fork, Wyoming.

### *Sciuravus parvidens*, sp. nov.

A diminutive rodent, with molar teeth resembling those of *Sciuravus*, is evidently represented in our collections by a lower jaw containing the third molar, part of an upper jaw with the penultimate molar, and several isolated teeth. These remains indicate an animal about half the bulk of *Sciuravus undans* Marsh. The lower incisor is more convex in front than in that species. The jaw is short and deep, and the masseteric fossa ends in front under the second molar. The upper penultimate molar has three fangs, and the last one had four.

*Measurements.*

| | |
|---|---|
| Space occupied by three posterior lower molars, | 5·2 mm. |
| Antero-posterior diameter of third lower molar, | 2· |
| Transverse diameter, | 1·9 |
| Depth of jaw below second lower molar, | 5·3 |
| Transverse diameter of lower incisor, | 1·2 |

The remains at present representing this species were found, last autumn, at Henry's Fork and Grizzly Buttes, Wyoming, by Mr. G. M. Keasbey and the writer.

### *Colonymys celer*, gen. et sp. nov.

Another small rodent, about the size of the animal last described, is indicated by several isolated molars, which differ widely from the corresponding teeth in any genus of this group from the Green River Tertiary deposits. A typical upper molar in perfect condition has its crown composed of four principal cusps, regularly arranged in two pairs. The outer pair are entirely separated from each other. The inner pair are very near together, and have their summits turned toward the center of the tooth. The two anterior cusps are connected by an outward curving basal ridge. A similar ridge passes from the outer posterior cone inward, but does not reach the opposite cusp. This upper molar, which is apparently the penultimate, measures 2·4$^{\text{mm.}}$ in antero-posterior diameter, and 2·1$^{\text{mm.}}$ in transverse diameter. The known remains of the present species were found, last autumn, near Henry's Fork, by Mr. H. D. Ziegler and Mr. A. B. Mason, of the Yale party.

### *Apatemys bellus*, gen. et sp. nov.

A new and peculiar genus of very small mammals may be based on part of a lower jaw, with the penultimate molar in place, and well preserved. Extending through the base of the specimen is a portion of a large rodent-like incisor. The molar is of the insectivore type, and the animal should probably be placed with that group. The crown of this tooth has its posterior half deeply excavated, with the outer rim of the cavity the highest, and rising at the inner posterior angle into a pointed tubercle. The anterior half of the crown consists of a transverse pair of high pointed cusps, opposite each other, and connected together. An elevated basal ridge, rising into a sharp inner cusp, completes the anterior margin. The large incisor is oval in transverse section, and extends under all the molars. The animal was about as large as a mole.

*Measurements.*

| | | |
|---|---|---|
| Antero-posterior diameter of penultimate lower molar, | 2·3 | mm. |
| Transverse diameter, | 2· | |
| Depth of jaw below, | 5·6 | |
| Transverse diameter of incisor, | 1·6 | |

This unique specimen was found, last September, near Henry's Fork, by Mr. G. G. Lobdell, Jr.

### *Apatemys bellulus*, sp. nov.

Another diminutive mammal, apparently of the same genus, but somewhat smaller than the species last described, is well represented by a lower jaw with the last three molars perfect. The penultimate molar agrees in the composition of its crown with that of *A. bellus.* In the last lower molar, the outer border of the posterior cavity forms a slightly curved longitudinal ridge, which terminates in a small tubercle. The molar teeth are narrow, and the jaw compressed. The cavity for the incisor extends below all the lower molars.

*Measurements.*

| | | |
|---|---|---|
| Space occupied by last three lower molars, | 6· | mm. |
| Antero-posterior diameter of last lower molar, | 2·1 | |
| Transverse diameter, | 1·6 | |
| Antero-posterior diameter of penultimate lower molar, | 2· | |
| Transverse diameter, | 1·7 | |

This interesting fossil was found, last autumn, near Henry's Fork, Wyoming, by Mr. G. M. Keasbey.

### *Entomacodon angustidens*, sp. nov.

A very small insectivore, which appears to belong to the genus *Entomacodon*, left its remains in the Green River Tertiary deposits, and our collections contain a left lower jaw, with the last premolar and the two following molars in good preservation. The molars agree essentially in the composition of their crowns with those of *E. minutus*, but the three anterior cusps are nearer together, and the one in front is nearly as high as the others. The specimen indicates, also, a smaller animal than the type of that species. The jaw is much compressed, and the teeth are very narrow. The last premolar resembles the adjoining molar, but has the anterior cusp rudimentary.

*Measurements.*

| | |
|---|---|
| Space occupied by last four lower teeth, | 5·5 mm. |
| Antero-posterior diameter of last lower premolar, | 1·6 |
| Antero-posterior diameter of penultimate lower molar, | 1·7 |
| Transverse diameter, | 1· |
| Depth of jaw below last lower molar, | 2·5 |

This specimen was found by Mr. J. J. DuBois, at Grizzly Buttes, Wyoming, in September of last year.

### *Triacodon grandis*, sp. nov.

The genus *Triacodon* was established, by the writer, on a single lower premolar, which differed widely from any corresponding tooth then known. The species thus represented was called *T. fallax*, and its possible marsupial affinities were suggested.* The researches of the Yale party during the past year, in the same region where the original specimen was found, have added something to the knowledge of this peculiar mammal, but much still remains to be ascertained. A portion of a skeleton was found at Grizzly Buttes by the writer, and fortunately the peculiar premolar was in the jaw, although the other teeth were too imperfect for study. The jaws were stout, but not deep, and the lower border was convex longitudinally. There was a prominent canine, and the last lower molar was tubercular. The skull had a distinct sagittal crest. The humerus was slender, and curved as in the otter. The animal was probably a carnivorous marsupial.

A second and much larger species of the genus is represented by the corresponding right lower premolar, in excellent preservation. It agrees closely in its main features with the type specimen of *T. fallax*, but has a distinct antero-external basal

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* This Journal, vol. ii, p. 123, August, 1871.

ridge. It indicates, moreover, an animal several times the bulk of that species. Probably other remains of both species were found by our party, but have not been recognized as pertaining to this genus.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of last lower premolar, | 5· mm. |
| Transverse diameter, | 5·8 |
| Height of anterior cusp above jaw, | 5·2 |
| Height of exterior cusp above jaw, | 8· |

This rare specimen was found, by Mr. O. Harger, in the shale near Henry's Fork. The geological horizon was essentially the same as at Grizzly Buttes.

### *Triacodon nanus*, sp. nov.

A much smaller species, apparently not more than one-half the bulk of *T. fallax*, is indicated, likewise, by the peculiar last lower premolar. This tooth, like that in the other species, has two fangs. There is a faint basal ridge, and the three pointed cusps, that give the crown its character, are more nearly of equal size than in either of the larger species.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of last lower premolar, | 3· mm. |
| Transverse diameter, | 3· |
| Height of anterior cusp above jaw, | 2·5 |
| Height of exterior cusp above jaw, | 4· |

This specimen, the only remains of the species at present known, was discovered at Grizzly Buttes, in September last, by Mr. G. G. Lobdell, Jr.

### *Euryacodon lepidus*, gen. et sp. nov.

A small mammal, doubtless an insectivore, is represented by a fragment of an upper jaw containing the last two molars in perfect condition. Our collections contain other characteristic fossils which appear to be specifically identical with this specimen. The teeth preserved agree nearly in the composition of their crowns with the molars described by Dr. Leidy under the name, *Palæacodon verus*, but each has its inner margin produced into a small tubercle. In the penultimate upper molar, this tubercle is especially prominent. The outer margin, also, of these molars has but a single faint indentation between the external cusps. Both teeth are surrounded by a distinct basal ridge. The specimens preserved indicate an animal about as large as a weasel.

*Measurements.*

| | |
|---|---|
| Space occupied by last two upper molars, | 4·3 mm. |
| Antero-posterior diameter of penultimate upper molar, | 2·4 |
| Transverse diameter, | 3·8 |
| Transverse diameter of last upper molar, | 3·2 |

The type specimen of this species was found, last autumn, at Grizzly Buttes, by the writer, and other remains were discovered, at the same locality, by Mr. O. Harger.

### *Palæacodon vagus*, sp. nov.

Another small insectivore, about the same size as the preceding species, is clearly indicated by part of an upper jaw, with the last three molars perfect. In this specimen, the last two upper teeth are broader transversely than in *Euryacodon lepidus*, and both lack the inner tubercle. The outer margin of the crown and the basal ridge are very similar in the corresponding teeth of the two specimens. The first upper true molar has externally a deeper notch between the outer cusps, and at each external angle the basal ridge rises into a small tubercle. It is possible that the small tooth mentioned by Dr. Leidy, in his description of *Palæacodon verus*, may prove to belong to the present species. In that case his specific name may be retained for the animal represented by the larger tooth, which mainly is described, and the smaller species may bear the name here given.

*Measurements.*

| | |
|---|---|
| Space occupied by last three upper molars, | 7· mm. |
| Antero-posterior diameter of penultimate upper molar, | 2·7 |
| Transverse diameter, | 4· |
| Antero-posterior diameter of last upper molar, | 2·1 |
| Transverse diameter, | 3·6 |

The only known remains of this species were found at Grizzly Buttes, Wyoming, by Mr. F. Mead, Jr.

Yale College, New Haven, August 15th, 1872.

### *Postscript.*

The original specimens of the fossil mammals described by the writer in this and preceding articles are carefully preserved in the Museum of Yale College. The dates of publication of the principal papers are as follows: The two articles in Volume II. of this Journal (pages 35–44, and 120–127, July and August, 1871) were published together, in pamphlet form, and widely distributed, June 21st, 1871. The species there described, therefore, bear that date. The various parts of the present communication were issued separately as follows:—Part I, July 22d, 1872; Part II, August 7th, 1872; Part III, August 13th, 1872; and Part IV, August 17th, 1872,—the date of publication being printed on each pamphlet.

The brief descriptions here given are merely preliminary to a full description, with illustrations, now in preparation.

Yale College, August 19th, 1872.

## ART. XXIX.—*On certain Relations between the mean motions of the Perihelia of Jupiter, Saturn, Uranus and Neptune;* by Professor DANIEL KIRKWOOD.

IN Mr. Stockwell's able memoir* on the secular variations of the planary orbits, it is shown that the mean motions of the perihelia of Saturn, Uranus and Neptune are as follows:

| | |
|---|---|
| Saturn, | 22″.4608479 |
| Uranus,† | 3.7166075 |
| Neptune, | 0.6166849 |

Denoting these values by $N^{vi}$, $N^{vii}$, and $N^{viii}$, respectively, we have,

$$N^{vi}-7N^{vii}+6N^{viii} = 0''.1447048, \quad . \quad . \quad . \quad (1)$$

As these quantities depend upon the masses of the planets, some of which are not very accurately known, it seems probable that the second member of (1), if precisely determined, would be 0. The influence of the small planets, Mercury, Venus, the Earth and Mars, is, in this case, quite inconsiderable; no probable change in the received masses of Uranus and Neptune would reduce the second member of (1) to 0; and, finally, the mass of Jupiter is more accurately determined than that of any other planet. There remains, then, the large planet Saturn, the received value of whose mass is derived from Bessel's measurements of the elongation of the sixth satellite. The value of this element, according to the Königsberg astronomer, is 176″.55. Now, it will be found that a value of 177″.17 (which exceeds Bessel's by 0″.62), corresponds to a mass, $\frac{1}{34646}$, which substituted in Stockwell's formulas renders the equation

$$N^{vi}-7N^{vii}+6N^{viii} = 0, \quad . \quad . \quad . \quad (2)$$

accurately true. The difference, 0″.62, is less than that between the determinations of the equatorial diameter of Saturn by the best observers.

If equation (2) be exact, the corresponding relation between the mean longitudes of the perihelia will be obvious, and it must follow that *the perihelia of no three of the four outer planets can simultaneously have the same mean longitude.* In short, if $L^{v}$, $L^{vi}$, $L^{vii}$ and $L^{viii}$, represent the mean longitudes of the perihelia of Jupiter, Saturn, Uranus and Neptune, respectively,

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* Smithsonian Contributions, Washington, 1872.

† The mean motion of Jupiter's perihelion is precisely the same.

their mutual relations will probably be expressed by the following equations:

$$L^{vi}-7L^{vii}+6L^{viii}=180^\circ, \quad . \quad . \quad . \quad (3)$$
$$L^{v}-L^{vi}+6(L^{vii}-L^{viii})=0, \quad . \quad . \quad . \quad (4)^*$$

## SCIENTIFIC INTELLIGENCE.

### I. Chemistry and Physics.

1. *On the ammoniacal platinum bases.*—Cleve has continued his elaborate investigation of the ammoniacal compounds of platinum, and has described a number of new compounds containing aniline and ethylamin. By heating chloride of plato-semi-diamine in a sealed tube, with weak alcohol and an excess of aniline, a white crystalline powder is obtained, which has the formula,

$$Pt\left\{\begin{matrix}2NH_3\\2(NH_7C_6)\end{matrix}\right\}Cl_2.$$

This salt gives with chlorplatinite of potassium a voluminous rose-colored precipitate corresponding to the green salt of Magnus, and having the formula,

$$Pt\left\{\begin{matrix}2NH_3\,.\,Cl\,.\,Cl\\2(NH_7C_6)Cl\,.\,Cl\end{matrix}\right\}Pt.$$

The nitrate and oxalate of this base have respectively the formulas,

$$Pt\left\{\begin{matrix}2NH_3\\2(NH_7C_6)\end{matrix}\right\}2N\Theta_3,\text{ and }Pt\left\{\begin{matrix}2NH_3\\2(NH_7C_6)\end{matrix}\right\}C_2O_4+\Theta H^2.$$

By evaporating the chloride of this base with aniline, water, and a little alcohol, a new chloride is obtained containing one atom less of aniline, and having the formula,

$$Pt\left\{\begin{matrix}2NH_3\\NH_7Cl_6\end{matrix}\right\}Cl_2.$$

By the action of iodine upon the first chloride ammonia is disengaged, and a yellow powder having the formula,

$$Pt\left\{\begin{matrix}NH_3\\NH_7C_6\end{matrix}\right\}I_2,$$

is precipitated. We may consider this iodide as derived from the chloride,

$$Pt\left\{\begin{matrix}NH_3\,.\,NH_3\\NH_7C_6\,.\,NH_7C_6\end{matrix}\right\}Cl_2,$$

by the loss of the two external molecules, $NH_3$ and $NH_7C_6$. Under the same circumstances the isomeric chloride,

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* In the memoir already referred to, Mr. Stockwell has shown that while "the mean angular distance between the perihelia of Jupiter and Uranus is exactly 180°," the longitude of each may differ from its mean place to a considerable extent. In fact, the present angular distance is but 157°.

$$Pt\left\{\begin{matrix}NH_3 \,.\, NH_7C_6\\ NH_3 \,.\, NH_7C_6\end{matrix}\right\}Cl_2,$$

disengages the two external molecules of aniline, and we have iodide of platosamin,

$$Pt\left\{\begin{matrix}NH_3 \,.\, I\\ NH_3 \,.\, I.\end{matrix}\right.$$

The second chloride above mentioned unites also with platinous chloride to form a part which crystallizes in very brilliant, thin, micaceous scales.

When aniline is dissolved in a concentrated boiling solution of sulphate of plato-semi-diamine, colorless prismatic crystals are obtained having the formula,

$$Pt\left\{\begin{matrix}2NH_3\\ 3NH_7C_6\end{matrix}\right\}SO_4.$$

When chloride of platosamine, $Pt(NH_3 \,.\, Cl)_2$, is heated with aniline, water, and a little alcohol, thin nacreous scales are obtained, which have the formula,

$$Pt\left\{\begin{matrix}NH_3 \,.\, NH_7C_6\\ NH_3 \,.\, NH_7C_6\end{matrix}\right\}Cl_2,$$

and are isomeric with the salt first described. The chlorplatinite of this compound is a slightly soluble, crystalline, buff-colored powder. The author describes also the normal sulphate and nitrate.

By boiling the chloride of plato-semi-diamine with ethylamine, Clève obtained a chloride having the formula,

$$Pt\left\{\begin{matrix}2NH_3\\ 2NH_7C_2\end{matrix}\right\}Cl_2,$$

which gives a beautiful green chlorplatinite. The solution of the chloride gives with potassic iodide a precipitate having the formula,

$$Pt\left\{\begin{matrix}NH_3\\ NH_7C_2\end{matrix}\right\}I_2.$$

The isomeric chloride,

$$Pt\left\{\begin{matrix}NH_3 \,.\, NH_7C_2\\ NH_3 \,.\, NH_7C_2\end{matrix}\right\}Cl_2,$$

is easily obtained by the action of ethylamine upon chloride of platosamine. It is much less soluble than the compound first named. It yields a slightly soluble, green chlorplatinite, and a normal sulphate with 6 atoms of water of crystallization.—*Bull. de la Société Chimique*, 1872, p. 294. W. G.

2. *On compounds containing phosphorus and platinum.*—SCHÜTZENBERGER has continued his investigations of the very remarkable compounds of platinous chloride, and has arrived at interesting results. When the compound, $P(C_2H_5O)_3PtCl_2$, is treated with ammonia, it becomes fluid and gradually dissolves. The solution gives on evaporation the chlorhydrate of a new base,

$$PN(C_2H_5O)_3H_3Pt \,.\, N_2H_4 \,.\, 2HCl.$$

The compound, $P_2(C_2H_5O)_6PtCl_2$, also dissolves in ammonia, and yields the chlorhydrate,

$$P_2(C_2H_5O)_6Pt\,.\,N_2H_4\,.\,2HCl.$$

The methyl compounds, $P(CH_3O)_3PtCl_2$ and $Pt_2(CH_3O)_6PtCl_2$, yield similar compounds. It appears probable that the radical $P(C_2H_5O)_3Pt$ may be isolated by the action of zinc on the chloride. All of these compounds are derived from the bodies, $PCl_3PtCl_2$ and $(PCl_3)_2PtCl_2$, which are formed by the action of phosphoric chloride, $PCl_5$, upon spongy platinum. By the action of these substances upon alcohol the compounds, $P(C_2H_5O)_3PtCl_2$ and $P_2(C_2H_5O)_6PtCl_2$, are formed.—*Berichte der Deutschen Chem. Ges., Jahrgang* v, p. 390. W. G.

3. *On the specific heat of carbon.*—H. F. WEBER has re-determined the specific heat of carbon in the form of diamond and of graphite. The author employed the ice calorimeter of Bunsen, and arrived at the remarkable result that the specific heat of carbon increases with the temperature more rapidly than that of any other known substance. In the case of diamond it is trebled for an increase of temperature from 0° to 200° C. The author's experiments were made with two diamonds weighing respectively 447 and 634 milligrammes. Preliminary trials showed that these had the same specific heats within the limits of the necessary errors of experiment. For twelve different temperatures almost uniformly distributed in the interval from 0° to 200° thirty-three observations were made. These gave for the mean specific heat the parabolic formula:

$$Co-t = 0{\cdot}0947 + 0{\cdot}000497t - 0{\cdot}00000012t^2.$$

By means of this formula the true specific heat, or the quantity of heat necessary to raise the temperature of the unit of weight one degree at t°, may be determined, since we have:

$$t\,.\,Co-t = \int_0^t \gamma t\,.\,dt$$

and $$\lambda t = 0{\cdot}0947 + 0{\cdot}000994t - 0{\cdot}00000036t^2.$$

This gives the following values for diamond:

$0^\circ \quad \gamma = 0{\cdot}0947$
$50^\circ \quad \gamma = 0{\cdot}1435$
$100^\circ \quad \gamma = 0{\cdot}1905$
$150^\circ \quad \gamma = 0{\cdot}2357$
$200^\circ \quad \gamma = 0{\cdot}2791$

In the case of graphite the author, from want of snow, made only two determinations. These gave the equations:

$$\left.\begin{array}{r} Co-t = 0{\cdot}1167 + 0{\cdot}0008t \\ \lambda t = 0{\cdot}1167 + 0{\cdot}0016t \end{array}\right\}$$

These results explain the discrepancies in the determinations of the specific heats of carbon by other physicists. They show that diamond at about 525° C. would have a specific heat of 0·52 or $\frac{6{\cdot}3}{12}$, as the law of Dulong and Petit requires. Weber considers his result as furnishing a strong argument against the law in question, as in his opinion the law loses all chemical and physical value as soon as it is shown to depend essentially upon the temperature. The author promises a further investigation of this subject, which is of great importance for the relation between true specific heat and internal work.—*Berichte der Deutschen Chem. Ges., Jahrgang* v, p. 303. W. G.

4. *What determines Molecular Motion?—The Fundamental Problem of Nature;* by Mr. JAMES CROLL.—Mr. Croll closes an article of 25 pages in the July number of the Philosophical Magazine with the following remarks:

"Molecular physics has made great advance of late years; but it has not made much advance in that particular direction which can be of service in explaining how molecular motion in organic nature is determined. It is thought, however, by the advocates of the physical school that although at present we are unable to explain how organic nature can be built up by the play of the ordinary chemical and physical forces, yet at some future day, when we shall have come to know far more of molecular physics than we do at present, then we may be able to explain the mystery. This is the cherished hope of modern evolutionists, and of the advocates of the physical theory of life. But it is a mental delusion, a dream which will never be realized. A little consideration might satisfy any one that chemistry and physics will never explain the mystery of nature.

The terms light, heat, electricity, magnetism, &c., are different names which we apply to different modes of molecular motion; and it is true that at present little is known regarding the nature of these modes of motion; but notwithstanding this we have reason to conclude that, although we knew all that absolutely can be known regarding them, yet it *would not afford us any explanation* of the cause of the determination of molecular motion in organic nature.

The character of a cause may often to some extent be judged indirectly from the nature of the effects produced. It is from the effects produced that we know, for example, that that mode of molecular motion called heat differs from that mode called electricity. The effects do not as yet enable us to determine wherein this difference consists; but it enables us to conclude with certainty that there is a difference. Effects which are electrical we refer to that unknown mode of motion called electricity. We do not refer them to that mode called heat, because the effects are different from those which we ascribe to heat. Each mode of motion, each energy, is distinguished by the effects which it produces. Determination of the molecules of matter according to the objective idea of a plant or an animal, is an effect which is constantly taking place in organic nature. To attribute this effect to electricity, for example, would be far more absurd than to attribute electrical effects to gravitation or to heat; for the difference between this effect and any electrical effect is immeasurably greater than between electrical effect and any effects produced by heat, or by gravitation, or by any other of the forces of inorganic nature. It would be far more rational to attribute all the phenomena of the inorganic world, say, to heat, than to attribute the determination of molecular motion in the organic world to chemical and physical energies.

It must now be obvious that nothing which can be determined by the comparative anatomist, no biological researches, no microscopic investigations, no considerations regarding natural selection or the survival of the fittest, can solve the great problem of nature; for it lies in the background of all such investigations. The problem is molecular. From the hugest plant and animal on the globe down to the smallest organic speck visible under the microscope, all have been built up molecule by molecule; and the problem is, to explain this molecular process. If one plant or animal differs from another, or the parent from the child, it is because in the building-up process the determinations of molecular motion were different in the two cases; and the true and fundamental ground of the difference must be sought for in the cause of the determination of molecular motion. Here in this region the doctrine of natural selection and the struggle for existence can afford no more light on the matter than the fortuitous concourse of atoms and the atomical philosophy of the ancients. This, I trust, will be rendered still more evident when we come to examine in detail the arguments advanced by modern evolutionists in support of thier fundamental hypothesis, 'that the whole world, living and not living, is the result of the mutual interaction, according to definite laws, of the forces possessed by the molecules of which the primitive nebulosity of the universe was composed.' "—*Phil. Mag.*, xliv, 24.

5. *On the Separation of Yttria and Ceria from Zirconia and Iron;* by J. W. Taylor, F.G.S. (From a letter to one of the Editors.)—The solution in HCl is precipitated by ammonia, and boiled for a few minutes. An excess of oxalic acid is then added and the whole boiled for half an hour; the zirconia and iron will be dissolved and the yttria and ceria remain as a white precipitate, which, washed, dried and ignited, is redissolved in HCl, again precipitated, and the moist precipitate, dissolved in as little acid as possible, dropped into a strong solution of carbonate of ammonia —which will dissolve the yttria precipitate. A slight trace of ceria will also be dissolved? which may be eliminated by repeating the process.

## II. Geology and Natural History.

1. *On Quebec and Carboniferous Rocks in the Teton Range;* by Prof. F. H. Bradley, of the Hayden Exploring Expedition, from a letter to J. D. Dana, dated Teton Cañon, Idaho, July 26th, 1872.—I have to report the discovery of a few small trilobites, unmistakably of Quebec Group age, in the base of the mass of limestones which overlie the central granites of this Teton range.

The limestones are continuous up to the typical Carboniferous, full of *Productus*, *Zaphrentis*, etc. That which I can consider as plainly Quebec Group is about 400 feet thick; partly argillaceous, blue and mostly pebbly; then follows about 600 feet of a drab to pale buff, somewhat vesicular, magnesian limestone, entirely without fossils, which I am obliged to consider, from both character and position, as representing the "cliff" limestone of the Mississippi valley; and this, I believe, is immediately overlaid by true Carboniferous, though the direct connection was covered where I made the section. From the character of the section here, and its similarity to that seen in Malade valley, I am led to expect similar results there when I work out its details, and that this same Quebec Group age must finally be made to include the basal portion of the so-called Carboniferous limestone, through a large portion of this region. The fossils are so few and fragmentary, at least in most places, that it is not surprising that they have been overlooked heretofore. But for having suspected the age from the character of the rock, I should probably have given up the search long before finding the fossils.

We start to-morrow for the ascent of the Grand Teton, and are sanguine of success, although the profile of the easiest slope shows in *one* part a rise of 63°, and *most* of the peak reaches 48°.

Dr. Hayden is said to have left Bozeman on the 17th inst. We hope to meet him in the Firehole Basin about August 20th.

2. *On Changes of Climate during the Glacial epoch;* by JAMES GEIKIE, F.R.S.E. (Geol. Mag., vols. viii, ix.) Professor Geikie discusses in this memoir of 70 pages the Scottish glacial deposits and their relation to those in England and Ireland, the valley grounds and cave deposits of England, the later glacial deposits of Switzerland and Italy, and adopts Mr. Croll's theory of the physical cause of changes of climate, that referring it to the eccentricity of the earth's orbit.

The following are the conclusions, which the author considers as established.

(1.) That at some distant period (according to Mr. Croll's calculations, upward of 200,000 years ago), owing to the eccentricity of the earth's orbit being at a high value, and the winter of our hemisphere happening to fall in aphelion, a climate of intense severity covered Scotland, Ireland, and the major portion of England with a massive sheet of snow and ice. At the same time similar conditions characterized the mountainous and northern regions of Europe and America.

(2.) That the greater contours of the land were assumed at a much earlier date than the advent of the Glacial epoch, and therefore guided the flow of the ice from the high grounds to the sea.

(3.) That, while the ice moved along the line of the principal valleys, it yet disregarded minor undulations of the ground, and overflowed considerable hills.

(4.) That the till, "grundmoränen," and "moraines profondes" are the materials which gathered underneath the ice,—the general prevalence of smoothed and striated stones showing that the deposits referred to cannot have been derived from rocks above the level of the *mer de glace.*

(5.) That one result of glacial action was the erosion of rock-basins.

(6.) That intense glacial conditions were interrupted by intervening periods characterized by mild and even genial climates,—the changes of climate being indirectly due to the precession of the equinoxes, which, during a period of extreme eccentricity, would gradually cause the ice-cap to shift from one pole to the other.

(7.) That these interglacial climates are represented in Scotland by stratified deposits intercalated with the till, and containing in places mammalian and vegetable remains; in England by beds in the boulder-clay, and by some portions of the valley-gravels and cave-deposits, with paleolithic implements and bones of the extinct mammalia; on the continent by similar deposits; in America by layers of peat, with buried trees and extinct mammalia.

(8.) That the intermingling of northern and southern forms in the caves and river-gravels points rather to former oscillations of climate than to periods of strongly-contrasted summers and winters,—the arctic mammalia indicating both the gradual approach and the disappearance of glacial conditions; the southern forms being memorials of genial climatal conditions by which the cold or glacial periods were interrupted.

(9.) That the earlier stage of the Glacial epoch embraced several cold and warm periods; but how many the mutilated nature of the records does not at present enable us to say.

(10.) That the climate of the earlier cold periods was more severe than it became in subsequent glacial periods of the same great cycle.

(11.) That similarly the warmth of the interglacial periods was probably greater in the earlier than in the later stages.

(12.) That the disappearance of the last ice-sheet or confluent glaciers in Britain was followed by a period when it is probable that our islands were joined to the continent.

(13.) That this continental condition of Britain may have supervened at a time when the climate was cold and ungenial, and the arctic mammalia may then have revisited our land.

(14.) That the upheaval of land in the north of Europe, which joined Britain to the continent, was accompanied by a corresponding depression in the south.

(15.) That during this movement a large part of Italy was submerged, and deposits of sand filled up the rock-basins, which in all probability had been scooped out in former ice-ages at the mouths of certain Alpine valleys that open upon the plains of Piedmont.

(16.) That the fossils of these sands indicate a temperature for the Mediterranean similar to what obtains at present.

(17.) That during this depression in the south of Europe continental Britain attained to the enjoyment of a mild climate.

(18.) That paleolithic man, and species of mammalia characteristic of the temperate and warm-temperate zone, may have abounded in our country at this time, and left their relics in caves and river-beds.

(19.) That while the mild period continued, subsidence ensued in northern Europe, by which means Britain, from the north of Scotland down to the valley of Thames, was submerged,—the submergence reaching to a depth in Wales and Scotland of about 2000 feet below the present sea-level; and that Scandinavia, Denmark, Holland, northern Germany, northern Russia, and the northern regions of North America, were also carried below the level of the waves. (Formation of the kames, eskers, åsar, etc.)

(20.) That this depression in northern Europe was accompanied by a movement of elevation in the south, as indicated by the old alluvium, with remains of extinct mammalia overlying the marine sands of the plains of Piedmont.

(21.) That if the upheaval in the south at all equalled the degree of subsidence in the north, there must have been land-communication between Europe and Africa,—the soundings between Sicily and Cape Bon indicating the existence of a submarine platform at a less depth than 100 fathoms.

(22.) That the Dürnten lignite beds accumulated while this mild period continued in Italy and the north of Europe.

(23.) That when the submergence was approaching its limits in the northern latitudes of Europe, a change of climate gradually supervened, and icebergs and ice-rafts set sail from the frozen islets that represented Scandinavia and Great Britain and Ireland. (Erratics.)

(24.) That consequently such a change of climate could not have been due to geographical causes.

(25.) That during the submergence in northern Europe, the glaciers of Switzerland advanced and covered up with "newer moraines" some portion of those deposits which had gathered during the preceding mild period.

(26.) That similarly in Italy the Alpine glaciers stole out from their deep valleys, and, entering upon the plains of Piedmont, partially eroded the accumulations which had formed in their absence, but were unable to clear out the buried rock-basins which are inferred to occur at Ivrea and Rivoli (any partial erosion caused by this new advance of the glaciers may since have been obliterated by the deposition of "Alpine diluvium," which must have accumulated after the glaciers retired, just as it did before they advanced).

(27.) That if any deposits indicative of a cold sea were at this time formed in the Mediterranean, they must be still under water, —the land in the south of Europe being at that period most probably of greater extent than it is now.

(28.) That the cold of this period did not approach in severity that of the earlier stages of the Glacial epoch.

(29.) That the movement of depression in the north of Europe was reversed and converted into one of elevation. (Raised beaches; arctic shell clays.)

(30.) That this upheaval again joined Britain to the continent.

(31.) That glaciers during this stage continued to fill the mountain-valleys of Great Britain and Ireland. (Moraines.)

(32.) That in those turbaries and deposits, which can be clearly shown to be of more recent date than this last extension of the glaciers in Britain and the continent, the human relics obtained belong either to the Neolithic period or to still more recent times, and that these relics are never accompanied by the remains of hippopotamus, rhinoceros, or any of the southern forms of mammalia.

(33.) That the climate of Britain during the period of local glaciers was suited to the wants of arctic mammalia, and perhaps the mammoth and Siberian rhinoceros may have survived to this time.

(34.) That as the cold decreased, Britain became densely wooded,—the climate then resembling probably that of Canada before her forests were thinned. (*Megaceros; Bos primigenius; B. longifrons*, etc.)

(35.) That ere long Britain became insulated, and the sea reached to a greater height than its present level.

(36.) That finally the sea retired, and the present order of things obtained.

3. *On the Geology of the Northeastern West India Islands;* by P. T. Cleve. 48 pp. 4to, with 2 plates. (From the Kongl. Svenska Vet. Ak. Handlingar, ix, No. 12. Read Nov., 1870.)—Mr. Cleve, the author of this memoir, made his observations in the West Indies during the winter of 1868–9. He gives much new information with regard to the geology and mineralogy of the northeastern islands, and closes with the following summary of the facts:

From the account given above it is seen that the oldest rocks of the West Indies do not contain fossils, and are, on that account, of an unknown geological age. They occur in Trinidad, and are called the Caribbean series. They extend farther to the west in the northern part of South America. It seems very uncertain whether this series occurs in the other West India islands.

The oldest fossiliferous rocks of the West Indian archipelago belong to the Cretaceous formation.

The Cretaceous formation is observed in Trinidad, Jamaica and in the Virgin Islands, and it is not improbable that the conglomerates, and the metamorphic and igneous rocks of the large islands of Puerto Rico, San Domingo and Cuba may also be found to belong partly to that formation.

In Trinidad the Cretaceous formation seems to be of an older date than the same formation in Jamaica and the Virgin Islands. One Trigonia resembling *T. Boussingaultii* is found in the "older Parian," and this circumstance seems to indicate that the time for the deposit of those beds is about the Neocomian period.

The Cretaceous beds of Jamaica may be classed as a West Indian equivalent to the European Hippurite lime or to the "Turonien" and Gosau deposit.

The fact of the rocks in the West Indian Cretaceous formation being mostly igneous or igneo-sedimentary, evidently proves them to have been heaped up in a time of powerful volcanic activity, and, as the Miocene formation in several places covers the highly disturbed and metamorphosed Cretaceous rocks, in almost horizontal and undisturbed beds, we may conclude that, before the Miocene time, the Cretaceous rocks were raised to a mountain-chain, having a common direction from east to west, and running parallel with the northern coastline of South America.

Fossiliferous beds belonging to the Eocene formation are found in Jamaica, in Trinidad (the San Fernando beds), as also in St. Bartholomew. It may be regarded, too, as very probable that the same formation occurs in St. Martin, Antigua, Guadeloupe (the "pierre à ravets"), Barbados (Scotland), and possibly also in Cuba, San Domingo and Puerto Rico. The Eocene rocks of the West Indies may be classed as equivalents to the lower or middle Eocene formation of Europe (the lower "calcaire grossier" of Paris and "Bracklesham beds" in England.

The Eocene formation contains to a great extent igneous or metamorphic rocks, which fact indicates the activity of the volcanic power also during the Eocene time.

The Miocene formation consists mostly of limestones or marls, and is enormously developed in the West Indies. Limestone strata belonging to the Miocene formation cover large spaces in Cuba, San Domingo, Jamaica, and Puerto Rico. They are also found in Anguilla, Antigua, Barbados and Trinidad. In St. Croix the white marl also seems to belong to the Miocene time. The Miocene formation is continued in the northern part of South America and in Panama.* From the laborious paleontological researches made by Messrs. Moore, Duncan, Sowerby and Guppy, before quoted in several places, it appears that the fossil fauna has a great resemblance to the Miocene fauna of Europe (as in Malta and at Bordeaux, etc.), and also that it shows a close affinity with the still living fauna of the Pacific Ocean and the East Indies. Among the fossil species of the Miocene beds of the West Indies, some are still living in the Caribbean sea.

These facts have led to the supposition that in the Miocene time an open channel existed over Panama to the Pacific Ocean, and also that a connection with Europe existed in form of an archipelago extending from Europe across the ocean to the West Indies. This theory, indicated and developed by Messrs. Duncan, Sowerby, Guppy and Moore, seems very plausible, and it coincides with the hypothesis which Oswald Heer proposed to explain the close affinities between the European Miocene and the still existing subtropical North American flora.

The Miocena fauna of the West Indies does not, however, offer any close affinities with the Miocene fauna of North America.

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* Quart. Journ. Geol. Soc., vol. ix, 1853, p. 132.

The thickness of the Miocene strata of the West Indies, as well as their generally undisturbed position and the absence of volanic rocks, indicates that the Miocene time was, in the West Indies, a long period of calm, undisturbed by volanic phenomena.

A further exploration of the Miocene rocks in the West Indies may make it necessary to divide this formation into several subdivisions.

The Pliocene beds of the West Indies occur in Trinidad (the Matura beds) and in Barbados. To the newest Pliocene or Post-pliocene formation, the lime deposits of Brimstone Hill in St. Kitts, and of Basse-Terre (Guadeloupe) ought, most probably, to be referred. As these deposits occur among rocks ejected from still active volcanos, it may be concluded that the latter take their origin from the Pliocene or the Post-pliocene time. The Bahama Islands, the island of Anegada, and a part of Barbuda belong to a very recent time.

From the facts exposed above it may consequently be inferred, that of the two prevailing lines of elevation in the West Indies the one running from west to east originated before the Miocene time, and that the other from N. W. to S. E., commencing with the Bahamas and continuing in the same direction down to Trinidad, was formed after the Miocene time.

In the mineralogical section of the memoir, the following are described as new minerals:

*Resanite.*—A hydrous silicate of copper and iron, of an olive-green color, uncrystalline and G = 2·06. The analysis afforded

$\ddot{\mathrm{Si}}$ 35·08, $\dot{\mathrm{Cu}}$ 23·18, $\overset{\cdots}{\bar{\mathrm{F}}\mathrm{e}}$ 9·91, $\dot{\mathrm{H}}$ (vol. at 100° C.) 23·15, $\dot{\mathrm{H}}$ (ign.) 8·53=99·85.

It gives the formula, if the iron is protoxide, $\dot{\mathrm{R}}^2\ \ddot{\mathrm{Si}}^3$. It is easily decomposed by hydrochloric acid. Named after Pedro Resano.

*Bartholomite.*—In yellow nodules, composed of small needles. The analysis gave

$\dddot{\mathrm{S}}$ 44·75, $\overset{\cdots}{\bar{\mathrm{F}}\mathrm{e}}$ 22·71, $\dot{\mathrm{Mg}}$ 0·63, $\dot{\mathrm{Na}}$ 17·08, $\dot{\mathrm{H}}$ 8·08, Na Cl 2·88, insol. 3·56=99·69.

Separating the common salt, the magnesia as sulphate and the impurities, it gives the formula

$$2\dot{\mathrm{Na}}\ \dddot{\mathrm{S}} + \overset{\cdots}{\bar{\mathrm{F}}\mathrm{e}}\ \dddot{\mathrm{S}}^2 + 2\dot{\mathrm{H}} = \dddot{\mathrm{S}}\ 50{\cdot}00,\ \overset{\cdots}{\bar{\mathrm{F}}\mathrm{e}}\ 25{\cdot}00,\ \dot{\mathrm{Na}}\ 19{\cdot}38,\ \dot{\mathrm{H}}\ 5{\cdot}62 = 100.$$

It is related to botryogen.

4. *Devonian and Lower Carboniferous Plants.*—In the Proceedings of the Geological Society of London, published May, 1872, is an interesting paper, by Prof. HEER, on the Fossil Plants of Bear Island, Spitzbergen. As catalogued by him, the flora of this place, which occurs in shales associated with coal, and below the Lower Carboniferous limestone, includes a remarkable mixture of the plants of the Lower Carboniferous or Subcarboniferous of Europe and America, with ferns characteristic of the Upper Devonian. Unless there has been a mixture of plants from two distinct horizons on the part of the collectors, there must thus have been in this high northern latitude a transition from the Devonian flora to that of the Carboniferous. In North America such a transition occurs in Ohio, but in other localities these floras are somewhat

distinct; and in the east the two series containing them are unconformable. Prof. Heer, however, somewhat impairs the value of his paper by maintaining that all his plants are truly Carboniferous, and belong to what he names the "Ursa stage" at the base of that formation. He even goes so far as to hold that the plants of the American Chemung and Hamilton groups must be Carboniferous also. This, however, proceeds from want of acquaintance on his part with the rich Middle Devonian flora of Eastern America, the resemblance of which to that of the Coal-measures, in general facies, and especially in its richness in ferns, has misled other European paleobotanists accustomed to regard the Middle Devonian as comparatively barren in plants, whereas in fact it contains a flora comparable in richness with that of the Coal-formation, though distinct as to species.

Mr. Daintree has recently read before the same society a paper on the Geology of Queensland, Australia, in which he refers to a Devonian flora existing there; and Mr. Carruthers, who has examined his specimens, identifies some of them with species found in the Devonian of North America. We have thus the wonderful fact of the extension of a Lower Carboniferous and Devonian flora accompanied with the characteristic marine organisms of these periods, from 76° north latitude to 20° to 30° south—an indication of remarkable uniformity of climate, and perhaps also of the hardy and world-embracing constitution of the members of these old floras.

J. W. D.

5. *Proceedings of the Lyceum of Natural History.*—Vol. i of the Proceedings, containing records commencing with April 4, 1870, has been published as far as page 236, under date of May 8, 1871.—We take from these Proceedings the following:

Prof. Henry Wurtz, in an article (p. 103) on the rock of the Palisades (New Jersey), gives the following analysis of a specimen, having G=2·94, from Prof. Cook (Geol. of New Jersey, p. 215).—

$\ddot{\mathrm{Si}}$ 53·9, $\dddot{\bar{\mathrm{Al}}}$ 17·5, $\dot{\mathrm{Fe}}$ 8·0, $\dot{\mathrm{Mg}}$ 10·3, $\dot{\mathrm{Ca}}$ 8·0, $\dot{\mathrm{K}}$, $\dot{\mathrm{Na}}$ 2·3=100;

and, supposing it to consist of labradorite 60 p. c., and hypersthene 40, he gives for the calculated result—

$\ddot{\mathrm{Si}}$ 53·5, $\dddot{\bar{\mathrm{Al}}}$ 18·2, $\dot{\mathrm{Fe}}$ 8·7, $\dot{\mathrm{Mg}}$ 8·7, $\dot{\mathrm{Ca}}$ 7·4, $\dot{\mathrm{K}}$, $\dot{\mathrm{Na}}$ 2·7=99·2.

[We strongly suspect from the resemblance of the rock to that of the Connecticut trap, and an examination of a specimen of the Palisade rock, that part of the iron was in the state of magnetic iron. The magnetic iron in this rock near New Haven, Ct., is often in octahedral crystals, as may be sometimes seen on weathered surfaces; and the same is true of the Palisade trap examined. The rock appears to be true *dolerite*, the counterpart among eruptive rocks of hyperite among metamorphic rocks. The other constituents are essentially the same that have been obtained in analyses of the New Haven rock.—J. D. D.]

*A mica schist filled with minute crystals of kyanite* covers large areas, according to Professor D. S. Martin, on New York island on East 42d street, near the Union Depot, and also between 45th and 46th streets, west of Madison avenue; and he remarks that the rock is probably continuous from one point to the other.

Prof. Martin also states (p. 222) that *granular limestone* has been found in the gneiss in East 124th street, which is probably a prolongation of the ridge at Mott Haven and elsewhere in Westchester county.

Dr. J. L. Newberry states, on page 224, that the *iron ore* on the Clingwater, in Wyoming, discovered by Dr. Hayden, contains 23½ *p. c. of titanic acid.*

6. *U. S. Geological Survey of the Territories ;* F. V. HAYDEN, U. S. *Geologist in charge. Profiles, Sections and other Illustrations designed to accompany the final report of the Chief Geologist of the Survey, and sketched under his directions ;* by HENRY W. ELLIOTT. Under authority of the Secretary of the Interior, 1872. —This volume of illustrations contains sixty-five plates and over three hundred engravings, all reproductions of the excellent drawings of Mr. Elliott. Although mostly sketchy outlines, with but few touches, they bring out like portraitures the landscape scenery of the regions, showing even the geological features of the surface and stratification of the rocks. Moreover the kinds of rocks in the different parts of each scene have been mentioned by Dr. Hayden in the explanations of the plates. The views have therefore a high scientific interest, both geological and geographical.

7. *Damouritic garnetiferous schist of Salm-Château.*—Messrs. L. D. DE KONINCK and P. DAVREUX have analyzed a schist looking as if steatitic, and have found it to consist of a hydrous mica affording "perfectly" the compositions of damourite, "$\dot{K}$, $3\ddot{\bar{Al}}$, $6\ddot{Si}$, $2\dot{H}$." Some transparent and slightly elastic mica-like plates in the rock have the same constitution as the rock itself. The garnets are manganesian, or of the variety spessartite.—*Proc. Acad. Roy. Belg.*, April 6, *L'Institut*, July 17.

8. *Bathmodon radians of Cope.*—Prof. COPE stated that the largest mammal of the Eocene formation adjoining those of Wyoming, i. e., of the Wahsatch group of Hayden, was the *Bathmodon radians* Cope, of about the size of the rhinoceros. It was an odd-toed ungulate, with peculiar dental characters. The incisors were well developed above and below as in the tapir, but the dental series was little interrupted. The crowns of the upper molars were all wider than long, and presented mixed characters. On the outer margin one only of the two usual crescents of ruminants was present, but a tubercle represented the anterior one. The one which was present was directed very obliquely inward. Inner crescents were represented by two angles, the posterior forming the inner angular margin of a flat table, the anterior a mere cingulum at its anterior base. The arrangement of these parts was stated to be of interest in connection with the relationships between the types of hoofed animals. The single outer crescent was a ruminant indication, while the inner table resembled the interior part of the crown of *Titanotherium*. It differed, however, in its early union with the outer margin, its edge being thus possibly homologous with the posterior transverse crest in

*Rhinoceros.* The premolars had two or three lobes with crescentic section arranged transversely. He regarded the genus as allied to *Chalicotherium.*

He stated that the mammalian fauna of Wyoming and Utah more nearly resembled that of the Paris Basin than any yet discovered in our country, and that it had been discovered to contain a still greater number of generalized mammalian forms. One of the most marked of these was the genus Hipposyus, described by Dr. Leidy.

9. *On some New Species of Fossil Mammalia from Wyoming;* by Dr. Jos. Leidy. (From a letter to Mr. Tryon, dated Fort Bridger, July 24th, printed in advance of the Proceedings of the Academy of Natural Sciences of Philadelphia, and issued Aug. 1, 1872.)—The region of Fort Bridger is an immense basin, the bed of an ancient lake, bounded on the south by the Uinta mountains, and extending far north to the Wind River mountains. The deposits of the lake of the Tertiary period are estimated to be about 8,000 feet in thickness. They present the appearance of a succession of terraces or table-lands extending southerly from Green river to the base of the Uintas. The country for the most part is treeless, and, except along the water-courses, nearly a desert. The Tertiary deposits consist of strata so little inclined that they appear to be horizontal to the eye. The strata are composed mainly of clays, soft and crumbling or more or less indurated, often mixed with sand. Friable sandstones and indurated marls, often with abundance of fresh-water shells, also frequently occur. There are often isolated lands surrounded by broad plains or narrower valleys. These isolated lands are named buttes, and resemble great earthworks or huge railway embankments. Frequently their eroded sides gives them the appearance of a vast assemblage of Egyptian pyramids flanking the plains above. Such assemblages of earthworks, pyramids, mounds, piles of truncated cones, &c., rising from a plain, constitute what are named, in various parts of our great West, "bad lands," or "mauvaises terres."

As the buttes crumble away under the effect of the weather, the fossils of their strata become exposed to view.

On the 17th, in company with Dr. J. Van A. Carter and Dr. Joseph K. Corson, U.S.A., I made a trip to the valley of Dry Creek, forty miles from Fort Bridger. Here we encamped, and spent three days in exploring the neighboring buttes for fossils. The most abundant vertebrate remains are those of turtles, the shells of which are frequently met with in little heaps of fragments into which they have been reduced after exposure from the wearing of the buttes. Of mammalian remains the most abundant are those of the tapir-like animal which I have named *Palæosyops paludosus.* We also found a number of more characteristic specimens than I had before seen of the larger species of *Palæosyops major.* Dr. Corson further discovered the remains of a small species which may be named Palæosyops humilis. An upper molar tooth of this animal measures three-fourths of an inch in diameter. We have likewise found some additional remains of *Hyrachyus agrarius*, and better specimens than I before had of the larger *Hyrachyus eximius*. A well-preserved last lower molar of this species measures an inch fore and aft.

We were fortunate in obtaining the remains of two of the largest and most extraordinary mammals yet discovered in the Bridger Tertiary deposits. One of these was a tapiroid animal exceeding in bulk of body and limb the living rhinoceroses, though the head appears to have been proportionally small. Dr. Carter discovered many fragments of a skeleton of the animal, including a whole humerus, portions of jaws, and a much crushed and distorted cranium. The upper molar teeth have the crown composed of a pair of transverse lobes, with sloping sides and acute summits, separated externally and united internally in a V-like manner. A thick basal ridge bounds the crown in front and behind. A last upper molar measures an inch and a half in the median line fore and aft. The molars in advance are smaller. The lower molars have a trilobed crown. The anterior lobe, larger than the others, extends across the crown and rises in a prominent peak internally. The acute summit is worn away posteriorly. The middle lobe extends about two-thirds across the crown from the other side, and is less prominent than the others. The back lobe, second in size, is thickest internally. The fore and aft diameter of the last lower molar is equal to the corresponding upper tooth. The depth of the lower jaw at the last molar is three and a quarter inches. The humerus is nearly a foot and three-fourths in length and seven and a half inches in breadth at the condyles. I propose to name the great pachyderm of the Uinta country, the UINTATHERIUM ROBUSTUM.

If not the most interesting, the most exciting incident of our exploration of Dry Creek buttes was Dr. Corson's discovery of the upper canine teeth, apparently of the most formidable of carnivores, the enemy of the *Uintatherium*, *Palæosyops*, and other peaceful pachyderms. The teeth resemble in form those of the sabre-toothed tiger. The more perfect specimen consists of nearly nine inches of the enameled crown. In its perfect condition the tooth measured upward of a foot in length, so that it exceeded the canines of the great Brazilian *Machairodus*. The tooth is sabre-like, curved, and compressed conical. Its most remarkable character consists in the lance-head-like form of the terminal three inches. It is thickened at the axis, and impressed and expanded toward the edges, so as to be actually broader in one portion than immediately above. The antero-posterior diameter of the crown near its base is two inches; the thickness over an inch. These canine teeth, terminating in lance-like points, must have proved most terrific instruments of slaughter. Their possessor was no doubt the scourge of Uinta, and may therefore be appropriately named UINTAMASTIX ATROX.

10. *Descriptions of new species of Fossils from the Devonian rocks of Iowa, with a preliminary note on the Formations;* by James Hall and R. P. Whitfield.—Published July, 1872, in advance of the Report on the State Cabinet. 22 pp. 8vo, with 4 plates.—Contains descriptions of species of Corals, Brachiopods and Gasteropods. The figures are excellent.

11. *On the Fossil Man of the cavern of Broussé-roussé, in Italy, called the cave of Mentone;* by E. Riviere.—This skeleton is very nearly complete, it wanting only some bones of the feet, and also the lower extremity of the left tibia, and the posterior extremity of the calcaneum of the same side, broken by the stroke of the pick-axe which brought the skeleton to light.

The measurements show that the skeleton is one of large size. The skull was very dolicocephalous, and its facial angle good, approaching 85°. It closely resembles the man of Cro-Magnon found in 1868.

The different species of animals occurring in the immediate vicinity of the skeleton are *Felis spelæa*, *Ursus spelæus*, *Ursus arctos*, *Canis lupus*, *Erinaceus*, *Rhinoceros*, *Equus* (a molar), *Sus scrofa* (several teeth), *Bos primigenius*, *Cervus alces*, *Cervus elaphus*, *Cervus Canadensis*, a *Cervus* which may be the stag of Corsica, *Cervus capreolus*, *Capra primigenia*(?), *Antilope rupicapra* (or chamois), and a species of *Lepus*. Among these animals, three especially, the cave Felis and Ursus, and the Rhinoceros, indicate by their presence around the skeleton and at levels above it, the epoch to which the fossil man belonged.

Among the other objects present there are two flint knives, a bone pin cut from the radius of a stag, and 22 canines of the stag perforated.

The bones were all in place, the attitude being that of a man who had died in his sleep just where he was found, that is, on a bed made of ashes, charcoal and burnt stones, and amid the refuse of his daily life.—*Les Mondes*, July 11.

12. *Sharks' teeth of the Crag, supposed to have been bored by man.*—The Geological Magazine for June contains an article by T. McK. Hughes, on the bored sharks' teeth found in the Crag (Pliocene), in which the author offers "reasons for believing that there is not the slightest evidence for attributing the phenomena in question to the agency of man." The article is illustrated by figures of the bored teeth, natural size.

13. *The Geology and Physics of the Post-glacial period, as shown in the deposits and organic remains in Lancashire and Cheshire;* by T. Mellard Reade, C.E., F.G.S. (Proc. Liverpool Geol. Soc., Nov., 1871).—The author, after giving detailed descriptions of the region and its Post-glacial formations, presents the following conclusions: That after the laying down of the boulder-clay, the land was elevated above its present level, and again depressed below it, the valleys of the present Lancashire and Cheshire rivers having been mainly excavated during this period and a subsequent re-emergence, and stratified drift deposits made

out of the boulder drift; that a re-emergence took place, and a pause, when the *inferior peat and forest beds* were formed; that a second subsidence took place, denuding these peat beds, and making the Formby and Leasowe marine beds; that an upward movement succeeded, and then grew the forest trees, remains of which are seen at the base of the *superior* peat bed; that the last movement of the land, a downward movement, now took place, and, as a consequence, tidal silt was spread over the superior peat bed, and intercalated in the growing peat, the river bed at Crossens was tilted up, and other changes, both formative and denuding, went forward.

14. *The Birds of North America;* by Prof. S. F. BAIRD, of the Smithsonian Institution.—The publication of a work on the birds of North America, by Prof. Baird, with the coöperation of Dr. T. M. Brewer, of Boston, and Mr. R. Ridgeway, of Illinois, is promised by December 1st, by Messrs. Little, Brown & Co.; and, with such authors, and Dr. Baird at the head, the work will assuredly be one of high merit, and just what is needed. The illustrations will include a drawing of one species of each genus, and also outline figures of the wing, tail, bill and feet; and besides these, one or more figures of the head of each species of North American birds, in most cases of life size. The publishers state that two editions will be issued, one with colored, the other with uncolored, illustrations.

15. *Fossil Cephalopods of the Museum of Comparative Zoology: Embryology;* by ALPHEUS HYATT. Bulletin of the Museum, vol. ii, No. 5.—This memoir contains the results of a careful investigation with respect to the embryology and structure of ammonites and related cephalopods, by a study of the shell in its different stages of development, and also by a comparison of its characters with those of the living nautilus.

16. *Boston Society of Natural History.*—Number 3 of part i. of vol. iii, of the Memoirs of this Society, recently issued, contains an elaborate memoir by Elliott Coues, M.D., on the Osteology and Myology of Didelphys Virginiana, with an appendix on its brain, by Jeffries Wyman.

17. *Efflorescent Salt obtained twelve miles from Denver, Colorado,* contains, according to P. Frazer, Jr., sulphate of soda, 63·87 per cent., sulphate of lime 9·70, water 21·88, chloride of sodium, sulphate of magnesia, &c., 4·55.—*Hayden's Report on Wyoming,* 1871, p. 187.

## III. ASTRONOMY.

1. *Annals of the Observatory of Harvard College,* vol. vi, 1859–60; vol. vii, *Observations of Solar Spots,* 1847–49; by WM. CRANCH BOND, Cambridge, 1871.—The sixth volume of the Annals of the Harvard Observatory contains a *third* series of zone observations made under the direction of Professors W. C. Bond and G. P. Bond. The first series was published in vol. i, part ii, and included the places of 5500 stars; the second, in vol. ii, part ii,

and included the places of 4484 stars. The tables in vol. vi—the third series—contain the places of 6100 stars between 0° 40′ and 1° 0′ north declination.

Volume vii. comprises brief tables and numerous plates, illustrating the positions and characters of solar spots, from the observations of Prof. Wm. C. Bond during the years 1847–49. The plates number 112. Professor Winlock says, in his preface to the volume, that the drawings, here reproduced, "furnish a more perfect record of the appearance of the sun for a period of upward of two years than any with which I am acquainted, excepting of course the photographic records which have become possible in recent times, but could not be attempted when Prof. Bond made his observations." The plates are engraved with great apparent exactness and beauty.

2. *Astronomical Engravings of Moon-Craters, Sun-Spots, etc.; by the Observatory of Harvard College.*—The Observatory of Harvard College has issued the following circular, dated July 11, 1872, soliciting subscriptions.

"The Director of the Observatory of Harvard College purposes to publish a series of astronomical engravings, which shall represent as nearly as possible the most interesting objects in the heavens as they are seen with the powerful instruments of the Observatory under his charge.

The series will consist of at least thirty pictures, and will embrace the principal planets, moon-craters, sun-spots, solar prominences, nebulas, and spectra of variable stars.

To obtain some assistance toward defraying the expense of printing, as well as to secure for them a more general circulation than can be expected for volumes of Annals of an Observatory, they will be offered to subscribers at the rate of ten dollars for the set. The pictures will be delivered from time to time as they are completed, and they will be followed by some pages of notes and explanations."

The few specimen plates sent us are excellent, and of great interest, those of some sun protuberances being remarkably fine.

3. *Aurora Australis in March.*—Faint streamers of the Aurora Australis were seen for a short time on the evening of March 2d, shortly after 9 P. M. On the same day considerable magnetic disturbances took place, which began at 1 P. M., affecting the declination to the greatest extent. At 1.30 P. M. the maximum east declination occurred; it then gradually diminished, increasing and decreasing alternately until, shortly after 6 P. M., a steady decrease took place, the minimum occurring at 6.30 P. M., a decrease of 25 minutes of arc having taken place during the half hour, while the whole range of the disturbance amounted to 38 minutes of arc. The disturbance continued to a smaller extent until, between 2 and 3 A. M. on the 3rd, a small vibratory movement set in, which lasted until 7.30 A. M., after which the needle resumed its normal position. The horizontal force was comparatively little affected, the whole range of the disturbance amounting only to 0·0268 of

the absolute unit, while the vertical force showed very little change. —*Monthly Record of the Melbourne Observatory*, for March, 1872.

4. *The Sun's Light.*—A letter from Signor Tacchini, of Palermo, to M. Faye, states some new facts with regard to the presence of magnesium in the chromosphere. He says that on the 6th of May last he found in the sun some regions of great extent remarkable for the presence of magnesium, stretching over an arch from 12 to 168 degrees; and that on the 18th he presented to the Spectroscopic Society at Palermo a design of the whole border with indications as to the position of the magnesium, and its unaccountable predominance along the western border. He further states that on the 18th of June the magnesium was found to show itself to the spectroscope around the whole border, that is, the whole chromosphere was invested with vapors of this metal. Under this general ebullition, there was naturally an absence of protuberances, while the flames of the chromosphere were very marked and brilliant; and the more brilliant the flames the greater the amount of magnesium indicated.—*L'Institut*, July 10.

5. *The August Meteors* were observed, on the night of 9–10, at Sheffield Hall, by Prof. C. S. Lyman, aided by some young assistants. Previous to 10½ o'clock no regular watch was kept, but 30 or 40 were noticed. After that the numbers were as follows:

From 10½ to 11, 30, by 6 observers.
" 11 " 11½, 50, " " "
" 11½ " 12, 61, " " "
" 12 " 12½, 56, " 5 "
" 12½ " 1, 59, " " " (most of the time).
" 1 " 1½, 42, " 4 "
" 1½ " 2, 32, " 2 "
" 2 " 2½, 28, " 1 "
" 2½ " 3½, some haze; accurate count not kept; but the number was at about the same rate. Several were brilliant, leaving colored trains. At about 8 minutes past 12 one exploded with a bright flash in the east. The paths were not mapped of any, but a large proportion were *conformable* to the usual radiant. Not more than ¾ of the sky was observed most of the time. A more thorough scrutiny would doubtless have made the numbers greater. There was much auroral light in the north all night, with occasional streamers about midnight. No cloudiness in the sky until after 3 o'clock.

Professor R. W. McFarland, of Miami University, Oxford, Ohio, states, in a letter dated Oxford, Aug. 12, that he observed on the morning of the 10th from a quarter before 3 to 5 minutes before 4, and counted in that time 62 meteors.

6. *On two new planets;* by Prof. C. H. F. Peters. (From a letter to one of the Editors, dated Litchfield Observatory of Hamilton College, Clinton, N. Y., August 17.)—I have obtained the following observations on two new planets, which I found on the night of July 31, and which will be the 122d and 123d of the group of asteroids:

(1.) *Asteroid* (122).

| 1872. | Ham. Coll. m. t. | | | App. α. | | | App. δ. | | | No. of comp. |
|---|---|---|---|---|---|---|---|---|---|---|
| | h. | m. | s. | h. | m. | s. | ° | ′ | ″ | |
| July 31, | 15 | 9 | 37 | 21 | 48 | 56·47 | −11 | 41 | 54·9 | 11 |
| Aug. 1, | 11 | 32 | 49 | 21 | 48 | 22·36 | −11 | 45 | 1·8 | 12 |
| " 2, | 12 | 42 | 0 | 21 | 47 | 39·73 | −11 | 48 | 57·4 | 12 |
| " 3, | 12 | 1 | 16 | 21 | 46 | 59·52 | −11 | 52 | 38·2 | 10 |
| " 4, | 12 | 1 | 18 | 21 | 46 | 17·64 | −11 | 56 | 31·0 | 10 |
| " 5, | 11 | 57 | 8 | 21 | 45 | 35·47 | −12 | 0 | 21·1 | 10 |
| " 7, | 12 | 7 | 10 | 21 | 44 | 9·32 | −12 | 8 | 13·1 | 8 |
| " 8, | 11 | 9 | 40 | 21 | 43 | 28·11 | −12 | 12 | 4·4 | 10 |
| " 9, | 11 | 28 | 53 | 21 | 42 | 44·02 | −12 | 16 | 5·7 | 10 |

The magnitude of the planet is between 11th and 12th.

(2.) *Asteroid* (123).

| 1872. | Ham. Coll. m. t. | | | App. α. | | | App. δ. | | | No. of comp. |
|---|---|---|---|---|---|---|---|---|---|---|
| | h. | m. | s. | h. | m. | s. | ° | ′ | ″ | |
| Aug. 1, | 12 | 8 | 29 | 21 | 57 | 30·53 | −10 | 4 | 55·3 | 12 |
| " 2, | 13 | 22 | 13 | 21 | 56 | 39·73 | −10 | 6 | 42·3 | 8 |
| " 4, | 11 | 31 | 8 | 21 | 55 | 4·91 | −10 | 10 | 15·5 | 5 |
| " 5, | 12 | 53 | 38 | 21 | 54 | 11·34 | −10 | 12 | 15·0 | 6 |
| " 7, | 13 | 8 | 15 | 21 | 52 | 27·13 | −10 | 16 | 16·9 | 10 |
| " 8, | 12 | 2 | 38 | 21 | 51 | 37·20 | −10 | 18 | 17·3 | 10 |
| " 9, | 12 | 29 | 53 | 21 | 50 | 43·10 | −10 | 20 | 26·7 | 6 |

The magnitude of this planet is estimated about 12th. On July 31 I succeeded only in obtaining a rough estimate of position, the advanced twilight not permitting accurate measurements. There will be no difficulty in finding the planets again after the moonlight, even without ephemerides.

7. *Spectra of star-shine, night-light, the Zodiacal light;* by C. Piazzi-Smyth.—* * * And what sort of spectrum ought such star-shine and night-light to offer to the observer? Not a sharp, aurora-like line at all, but something, according to the manner and locality of its formation, very much like the spectrum of the last twilight of day. In fact, according to numerous observations on the Sicilian night sky, when free from any accusation of aurora, I found such night-shine to yield a short continuous spectrum; and that culminating, not at the aurora line place of W. L. 5579, but near W. L. 5350.

Now this place evidently corresponds within the limits of error of observation to that of the last residual portion of the continous spectrum of stronger daylight or sunshine (with the eclipse corona also, at W. L. 5322), modified slightly by the absorbing and reflecting powers of the atmosphere. No Fraunhofer lines are seen in such star-shine spectrum for these reasons; 1st, the continuous spectrum is too faint to show any black lines upon it clearly; and 2d, the slit used at the time of observation was too broad to have shown Fraunhofer lines even in noonday shine. Indeed, to see the spectrum of general night star-shine, the slit of the spectroscope had to be opened wider still than in the case of the zodiacal light; but gave then a closely similar spectrum; and thence we are led to the conclusion that the spectrum of the zodiacal light is the same in kind as that of either star-shine or faint sunshine. Whence the further deduction is inevitable, that the older astronomical theory of the zodiacal light being the solar illumination of infinitely small distant particles such as meteors revolving about the sun, whether in orbits of infra or ultra planetary ellipticity, is spectroscopically maintained; while, that such solar zodiacal light has any physical connection with the essentially terrestrial accompaniment of aurora, is just as eminently negatived by the spectroscope: for "no two spectra," as the lady most truly said in the Royal Observatory at Palermo, "can be more essentially different than those of the aurora and the zodiacal light. They are as different from one another as night from day.—*Monthly Notices Astron. Soc., June, p.* 285.

## V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *List of Elevations and Distances in that portion of the United States west of the Mississippi River;* collated and arranged by Prof. C. THOMAS, Assistant U. S. Geological Survey under Dr. F. V. HAYDEN. 32 pp. 12mo. Washington, 1872.—We cite here Table XI, of elevations in Colorado, from this valuable catalogue of elevations:—

| Names of Points. | Altitude above the sea. |
|---|---|
| | *Feet.* |
| Mount Harvard (Whitney), --- | 14,270 |
| Gray's Peak " --- | 14,145 |
| Mount Lincoln " --- | 14,123 |
| Mount Yale " --- | 14,078 |
| Pike's Peak (Parry), --------- | 14,216 |
| Long's Peak, --------------- | 14,056 |
| Barry's Peak, --------------- | 13,133 |
| Mount Flora, --------------- | 12,878 |
| Mount Wright (E. Berthoud's Pass), ------------------ | 11,800 |
| Cherry Creek Divide, --------- | 7,575 |
| Denver, ------------------- | 5,317 |
| Golden City, --------------- | 5,882 |
| Mount Vernon, ------------ | 6,479 |
| Golden Gate, --------------- | 6,226 |
| Junction N. and S. Clear Creeks, ---- ---------- ----- | 6,466 |
| Black Hawk, -------------- | 7,543 |
| Central City, --------------- | 8,043 |
| Missouri City, -------------- | 9,073 |
| Head Virginia Cañon, -------- | 9,690 |
| Idaho, -------------------- | 7,149 |
| Georgetown, --------------- | 8,245 |
| Berthoud Pass, ------------- | 10,896 |
| Boulder Pass, -------------- | 11,670 |
| Jones's Pass, --------------- | 12,400 |
| Argentine Pass, ------------- | 13,100 |
| Georgia Gulch Pass, --------- | 11,487 |
| Ute Pass, ----------------- | 11,200 |
| Vasquez Pass (estimated), ---- | 11,500 |
| Hot Springs (Idaho City), ----- | 7,050 |
| Hot Springs (Middle Park), --- | 7,725 |
| Soda Springs (near Pike's Peak), | 6,515 |
| Gold Hill, ----------------- | 8,636 |
| Bergen's Ranch (Jefferson Co.), | 7,752 |
| Jefferson, South Park, ------- | 9,842 |
| Tarryall, ------------------ | 9,943 |
| Foot Berthoud's Pass, -------- | 9,325 |
| Osborn's Lake, -------- ----- | 8,821 |
| Velie's Peak, -------------- | 13,456 |
| Mount Audubon, ------------ | 13,402 |
| *Timber Line* (Parry). | |
| On Pike's Peak, ------------ | 12,000 |
| On Snowy Range, ----------- | 11,800 |
| On Mount Audubon, --------- | 11,325 |
| On Long's Peak, ------------ | 10,800 |
| On Wind River Mountains, --- | 10,160 |
| On Gilbert's Peak (Uinta Mountains. Hayden's survey), --- | 11,100 |

The pamphlet closes with the following "Remarks:"—

"An examination of the foregoing tables, together with those found in the reports of the surveys of Professor F. V. Hayden for 1870 and 1871, reveals some very important facts in regard to the topography of the west; among which we may mention the following as of general interest.

That the Llano Estacado, or "Staked Plains," at their northern extremity, immediately south of the Canadian river, vary in elevation from 3,200 to 4,000 feet above the sea; while at the southern extremity, where Captain Pope's line crossed them, they reach an elevation of 4,700. At the north end they are somewhat irregular and broken, the highest point being near the west margin; at the south end they are much more regular, the highest point being near the eastern flank. The average elevation of this extensive table land is about 4,000 feet above the sea.

The average ascending grade along Captain Pope's line, from Preston to the margin of the Llano Estacado, is about ten feet to the mile.

The average ascent along Lieutenant Whipple's line, from Fort Smith to White Sandy creek on the margin of the Llano Estacado, is about six feet to the mile. From White Sandy creek to Laguna Blanco, the highest point on this line east of the Rio Grande, the ascent is at the rate of ten feet to the mile.

Along the line of the Kansas Pacific Railroad, from Kansas City to Denver Junction, the ascending grade averages a little over eight feet to the mile. Along the Union Pacific Railroad, from Papillion (a station a short distance west of Omaha) to Cheyenne, the ascent averages just ten feet to the mile; but if we stop at Sidney it is a little less than eight feet; while from Sidney to Cheyenne it is over nineteen feet to the mile.

The land ascent from Fort Union, at the mouth of the Yellowstone, to Fort Benton, is only about two and a half feet per mile; but from there west to the base of the mountain it increases rapidly until it averages about twenty feet to the mile.

These facts show that from the southern boundary of Colorado to the northern boundary of Nebraska the slope of the great plains is tolerably uniform, while north (measuring east and west) it decreases more rapidly.

This is, doubtless, owing to two facts; *first*, the heaviest mountain mass, from which the supply of *débris* comes, lies directly west of the first section; that is, between the southern boundary of Colorado and northern boundary of Nebraska; *second*, the northern line is not measured directly at right angles with the range. The line along the Yellowstone would give the ascent more correctly.

It would be an interesting problem in mathematical geology (if we may use such a term) to ascertain the immense amount of débris covering these vast plains which has been washed down from the mountains, and therefrom to form some estimate of their original height.

The average elevation of the great mountain plateau which lies between the Black Hills and Wahsatch range is about 6,500 feet above the level of the sea, and is about 1,200 feet above the western borders of the plains, and 2,200 feet above the Salt Lake basin.

The average fall of the principal rivers is as follows:

Of the Rio Grande, from Isleta to El Paso, about five feet to the mile; but from Isleta south for some distance it is about six feet.

That of the Pecos is not more than four feet to the mile.

The Canadian, from the mouth of the Pajarito creek for two hundred miles eastward, is nine feet to the mile.

The Arkansas, from the mouth of the Apishpa river to Fort Atkinson, eight or nine feet.

The South Platte, from Denver to its junction with the North Platte, is nine or ten feet to the mile; while that of the North Platte, from Fort Fetterman to the junction, is only seven feet. The Platte below the junction has an average descent of about six feet to the mile.

The fall of the Missouri river, from Fort Benton to the mouth of the Yellowstone, is about two feet to the mile; and from the latter point to its mouth, near St. Louis, it averages a fraction less than one foot.

Snake river, from the mouth of Ross's Fork, north, about six feet to the mile.

These facts in regard to the average descent of these rivers furnish important data for estimating the probability of utilizing the water from these streams in irrigating the bordering plains."

2. *Effect of change of barometric pressure on human beings.*—Mr. Paul Best has shown that in the destruction of life by diminishing the barometric pressure, the direct cause is the deficiency of oxygen, and that an animal that will die with the pressure reduced to 18 cm. (7 inches) will endure a reduction to 6 cm. (2·4 inches) before life is extinct, if oxygen be added. And the converse is also true that the evil of too great pressure comes mainly from the too large amount in that case of oxygen, dilution with nitrogen prolonging life. He remarks that workmen employed at great elevations would accordingly find benefit in an arrangement for supplying more oxygen; and those occupied in diving for pearls, etc., by a contrivance for supplying nitrogen.—*Les Mondes*, July 11.

3. *The Temperature and Rainfall of July*, 1872; by C. Keutgen, Jr., Meteorologist to the Board of Health, County of Richmond (Staten Island), N. Y. (Communicated for this Journal.)—The month of July, 1872, exhibited the highest temperature and greatest rainfall ever recorded on this island, for any one month. The temperature mean was 78·1, the maximum 96, and the minimum 64·2 degrees. The total rainfall amounted to 11·34 inches. On July 26th, 4·29 inches of rain descended within 12 hours.

4. *A Manual of Qualitative Analysis;* by Robert Galloway, F.C.S., etc., from the fifth rewritten and enlarged London edition, with illustrations. Philadelphia: Henry C. Lea, 1872. 12mo, pp. 402.—Prof. Galloway's books are deservedly in high esteem, and this American reprint of the fifth edition (1869) of his Manual of Qualitative Analysis, will be acceptable to many American students to whom the English imprint is not accessible. The publisher does not say that the present issue has received the revision or approval of the author, and the absence of this information implies a doubt which a word of affirmative statement would remove.

THE

# AMERICAN

# JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

## ART. XXX.—*On the nature and duration of the discharge of a Leyden Jar connected with an induction coil;* by OGDEN N. ROOD, Prof. of Physics in Columbia College. Part III.

IN the first and second parts of this paper,[*] I described with some minuteness the nature and duration of the discharge of a moderate sized and of a small jar, when connected with an induction coil; one important point has, however, thus far remained untouched, and to its examination the present paper is devoted.[†]

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[*] This Journal, Sept., 1869, and Sept., 1871.

[†] Prof. Joseph Le Conte, in a beautiful and elaborate paper on the subject of binocular vision (this Journal, July, 1871), calls attention to the fact that most writers on Physics, myself among the number, make the statement that Wheatstone, in his celebrated memoir on the duration of the electric light, &c. (Phil. Trans., 1835, part ii, p. 583), arrived at the result that it lasted less than one-millionth of a second. Mr. Le Conte, on the authority of De la Rive and Daguin, regards this as an error, holding that the interval of time assigned to the discharge of a Leyden jar by Wheatstone was $\frac{1}{24000}$ of a second. At the time I wrote my first paper on this subject, I consulted Wheatstone's memoir, or rather the complete translation of it in Pogg. Ann., bd. xxxiv, s. 464, and as I thought did justice to the claims of this eminent physicist. The facts at all events are as follows: In this memoir Wheatstone, after describing an arrangement by which he supposed he could measure an interval of time as small as $\frac{1}{72000}$ of a second, says (translating from the German): "I presented successively to the mirror electric sparks four inches in length, which were drawn from the prime conductor of an electrical machine, discharges of a Leyden jar, a glass tube four feet long, in which the electric spark was obliged to spring over a series of pieces of tin foil placed on it in a spiral manner, an evacuated tube six feet long, in which the spark during its passage generated an unbroken line of weakened electric light, and finally different figures as birds, stars, &c., formed from electric sparks. But in all these cases the reflected pictures, when they fell in the field of view, appeared entirely unaltered, and exactly the same as though they had been reflected from a motionless mirror." This he regarded as proof that the duration of the discharge of a Leyden jar, &c.,

When the primary coil of an inductorium is connected with a voltaic battery, the act of interrupting the connection, as is well known, produces a current of electricity in the secondary coil, which can be accumulated in a Leyden jar, and then discharged by a spark. Now it is possible, as I shall show, to adjust either the electrical surface of the jar, or its striking distance, so that with a given coil only a single spark will be produced each time that the battery circuit is broken, but in the great majority of cases it will happen that enough electricity will be generated to charge and discharge the jar a number of times. The circumstance that electricity is continuously furnished by the coil during a fraction of a second, is favorable to the production of these *multiple discharges*, and it is to their study that I would now draw attention.

### *Apparatus.*

Much of the apparatus employed was that already described, but it may not be amiss to add here some details respecting it, which seem demanded by the nature of the new experiments.

*The coil.*—This as stated was a large one by Ritchie, arranged vertically, and made about twelve or fifteen years ago. Mr. Ritchie kindly informs me that the primary coil consists of about 48·8 meters of copper wire, having a diameter of 4·23 millimeters, and making 264 turns. The diameter of the wire in the secondary coil is ·21 of a millimeter; its length thirty English miles. I mentioned in the first part of this paper that the simple sparks furnished by it did not, at the time of my experiments, exceed seven inches,—meaning, when the coil was excited by a moderate battery, i. e., six Bunsen cups.

*The battery, &c.*—In all the experiments described in the three parts of this paper, the battery consisted of only two Bunsen cups, arranged for intensity, and the length of the simple induction spark furnished by the coil under these circumstances, blunt brass points being used as electrodes, ranged from 48 up

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was less than $\frac{1}{72000}$ of a second. These experiments, he states, were afterward repeated with a mirror revolving 800 times in a second, with exactly the same result. "They" (the images) "were reflected upward, as distinct and unaltered as the objects themselves, by direct vision." And finally, in summing up his results at the close of the article, Wheatstone says: "The light of electricity of high tension has a duration less than one-millionth of a second."

At the time I wrote my paper I was perfectly well aware that Wheatstone mentioned in the same paper that he had obtained for the discharge of a Leyden jar, taking place through *half an English mile of copper wire*, a duration of $\frac{1}{24000}$ of a second. This I take to be an entirely different matter intrinsically, and different also from my own experiments, where the circuit was as short as possible, and it is easy to see that a corresponding opinion was entertained by Wheatstone himself, for he remarks of his experiment where he obtained a duration of $\frac{1}{24000}$ of a second: "The lengthening of the spark at the above mentioned interruption of the wire was without doubt due to the circumstance that the wire was not thick enough to allow the jar to discharge itself otherwise than in a successive way."

to 66 millimeters. In the experiments about to be detailed, the strength of the battery current was from time to time tested with a tangent-compass, and its constancy within narrow limits maintained. In addition, however, I from time to time measured the length of the simple induction-spark furnished by the coil alone, blunt brass points being used as electrodes, a proceeding which served to show whether the automatic apparatus for breaking the circuit was working with uniformity. The length of the simple induction-spark is quite a variable quantity, and I contented myself with an approach to its maximum length, the electrodes being placed so far apart that one-quarter or one-third of the breaks generated visible sparks. Their average length in these experiments was 54·3$^{mm}$. Some variation will be found in this matter, but it is never to be attributed to a corresponding change in the current of the battery, but rather to the apparatus for breaking the circuit, and to the judgment of the experimenter.

*Leyden jars.*—The same Leyden jars formerly mentioned were again used; the electrical surface of the larger one was 114·4 square inches (738·06 square centimeters nearly), while that of the smaller one was only eleven square inches (70·96 square centimeters nearly). They were always, when in use, placed on insulating stands.

*Electrodes.*—In the experiments described in all three parts of this paper, the same set of electrodes was used; the brass balls had a diameter of 9 millimeters; the platinum points consisted of wire with a diameter of ·3 of a millimeter; they were allowed to waste away under the influence of the explosions, never being sharpened by cutting or otherwise.

*Automatic arrangement for breaking the circuit.*—This, was arranged with the use of alcohol and mercury, and was so contrived that by weighting it the interruptions could be made as slowly as two per second, which was quite essential in some of the experiments. I also made some efforts to find whether the nature of the multiple discharges changed as the interruptor operated more rapidly, but was not able to ascertain that doubling or trebling its velocity made any perceptible difference. No attempt was made to regulate the instant at which the discharge took place, the experimenter depending entirely on chance, which, if it demanded a little more patience, on the other hand withdrew all interference from the act of discharge, and allowed of its study, if I may use the expression, under more natural conditions.

*Revolving apparatus.*—The same train of toothed wheels was used, a double silvered mirror being now attached to the second axis, reckoning from that with the weight. Its rate of revolution could be varied from half a revolution up to three per second. This rate was accurately ascertained by causing the lowest wheel to wind up a fillet of paper, on which second marks were at the time impressed.

*Optical apparatus.*—This consisted of an achromatic lens, the mirror just mentioned and a plate of ground glass. In the experiments detailed in the present paper, the distance of the mirror from the ground-glass was only 165 millimeters; the lens was the objective of an opera-glass, as with the lens previously used by me certain violet streaks hereafter to be described were quite invisible.

*Micrometers.*—The general micrometric arrangement described in part second of this paper, under the head "*Micrometer*," was sometimes employed, the strip of white paper being replaced by a small cylindrical mirror, situated under the spark. A tube of glass filled with mercury was used for this purpose. On the other hand, sometimes an achromatized calc spar prism was employed, being located between the eye of the observer and the image on the ground-glass; but for actual work, another and much simpler proceeding was found to furnish results equally reliable, with an expenditure of less time and patience. This method, which at first sight may seem rude and unreliable, consisted merely in holding on the ground-glass along the path of the elongated images a pair of compasses, and repeatedly adjusting them, till they enclosed a length supposed to be equal to that of the image, actual comparison and estimation both alike being used. This length was then transferred to paper, and the process repeated, until a number of observations had been accumulated, their average being finally taken. With electrical phenomena of an unvarying nature, the other methods would probably have yielded more accurate results; but in the present case, where the actual variations often rose as high as 50 per cent of the quantity involved, this mode, as the following experiment shows, is all that could be desired.

I pasted a piece of white paper on a circular black disc as shown in fig. 1, and attached the disc to an ordinary Dove's rotation apparatus. A diminished image of the disc and strip of paper was formed on a plate of ground-glass by a camera obscura, the ground-glass being arranged so as to be easily accessible to the hands of the experimenter. The disc was now set in rapid rotation and illuminated with simple (not multiple) electric sparks, and attempts were made to measure the length of the image of the strip of paper with a pair of compasses, in the manner just indicated. The room was only sufficiently light to enable me to perform this work, and the meas-

1.

urements were from time to time transferred machanically to paper, without inspection. After the end of the experiment, the actual size of the image on the ground-glass was measured, furnishing thus the test of the degree of accuracy attained.

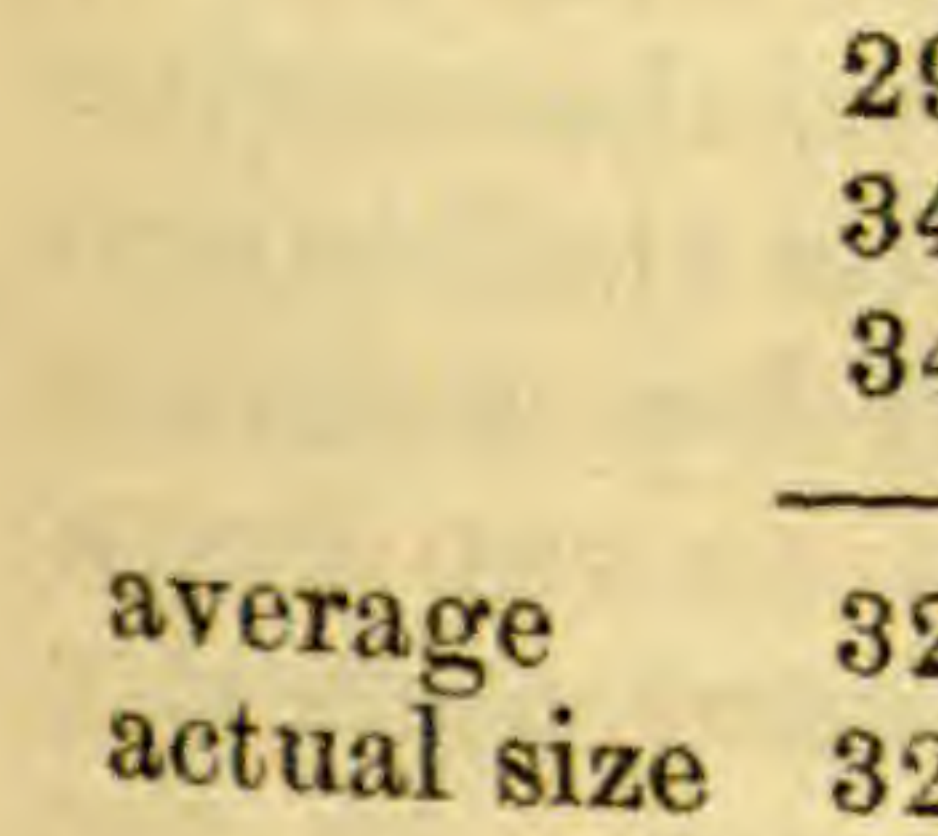
*Observations.*

| | | | |
|---|---|---|---|
| | 33·5 millimeters. | | 35· millimeters. |
| | 34·3 | | 42· |
| | 31·2 | | 43·3 |
| | 34·6 | | 42·3 |
| | 33·6 | | 44·4 |
| | 32·8 | | 45· |
| | 34·1 | | 44·5 |
| | 33·5 | | 41.5 |
| | 28· | | 42· |
| | 34· | | 43·9 |
| | 32· | | |
| | 30· | average | 42·39 |
| | 34·4 | actual size | 43·50 |
| | 32· | | |
| | 34·4 | | |
| | 31·5 | | |
| | 29· | | |
| | 34· | | |
| | 34·1 | | |
| average | 32·6 | | |
| actual size | 32· | | |

In the first experiment with nineteen observations the error is 1·8 per cent, while in the second, with only ten trials, it is about 2·5 per cent. I may remark here that my friend President Morton, who repeated for me some of these experiments, obtained without difficulty similar results.

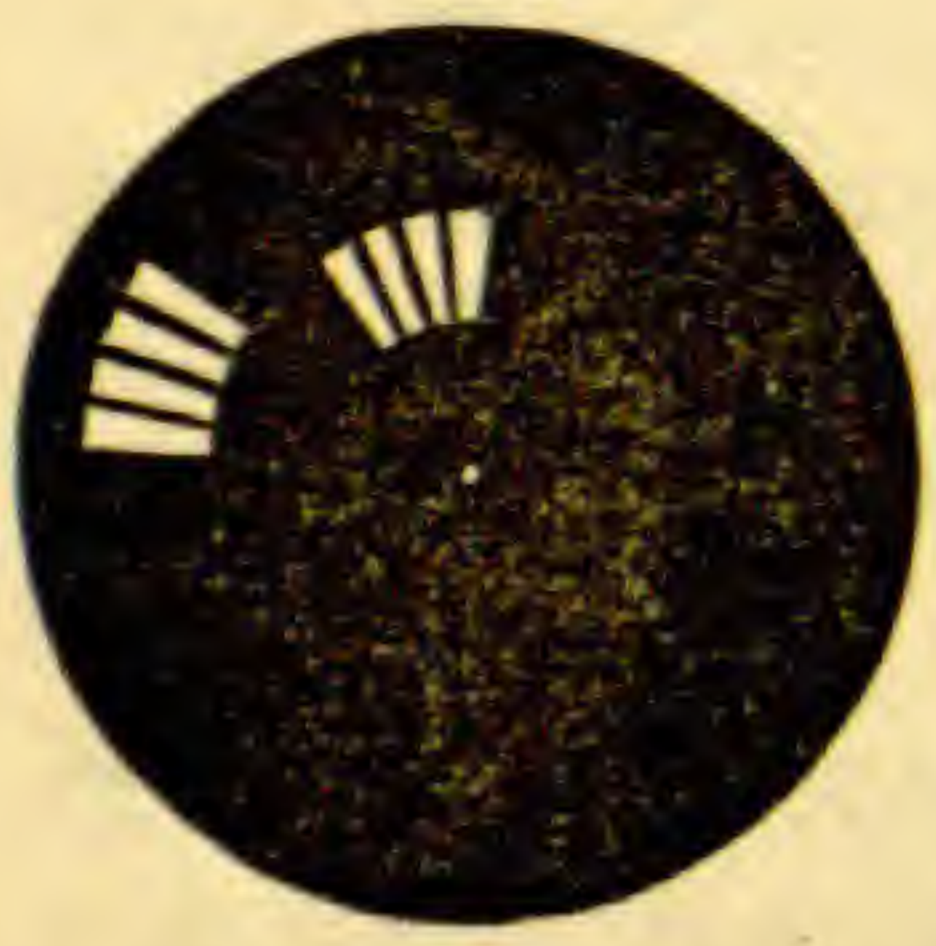
*Micrometer disc.*—It was considered desirable, nevertheless, to verify if possible the results obtained with the revolving mirror, by the aid of a different and quite independent mode of proceeding, and for this purpose a kind of micrometer disc was contrived. Besides this office, it also served to unravel certain matters regarding the *form* of the discharge, which otherwise would have remained obscure, and it always furnished by far the most reliable information, relative to the distribution and number of discharges, included in a multiple spark. Let us suppose that we have a black card-board disc, with two open sectors of 2½° each, disposed as in fig. 2. If now this disc be set in rotation in front of a sheet of white paper, the latter being illuminated by a multiple electric spark, the disc will present

2.



to the eye of the observer placed ten or twenty inches behind it, an appearance like that in fig. 3. It is evident that by increasing the rate of rotation of the disc, or by diminishing the angle between its open sectors the termination of the outer series of illuminated sectors, may be made to coincide with the beginning of the interior series, when the included angle will give a measure of the total duration of the phenomenon. In practice the interior sector was cut in a second and smaller disc, which was placed on the same axis with the first, a portion of this latter being removed so as to allow of any angular adjustment within 90°. The larger disc was graduated on its circumference. In some of the experiments this arrangement of movable sectors was repeated for each quadrant, but as a general thing it was found safer to confine the operation to a single quadrant. Besides this, the exterior portion of the outer sector was provided with a prolongation, consisting of small slit, which was very useful in furnishing information regarding the *number* of the discharges in a given case, as with sectors 2½° in width the images frequently overlapped. Most of the disc observations were made with a train of toothed wheels run by a weight, the motion proving sufficiently uniform. For the highest and lowest rates, a train of toothed wheels moved by a spring was employed, the lowest wheel being allowed to run down only half a turn in any experiment, which insured a greater degree of uniformity than was absolutely necessary. In the disc experiments the highest rate used was 17·4 revolutions per second, the lowest only 2·4. In this last case the disc was attached, not to the terminal axis, but to its immediate neighbor.

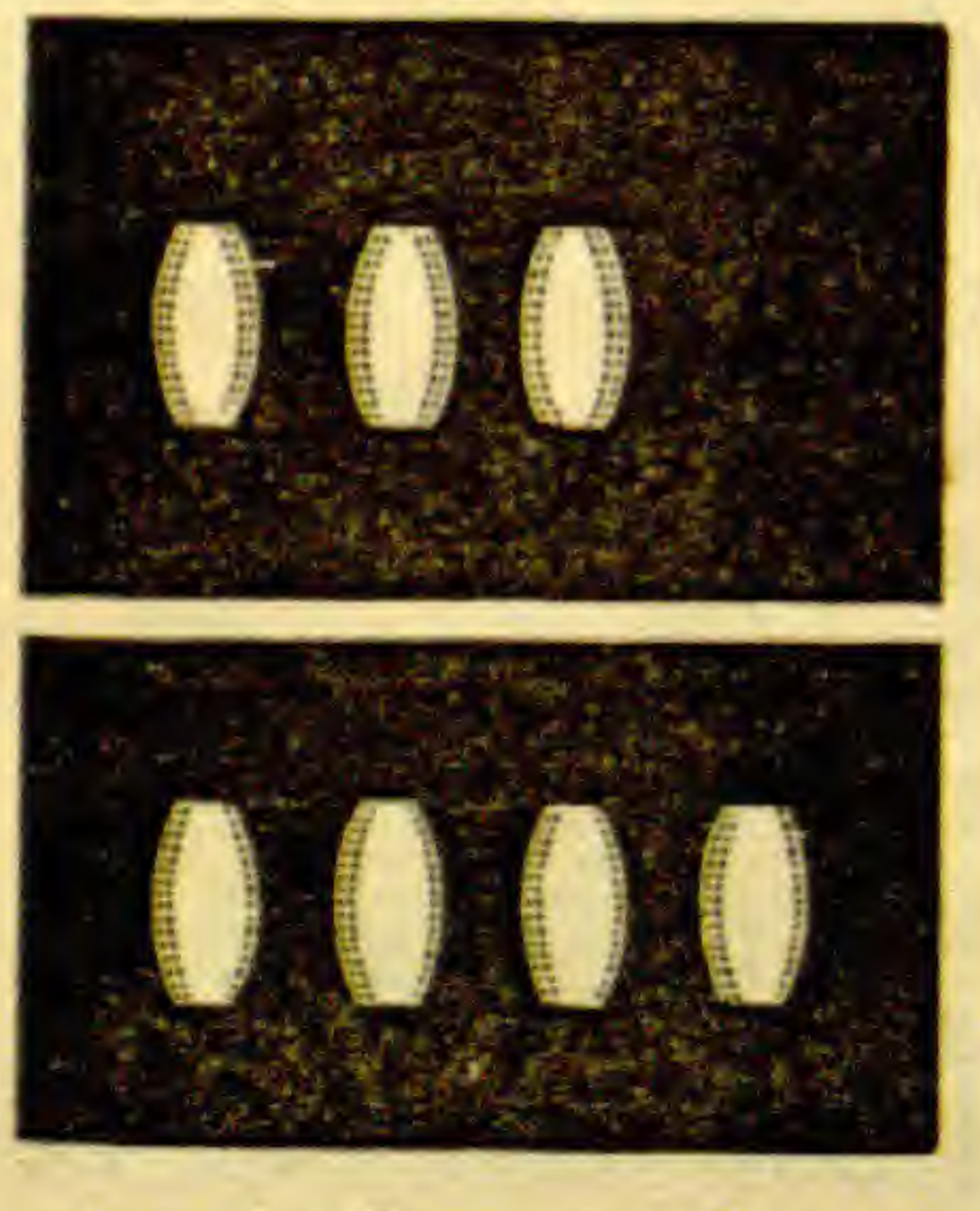
3.



### *Form and duration of the discharge of the larger jar, with brass balls as electrodes.*

In this set of experiments the length of the simple induction spark, blunt brass points being used as electrodes, was 48 millimeters.

4.



*Striking distance* 1 *millimeter.*—The form of the discharge was quite simple; it consisted merely of three or four instantaneous* sparks following each other rapidly, the interval of time between the last spark and its neighbor being somewhat greater than the others. Fig. 4 roughly represents the appearance with the revolving mirror. The three and four-fold sparks occurred intermingled, so that while it was not difficult to measure the aver-

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* The actual duration of sparks called in this paper instantaneous has already been given.

age duration of the mixed phenomenon, the determination of the duration of the two constituents was more troublesome.

Total duration with 3 discharges, .......... ·0042 sec.
“ “ “ 4 “ .......... ·0063 “

This gives an average interval between the individual discharges of ·0021 sec.

*Striking distance* 2 *millimeters.*—Only a single discharge was generated, and the same was true up to the maximum striking distance, which in this experiment was 2·6 millimeters.

### *Form and duration of the discharges of the larger jar with platinum points as electrodes.*

The length of the simple induction sparks was 48· millimeters.

*Striking distance* 1 *millimeter.*—The form was the same as shown in fig. 4; the number, however, varied commonly from 2 to 4, rarely reaching 5.

Total duration with 2 discharges, .......... ·0020 sec.
“ “ “ 4 “ .......... ·0064 “
The average interval was ................ ·0020 “

*Striking distance* 2 *millimeters.*—The form was still the same, with two, three and rarely four discharges.

Total duration with 2 discharges, .......... ·0027 sec.
“ “ “ 3 “ .......... ·0061 “
The average interval was ................ ·0028 “

*Striking distance* 3 *millimeters.*—Two and seldom three discharges.

Total duration with 2 discharges, .......... ·0040 sec.

*Striking distance* 4 *millimeters.*—Generally only one discharge; rarely two. In *one* disc experiment the interval between the two discharges was doubtfully determined as ·0036 sec.

*Striking distance* 5 *millimeters.*—Only a single discharge—about one spark in 20 was double.

*Striking distance* 6 *millimeters.*—Using the revolving disc, I looked for a long time and was not able to detect any double discharges, so that from 6 millimeters up to 11·5 millimeters, which was the maximum striking distance, the discharges proved to be simple, instantaneous sparks.

It would then appear probable from these experiments that an increase of the striking distance is accompanied by a corresponding increase in the interval between the sparks composing the multiple discharges, though upon the whole it shortens the total duration of the act, by diminishing the actual number of discharges.

## ART. XXXI.—*Notice of some New Tertiary and Post-Tertiary Birds;* by O. C. MARSH.

THE discoveries of the last few years have shown conclusively that remains of Birds, so long unrepresented among the fossils of this country, are occasionally found well preserved in some of our deposits of Cretaceous age.* Although still rare, they are more numerous in the Tertiary formation; and our Post-pliocene beds, doubtless rich in such remains, are beginning to yield many interesting forms of this class. The present paper contains preliminary descriptions of several species of birds, which were found by the Yale party during their explorations of last year in the lower Tertiary strata of Wyoming. To these are added a few species of interest from the Post-pliocene of the Atlantic coast.

### *Aletornis nobilis*, gen. et sp. nov.

A large wading bird, nearly equal to a Flamingo in size, is indicated in our collections by the distal end of a tarso-metatarsal bone, and by a few other fragmentary remains. The former specimen resembles in its main features the corresponding bone in the Cranes, but the inner or second metatarsal element is more produced distally, and its articular face has the posterior tubercle less developed. The tibia, also, has the trochlear groove on the posterior face of its distal extremity much shallower. The distal articular ends of the second and fourth metatarsals are nearly equal in size, and the foramen between the third and fourth is large.

*Measurements.*

| | |
|---|---|
| Transverse diameter of distal end of tarso-metatarsal, - - - - - | 16· mm. |
| Antero-posterior diameter of distal end of second metatarsal, | 9·3 |
| Transverse diameter, - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - | 4·8 |
| Antero-posterior diameter of distal end of fourth metatarsal, | 11· |
| Transverse diameter, - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - | 5· |

The known remains of this species were found in September last, at Grizzly Buttes, Wyoming, by Mr. O. Harger of the Yale party. The geological horizon was lower Miocene, or Eocene.

### *Aletornis pernix*, sp. nov.

A smaller species, which may for the present be referred to the same genus, is represented also by some fragmentary specimens, found together and evidently belonging to one skeleton. The most important of these remains is the distal end of a

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* This Journal, vol. xlix, p. 205, March, 1870, and vol. iii, p. 360, May, 1872.

tibia, which differs from that of the preceding species, aside from its smaller size, in having a deeper groove on the posterior trochlear surface. This tibia is, moreover, peculiar in its short antero-posterior diameter between the condyles, which are narrow in front, and divergent. The opening below the supra-tendinal bridge is transversely oval in outline, and looks forward, and downward. The outer condyle has its margin semi-circular below, and its external face is excavated, especially near the anterior border. The species was about as large as a Scarlet ibis.

*Measurements.*

| | |
|---|---|
| Antero-posterior diameter of outer condyle at distal end of tibia, | 9· mm. |
| Transverse diameter in front, | 3· |
| Vertical diameter in front, | 8· |
| Width of trochlear groove on posterior face of tibia, | 5· |
| Width of opening below supra-tendinal bridge, | 2· |

The specimens on which this species is based were found by the writer, last autumn, near Henry's Fork, Wyoming. The geological position of this locality is essentially the same as that at Grizzly Buttes.

### *Aletornis venustus*, sp. nov.

Another species of wading birds, apparently belonging to the genus *Aletornis*, is indicated by the distal part of a tibia in perfect preservation. This fossil has the condyles less expanded in front than in the specimen last described, and the trochlear groove on the posterior face is not so deeply excavated. The supra-tendinal bridge is transverse, of medium width, and almost entirely inside the central line. The opening below the bridge is a broad transverse oval, and looks forward, downward, and slightly inward. The tendinal canal is broad, and deepest on the inner side: its floor is nearly flat. The outer condyle has its margin nearly semi-circular below, and its external face regularly concave. The inner condyle has a very short antero-posterior diameter. The trochlear groove is deepest near its external margin, where there is a faint narrow channel. This specimen indicates a bird about the size of a Curlew.

*Measurements.*

| | |
|---|---|
| Transverse diameter of distal end of tibia in front, | 7·3 mm. |
| Transverse diameter below, | 6·2 |
| Width of trochlear groove on posterior face, | 5· |
| Antero-posterior diameter of outer condyle, | 7· |
| Antero-posterior diameter of inner condyle, | 7· |
| Width of supra-tendinal bridge, | 2·3 |
| Width of opening below bridge, | 1·9 |

This unique fossil was found by Mr. G. M. Keasbey, of the Yale party, in September last, near Henry's Fork, Wyoming.

### *Aletornis gracilis*, sp. nov.

A small aquatic bird, about the size of a Woodcock, is represented in our Wyoming collections by the proximal end of a humerus in excellent preservation, and probably by some less important remains. The species thus indicated may be placed provisionally in the genus *Aletornis*, until the discovery of additional material determines more closely its affinities. The portion of the humerus preserved resembles in its general characters the corresponding bone of the American woodcock (*Philohela minor* Gray), but differs from it in having the head broader vertically, and less prominently convex, and the shaft below the head subtrilateral in transverse section. Just beneath the head, on the palmar side, the surface is concave.

*Measurements.*

| | |
|---|---|
| Greatest diameter of proximal end of humerus, | 11·4 mm. |
| Greatest diameter of articular head, | 8·4 |
| Least diameter, | 3·6 |
| Least diameter of shaft below head, | 3·6 |

The specimen here described was found by Mr. H. D. Ziegler, in September last, near Henry's Fork, Wyoming.

### *Aletornis bellus*, sp. nov.

A diminutive species, about half the bulk of that last described, and which may for the present be referred to the same genus, is indicated by the distal end of a tarso-metatarsal, and probably by a few other isolated and less characteristic specimens. The tarso-metatarsal is similar in its essential features to the same bone in the Killdeer plover (*Ægialitis vociferus* Cass.), and about the same size. The outer, or fourth, metatarsal element, however, is more produced distally, and its extremity is obliquely compressed. The trochlear groove in the distal end of the third metatarsal is not quite in the middle, the outer articular surface being slightly the larger.

*Measurements.*

| | |
|---|---|
| Transverse diameter (approximate) of distal end of tarso-metatarsal, | 4· mm. |
| Antero-posterior diameter of distal end of third metatarsal, | 2· |
| Transverse diameter, | 2· |
| Antero-posterior diameter of fourth metatarsal at distal end, | 3· |
| Transverse diameter of shaft through lower foramen, | 3·1 |

The remains at present representing this species were found by the writer, at Grizzly Buttes, Wyoming, in September last.

### *Uintornis lucaris*, gen. et sp. nov.

A small bird evidently belonging to the Scansores, and probably related to the Woodpeckers, is represented by the distal end of a tarso-metatarsal in perfect condition, and by some other fragmentary remains of different individuals. These specimens indicate a bird about as large as the Golden-winged woodpecker (*Colaptes auratus* Sw.). The tarso-metatarsal bone has the second and fourth metatarsal elements divergent, and of nearly the same length distally. The fourth is the larger, and is turned obliquely inward and backward, its outer half being especially produced. The end of the third metatarsal is largest of all, and has its extremity cleft, somewhat unequally, by a deep groove. The shaft above the union of the three elements is broad, and nearly flat in front. On the posterior side it is somewhat concave. The foramen between the third and fourth metatarsals is small, and the groove above it quite narrow.

*Measurements.*

| | |
|---|---|
| Transverse diameter of tarso-metatarsal at distal end,.... | 4·8 mm. |
| Antero-posterior diameter of third metatarsal,.......... | 2· |
| Transverse diameter,.............................. | 1·7 |
| Antero-posterior diameter of fourth metatarsal,......... | 3· |
| Width of shaft through lower foramen,................ | 3·5 |

The type specimen of this species was found by the writer, last autumn, near Henry's Fork, Wyoming.

### *Catarractes affinis*, sp. nov.

A new species of *Catarractes*, somewhat larger than *C. lomvia* Linn., may be based upon a right humerus, which is entire, and in an excellent state of preservation. This specimen was referred to by the writer in the description of *Catarractes antiquus*, from the Tertiary of North Carolina,* and the latter is evidently a nearly related species. The present humerus, however, indicates a rather smaller bird, and differs from the corresponding bone of that species in several particulars. The shaft on the anconal side below the articular head is roof-shaped, instead of being rounded, and the impression for the insertion of the second pectoral muscle is broader, and has its margin less elevated. At the distal end, also, the grooves for the tendons of the triceps muscle are of unequal size in the present species, the one on the ulnar side being much the larger.

*Measurements.*

| | |
|---|---|
| Length of entire humerus,.......................... | 95· mm. |
| Greatest diameter of proximal end,.................. | 20·3 |
| Transverse diameter of distal end,.... .............. | 10· |

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* This Journal, vol. xlix, p. 213, March, 1870.

| | |
|---|---|
| Greatest diameter of articular head, | 14· mm. |
| Least diameter, | 7·2 |
| Greatest diameter of shaft at middle, | 7· |
| Least diameter, | 4·2 |

This interesting specimen, which belongs to the Academy of Natural Sciences in Philadelphia, was found by Dr. A. C. Hamlin, in the Post-pliocene clay near Bangor, Maine.

### *Meleagris altus.*

***Meleagris altus*** Marsh. Proceedings Philadelphia Academy, 1870, p. 11, and American Naturalist, vol. iv, p. 317. (***Meleagris superbus*** Cope, Synopsis Extinct Batrachia, etc., p. 239.)

This species, which was based on portions of four skeletons, resembles most nearly in size and general features the common wild Turkey of North America (*Meleagris gallopavo* Linn.). It may readily be distinguished, however, by its more slender proportions, and especially by the more elongated posterior limbs. The more important characters of the species were given by the writer at a meeting of the Philadelphia Academy (March 8th, 1870), when the discovery was announced, but through an oversight the communication did not appear in full in the Proceedings of the Society.

The humerus in this species apparently differs from that of the wild turkey in being proportionally longer, and in having the shaft straighter, or less sigmoid. The coracoid is elongated, and its lower end expanded transversely. Its pneumatic foramen is large, but not more so than is often seen in the wild turkey. The femur and tibia are both longer than in the latter species, the tibia especially so. The tarso-metatarsal is particularly slender and elongated. The hypotarsus, or calcaneal process, of this bone has two canals for the passage of tendons, but in the specimens preserved, although belonging to fully grown birds, there is no osseous bridge over the inner canal, as in the adult turkey and other gallinaceous birds. The bridge, if it existed in an ossified condition, was less massive than in the wild turkey; otherwise some portions of it would have been preserved in the present specimens. The calcar, or spur, of the male in this species was placed lower on the tarso-metatarsal than in the turkey.

*Measurements.*

| | |
|---|---|
| Length (approximate) of humerus, | 159·5 mm. |
| Greatest diameter of proximal end, | 42· |
| Greatest diameter of distal end, | 33· |
| Length of coracoid, | 122· |
| Transverse diameter of lower end, | 37·5 |
| Length of femur, | 150· |
| Transverse diameter of distal end, | 31· |

| | |
|---|---|
| Length of tibia, | 243· mm. |
| Transverse diameter of distal end, | 18· |
| Length of tarso-metatarsal, | 176·5 |
| Transverse diameter of proximal end, | 23· |
| Distance from proximal end to spur, | 110· |

The specimens here described were found in the Post-pliocene deposits of Monmouth County, New Jersey.

### *Meleagris celer*, sp. nov.

A much smaller species of the same genus is represented by two tibiæ and the proximal half of a tarso-metatarsal, which were found together, and probably belonged to the same individual. The tibia is slender, and has the shaft less flattened from before backward than in the last species. The distal half of the shaft has its anterior face more distinctly polygonal. From the head of the tibia, a sharp ridge descends a short distance on the posterior face, where it is met by an external ridge of similar length. The tarso-metatarsal has the external ridge of the proximal end more prominent, and the posterior tendinal crest more ossified than in the larger species. The remains preserved indicate a bird about half the bulk of *M. altus*.

*Measurements.*

| | |
|---|---|
| Length of tibia, | 183· mm. |
| Greatest diameter of proximal end, | 34· |
| Transverse diameter of shaft at middle, | 9·6 |
| Transverse diameter of distal end, | 16·5 |
| Antero-posterior diameter of outer condyle | 16· |
| Transverse diameter of proximal end of tarso-metatarsal, | 19· |
| Antero-posterior diameter, | 14· |

The known specimens of the present species are from the Post-pliocene of Monmouth County, New Jersey.

### *Grus proavus*, sp. nov.

An extinct species of Crane, somewhat smaller than *Grus Canadensis* Temm., is indicated in the collections of the Yale Museum by a nearly perfect sternum, a femur, and a few other less important remains, which are probably all parts of the same skeleton. The sternum apparently resembles most nearly that of the Sand-hill Crane, but differs essentially from it in not having the grooves for the coracoids meet on the median line. They are in fact separated from each other by a space nearly equal to the width of the adjacent groove. The sternum is, moreover, less constricted near the middle than in *G. Canadensis.* The femur differs from the corresponding bone in that species mainly in having the shaft less curved: in other respects the resemblance is close.

*Measurements.*

| | |
|---|---|
| Width of sternum between outer ends of coracoid grooves, | 45·mm. |
| Width of sternum at middle,........................ | 39· |
| Distance between coracoid grooves,.................. | 5· |
| Length (approximate) of femur,...................... | 126· |
| Transverse diameter of shaft at middle,............... | 12·5 |
| Transverse diameter of distal end,................... | 26· |

The remains on which this species is established are likewise from the Post-pliocene deposits of Monmouth County, New Jersey.

Yale College, New Haven, August 28th, 1872.

## ART. XXXII.—*On the Oviducts and Embryology of Terebratulina;* by EDWARD S. MORSE, Ph.D. With Plate III.

FOR several years past I have made a special study of the Brachiopoda. The publication of the results of these investigations has been purposely delayed, till I could incontestably demonstrate the genital nature of the Cuvierian hearts, so plainly shown to be oviducts by Hancock and Huxley, and till something at least could be given of the embryology of some brachiopod. For these two matters I have visited Eastport, Maine, for the third time, and now my heretofore fruitless endeavors have been met with success.

The results of these observations were communicated at the 19th of June meeting of the Boston Society of Natural History. I had before seen the ciliary lining of the oviducts in Lingula and Terebratulina, but I wished to see the eggs in their actual passage through the tubes. This I have now repeatedly observed in Terebratulina. The eggs were seen discharged from the sinuses in the pallial membrane, afterward floating freely in the perivisceral cavity; the eggs were then seen gathered at the trumpet-shaped mouth of the oviduct, and have been watched as they were slowly passing through the tube and have been caught as they were discharged at the external orifice. These eggs have then been followed in their development until they assumed the form of a deeply annulated embryo, composed of four distinct rings, which had a marked vermian contraction upon each other. At this stage they appeared to be attaching themselves by the caudal segment. During the latter part of this examination my embryos were unfortunately lost. I had not the neccessary appliances to keep the water at the frigid temperature they were accustomed to, and the increased temperature of the water led to a rapid development of Paramæcia, and other infusoria, and my poor embryos were ruthlessly eaten up. I have, however, nearly three hundred outlines of the embryos during their development, a few of which are presented with this brief communication. Next year it is hoped a complete history of their development will be made, as many things have been observed in their proper management of which I shall profit in my next attempt.

There was also discovered prominent glands at the external openings of the oviducts in Terebratulina, which I have every reason to believe represent the testes. These glands surrounded the external orifice of the oviducts, which protruded somewhat from the anterior walls of the body, and the glands were invariably found filled with spermatozoa.

From Eastport, Maine, I hurried to the St. Lawrence, with the hopes of securing some data regarding the embryology or early stages of another brachiopod found there, *Rhynchonella psittacea*. I was altogether too late for this, but had the pleasure of studying *Rhynchonella alive*, to note the ciliary action in the oviducts driving currents outward, and to establish the correctness of Owen's supposition that the arms of Rhynchonella can be protruded. A jar of specimens dredged by Dr. P. P. Carpenter, who kindly accompanied me from Montreal, was left standing undisturbed for twenty-four hours, when one of the specimens protruded its arms their entire length from the partially opened shells. I poured the sea water carefully out, and suddenly poured in the strongest alcohol, and the specimen is now preserved in this exerted position.

John E. Gavit, Esq., and Dr. Thomas T. Sabine of New York, followed all my examinations at Eastport. In a forthcoming memoir of the Boston Society of Natural History all the details of these examinations will be given.

### EXPLANATION OF PLATE III.

*Genitals.*

Figure 1. Glandular organs supposed to be testes, seen from below.
" 2. Portion of left oviduct with its relation to the supposed testis. *a*, oviduct. *b*, its external opening. *c*, testis.
" 3. Left oviduct as it appears from the front through perivisceral wall. *a*, oviduct. *b*, its external opening. *c*, internal opening. *d*, ovaries in pallial membranes. *e*, left divaricator muscle. F. F. F. Eggs entering, passing through, and escaping from oviduct.
" 4. Right oviduct seen from behind. *a*, intestine. *b b*, anterior occlusor muscles. *c*, oviduct. *d*, internal mouth of oviduct held in the ilio-parietal band "like a landing net in its loop." *e*, ilio-parietal band. *f*, ventral mesentery. *g*, accessory heart of Hancock.
" 5. External orifice of oviduct.

*Note.*—The severed portion of intestine is thrown into folds, in consequence of the contraction of the outer wall of intestine.

*Embryology.*

Figs. 1 to 12, showing various stages of embryo.
Figs. 6 and 8, partial side views.
Figs. 7 and 11, side views.
Fig. 12, partial end view.

## ART. XXXIII.—*Erratum of the Errata*, or "*A Few Millions*;" by ALFRED M. MAYER, Ph.D.

I AM indebted to Mr. A. Cowper Ranyard, of London, for calling public attention to errors existing in the illustrative appendix to a research entitled "Acoustical Experiments, &c.," which article of mine the editor of *Nature* honored with a republication in his journal, on May 9, 1872.

The existence of these errors has been known to me since a few weeks after the original publication of my paper; but as they did not affect in the least the subject proper of the research, and would be apparent to anyone who might take the trouble to review the calculations, I allowed them to pass unnoticed; and even now I would not pursue the subject further had Mr. Ranyard really corrected my errors; but he has *himself* committed the error of "A Few Millions" (the title of his communication),* which he would attribute to *me*, when, in these words, he undertakes the correction of my figures: "Taking the velocity of light as 185,300 miles per second, and the wave-length of $D_1$, as given by Angström, at 0·00058950 millimetres, gives 5,058,700,000,000,000 vibrations per second, or a little more than *five thousand millions of millions*, instead of a little less than *six hundred millions of millions* vibrations per second, as given by Dr. Mayer." The following is the correct calculation:

$$185{,}300 \text{ miles} = \frac{298{,}212{,}000{,}000 \text{ millimeters}}{\cdot 0005895 \text{ millimeter}} = 505{,}870{,}000{,}000{,}000$$

and 5,058,700,000,000,000 (Mr. R.'s result) minus 505,870,000,000,000 (Mr. M.'s result) gives Mr. Ranyard 4,552,830,000,000,000 tremors.

Thus it appears that both Mr. Ranyard and myself can commit errors in simple arithmetic; but I am sure that our mutual friends will not attribute them to want of sufficient mathematical culture to accomplish "a simple rule-of-three sum." (A.C.R.) He that is without sin let him first cast a stone. I, however, do not wish Mr. Ranyard's errors in any way to extenuate my own greater negligence, which has difigured the appendix of my paper; containing, as it does, "some strange numerical errors, which perhaps it will be well to point out, lest some of your readers should make use of the numbers given at the end of the paper without previously testing them." (A. C. R.) I will, therefore, ask my readers to substitute the following for the second paragraph under the heading of

"*Quantitative Relations in the experiments and analogical facts in the phenomena of light.*"

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* See *Nature*, June 20, 1872, p. 142.

We will now examine the analogical phenomena in the case of light. Let fork No. 1, giving 256 vibrations a second, stand for 508,730,000,000,000 vibrations a second; which will be the number of vibrations made by the ray $D_1$ of the spectrum, if we adopt 300,000 kilometers per second as the velocity of light. Then fork No. 3 will represent 504,750,000,000,000 vibrations per second; which latter give a wave-length ·0000048 millimeter longer than that of $D_1$ and belong to a ray removed from $D_1$, toward the red end of the spectrum, by eight times the distance which separates $D_1$ from $D_2$. We saw that fork No. 3, giving 254 vibrations a second, had to move toward the ear with a velocity of 8·734 feet to give the note produced by 256 vibrations per second, emanating from a fixed fork; so, if a star, which only sends forth those rays which vibrate 504,750,000,000,000 times a second, should move toward the eye with a velocity of 2,442 kilometers, or 1,517 miles, its color would change to that given when $D_1$ emanates from a stationary soda-flame.

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## ART. XXXIV.—*On some points in the Geology of the Southwest;* by E. W. HILGARD, of the University of Mississippi.

THE third annual Report of the Geological Survey of Louisiana, by Prof. F. V. Hopkins, contains a number of statements and discussions controverting, apparently at least, some of the facts and views heretofore published by me, especially as regards the quaternary history of the Mississippi Valley and Gulf of Mexico. While some of the points made by my respected friend are based merely upon misunderstandings, there are others which result from material differences in the apprehension of facts, and as such require notice at my hands.

As regards the inadmissibility of Prof. Hopkins's conjecture that the Labrador current may have been instrumental in distributing the drift over the Mississippi Valley, I have little to say that could add to the cogency of Prof. Dana's remarks on the same subject, in the August number of this Journal. The southern drift bears everywhere the character of a deposit formed by "fresh water in a state of violent flow, and devoid, or nearly so, of animal life," as I have repeatedly stated, on the strength of an array of evidence against which my friend does not bring a single paleontological, lithological, or stratigraphical fact. For the origin of that flood I do not hold myself responsible; but I must demur to the broad and unqualified statements, that the drift "must date from the period of depression," and that "during a long period after the deposition of the drift the land stood at about its present level, allowing the valleys to be cut into the drift, etc." If, as Prof. Hopkins assumes, the continent was so far depressed as to allow the Labrador current to sweep down the Mississippi Valley, how does he account for the occurrence of the large rounded pebbles in the Calcasieu wells, at a depth of 450 feet, and the extension of the drift beds (assumed by himself, and doubtless correctly) far out beneath the Gulf of Mexico? Or is he prepared to admit that an ocean current, with a minimum depth of 1,150 *plus* 450 feet on the Gulf shore, could roll pebbles over hundreds of miles of "continent" or plateau, and then and there form deposits with the wavy stratification of river alluvium, and totally devoid of marine fossils? So far as our experience goes, *shallow running water*, or the flux and reflux of waves and tides, alone produce such structure; and, if so, the Gulf shore must have been elevated to the extent of at least 450 feet above its present level, at the time the Calcasieu drift was deposited.† Moreover, the Calcasieu profiles‡ show most convincingly the existence of ridges of denudation at the drift surface, as well as beneath it; and, similarly, subterranean ridges of drift material are frequently struck in wells on the Mississippi coast.† The drift materials are, equally, the last thing so far found beneath the Port Hudson clays in the Mississippi bottom;—to what extent they have filled up that ancient trough, future borings must determine.

I cannot, therefore, see on what grounds Prof. Hopkins assumes that the erosion of the drift surface took place while the land stood "nearly" at its present level. If, as I expect, drift gravel should be found underlying the strata of the New Orleans well, the minimum elevation of the Gulf coast, during and even after the Drift period, would be increased by several hundred feet. And I cannot refrain from once more calling attention to the obvious difference between the chemical status of the stratified drift of the South, and that of Illinois and Indiana. In the latter, lignitized trees and layers of muck are abundant, indicating submersion at a comparatively recent period; while the "orange sand" of the Southwest, as heretofore repeatedly stated by me, as a rule contains nothing that is capable of further oxidation or solution by atmospheric agencies, unless it be silex. Such complete peroxidation and lixiviation, the effects of which have largely extended into underlying formations,‖ unquestionably indicate a long subaërial exposure, from which the Northwestern stratified drift was in a great measure exempt.

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* This Journal, November, 1869, p. 335.

† Ibid, p. 344.

‡ Miss. Report, 1860, pp. 28 and 29.

‖ Miss. Rep., 1860, p. 23.

As regards the later formations, I note that the propriety of substituting Dana's prior name of "Champlain period" by that of "Bluff period," as proposed and carried out by Prof. Hopkins, seems to me at least doubtful. A *descriptive* name should at least give the predominant and essential character of the greater part of the formations concerned. That Swallow's name, as applied to the Loess, is preëminently characteristic, no one that knows that formation, as invariably exhibited on the Mississippi and its great tributaries, will deny; while, apart from the Port Hudson bluff itself, few, probably, besides Prof. Hopkins and myself, know of any prominent example of bluffs formed by the Port Hudson strata—a formation as positively characterized by plateaus and prairies, from Pensacola to the Rio Grande, as the Loess is by "bluffs." As for the Yellow Loam and its equivalents, spread like a blanket over the whole country, up hill and down, it is peculiarly apt, *if in situ*, to be absent from *bluff* banks.

The name apart, I am constrained to believe that, while apparently differing widely from me in his interpretation of the strata penetrated in the New Orleans artesian well of 1856, he nevertheless agrees, substantially, in all but the use of a name. For when, on p. 185, he speaks of the Port Hudson strata as "the delta formed by the Mississippi, from the end of the Drift period to the beginning of the era of the Loess," he merely differs from me in conferring the name of the "Mississippi" upon that broad expanse of swamps, marshes, and lagoons which then filled the trough remaining after the Drift period, and through which the continental waters made their way as best they might. In this broad sense, I cheerfully admit the whole of the strata underlying New Orleans to be "Mississippi delta deposits." Similar ones, however, were at that time forming all along the northern and western Gulf border, constituting the "blue clay bottom;" which, is as well known on the coast of Alabama and Texas, as is the sudden seaward slope at a variable distance from the main land, that Prof. Hopkins erroneously supposes to be peculiar to the mouths of the Mississippi, and to be formed by river deposit.

But while the modern delta deposits proper everywhere exhibit an abundance of drift-wood particles, and a rapid alternation of character corresponding to the frequent rise and fall of the river: the deeper deposits of the New Orleans well lack both these characteristics, being remarkably uniform through considerable thicknesses of material.* True, the shells so far as previously known are of living species; but so far as I am aware, nothing else is expected of quaternary marine beds. Yet the fact that three or four of the species are not now known to be

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* See "Report on the Age of the Delta," in Rep. U. S. Eng. Dept. for 1870.

living in the Gulf or elsewhere, conveys a hint that when these "delta" deposits were made, the present state of things had not come to pass. From the somewhat arbitrary standpoint of some paleontologists, the strata in question would even have to come under the head of marine Pliocene!

I confess, also, to a violent distrust of the chemical method of identifying formations, as applied by my friend to deposits so exceedingly variable in their nature, and over such extensive areas. Had he gone to New Orleans instead of Arkansas, he might have found in the "drove wells" of that city about as great a variety of waters, as the two extremes he refers to as characterizing the Port Hudson and river deposit waters, respectively. That "at various points between Baton Rouge and Arkansas, the alluvium is over a hundred feet in depth," I have not the slightest doubt; for the same is true of the Mississippi river, whose ancient channels everywhere traverse the bottom. I therefore seriously demur to the sufficiency of the proof intended to be conveyed, that such is the least average depth of the alluvium. All direct stratigraphical observations heretofore made have led the observers to a contrary conclusion.*

As regards the Loess and Yellow Loam, I observe that Prof. Hopkins assigns to the age of the latter sundry deposits partly of earlier, partly of later date, whose nature and distribution differs entirely from that of the Yellow Loam proper. His description of the material as "a clayey silt, retaining a remarkable fineness of character over large areas," can apply but very rarely indeed to the formation I have so designated; which is predominantly a loam or brick clay, always containing more or less coarse sand, but modified *somewhat*, in accordance with the nature of the underlying strata,† in Louisiana as well as elsewhere. This is the case even where it is in situ; but where, as in the case mentioned by him as occurring on Sicily Island (p. 177), it is merely a *talus*, commingled with the other materials furnished by the degradation of the hills, it of course will be changed according to the nature and amount of the admixture.‡ I doubt that there is any Yellow Loam to be found *in situ* on Sicily Island.

The fine, more or less indurated silts, forming perpendicular walls when eroded, to which Prof. Hopkins refers, are clearly anterior in time, and distinct from the Yellow Loam proper; as may be seen at Port Hudson itself, and at numerous points along the edge of the Loess region in Mississippi, where a gradual transition into the Loess proper is frequently observable. It is

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* See Humphrey's and Abbot's Report on the Mississippi river; this Journal, December, 1871, p. 402; Proceed. Am. Assoc. Adv. Sci., 1871, p. 252.

† Miss. Rep. 1870, p. 197, et al.

‡ Miss. Rep. 1860, pp. 319–20, and 198, §334.

for this reason that I have regarded the upper yellow silt stratum at Port Hudson as the probable equivalent of the Loess, or at least of its lower portion;[†] and the same applies, in my view, to the materials exhibited as forming the level portion of Sicily Island, on the banks of Lake Louis; as well as, judging from description, to the body of the Bayou Maçon hills. Similar materials underlie the very well defined Yellow Loam stratum on the fertile Côte Gelée, and the Opelousas prairie; while farther west, where these silts themselves have formed the soil, we have the poor white, putty-like soils of the Calcasieu pine-flats and "Bay Galls;" or the ashy ones of the pine prairies. That the character of the Yellow Loam proper was still substantially the same even in this latitude, is shown by its outliers on the Five Islands, where (e. g., at Weeks' island) the loam is undistinguishable from that overlying the Loess in Adams and Jefferson counties, Miss.

In the upland regions of Louisiana, as well as in those of the adjoining States, there is generally little difficulty in recognizing the Yellow Loam, where the ridges are not too abrupt; except in so far that it is oftentimes not easy to determine whether it is in its original place, and that sometimes a hardpan transition stratum between it and the drift materials[*] is alone left on the hilltops. It is only near the larger river channels that its true geological relations can be clearly observed.

Of course, not only do we find all over the country materials of more or less indefinite position and composition, resulting from the action of the aqueous and atmospheric agencies that have been at work since the deposition of the Loam; but at times we also find the geological place of the Loam occupied by materials bearing not the least resemblance to its usual facies. Such is the case, e. g., on the sandy uplands of south-east Mississippi, where a 5 to 8 foot stratum of sandy hardpan, fertile and quite distinct from any materials of the drift, caps all the ridges.[†] Its transitions into the common Loam confirm the conclusion derived from its position; just as is the case in the cretaceous prairies and "flatwoods."[‡]

Concerning the Grand Gulf group, I have to remark that I must differ from my friend as to the uselessness of "speculating upon the absence of animal life in the waters that deposited it, until its outlines shall have been fully made out in Texas." Nothing that can be found in Texas or elsewhere can invalidate the facts observed in Alabama, Mississippi and Louisiana, near to the main axis of the Mississippi embayment; where the maximum development of all the post-cretaceous formations on the Gulf border has manifestly taken place. If a slice of formation 125 miles wide at one point, over 200 feet in thick-

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[*] This Jour., Jan., 1869, p. 80.

[†] Miss. Rep., p. 198, §335.

[‡] Miss. Rep., p. 304.

[‖] Ibid., pp. 198–99.

ness, and known to extend through six degrees of longitude with a remarkable uniformity of character, may not speak for itself, we shall have to suspend our discussions of a large portion of the geology of the globe.

However, I am in possession of data and specimens from Texas, sufficient to show the approximate correctness of the outline given in my "Map of the Mississippi Embayment," as well as the close correspondence of the character of the formation in that State, to that exhibited by it in the Anacoco region, in western Louisiana.

To the two localities of Cretaceous outcrops mentioned by Prof. Hopkins, I have to add another, viz: at King's Salt Works, in Bienville; where a genuine "rotten limestone" forms the bed of Bayou Castor.

University of Miss., August, 1872.

## ART. XXXV.—*Contributions from the Sheffield Laboratory of Yale College.* No. XXV.—*Results of a Chemical Investigation of some Points in the Manufacture of "Malleable Iron;"* by RUSSELL W. DAVENPORT, Ph.B.

THE annealing process employed in making malleable iron consists, as is well known, in packing the castings with oxide of iron scale in cast-iron chests, placing six or eight of these chests upon the hearth of a kiln or furnace, resembling a reverberatory furnace, and exposing them for five or six days to a bright red heat; the furnace is then allowed to cool, and the castings, as soon as they can be handled, are ready for finishing. The following analyses, made of two samples about ¼ inch in thickness, each annealed twice and analyzed before and after each annealing, show what influence the process has upon the impurities contained in the iron. It will be seen the iron used was a fairly good charcoal-iron. The unannealed castings, when broken, showed a white fracture, all the carbon being in the combined state. This last property must be possessed by all castings to insure the success of the process. The annealed castings when broken were up to the average toughness of "malleable iron," and their strength did not materially decrease after the second annealing.

I. *Casting No.* 1. *Before annealing.*

| | 1. | 2. | Average. |
|---|---|---|---|
| Silicon, | ·44 | ·45 | ·445 |
| Phosphorus, | ·29 | ·34 | ·315 |
| Manganese, | ·524 | ·534 | ·529 |
| Sulphur, | .064 | .054 | ·059 |
| Total Carbon, | 3·44 | 3·42 | 3·43 |

II. *Casting No.* 1. *After first annealing.*

| | 1. | 2. | Average. |
|---|---|---|---|
| Silicon, | ·440 | ·436 | ·438 |
| Phosphorus, | ·323 | ·330 | ·327 |
| Manganese, | ·57 | ·60 | ·585 |
| Sulphur, | ·062 | ·072 | ·067 |
| Total Carbon, | 1·53 | 1·49 | 1·51 |

III. *Casting No.* 1. *After second annealing.*

| | 1. | 2. | Average. |
|---|---|---|---|
| Silicon, | ·447 | ·451 | ·449 |
| Phosphorus, | ·31 | ·32 | ·315 |
| Manganese, | ·51 | ·54 | ·525 |
| Sulphur, | ·086 | ·081 | ·083 |
| Total Carbon, | below 0·10 per cent. | | |

IV. *Casting No.* 2. *Before annealing.*

| | 1. | 2. | Average. |
|---|---|---|---|
| Silicon, | ·59 | ·58 | ·585 |
| Phosphorus, | ·29 | ·27 | ·280 |
| Manganese, | ·55 | ·62 | ·585 |
| Sulphur, | ·11 | ·10 | ·105 |
| Total Carbon, | 3·50 | 3·43 | 3·48 |

V. *Casting No.* 2. *After first annealing.*

| | 1. | 2. | Average. |
|---|---|---|---|
| Silicon, | ·616 | ·612 | ·614 |
| Phosphorus, | ·290 | ·291 | ·290 |
| Manganese, | ·619 | ·613 | ·616 |
| Sulphur, | ·152 | ·143 | ·147 |
| Total Carbon, | ·43 | -- - | ·43 |

VI. *Casting No.* 2. *After second annealing.*

| | 1. | 2. | Average. |
|---|---|---|---|
| Silicon, | ·615 | ·613 | ·614 |
| Phosphorus, | ·29 | ·30 | ·295 |
| Manganese, | ·59 | ·56 | ·575 |
| Sulphur, | ·161 | ·163 | ·162 |
| Total Carbon, | below 0·10 per cent. | | |

From the above analyses the following conclusions may be drawn; first, that the *silicon*, *phosphorus* and *manganese* are in no way affected by the annealing process; second, that the amount of sulphur is not diminished and may be slightly increased; and third, that the amount of carbon is reduced by each annealing until finally a mere trace remains. The slight increase of sulphur shown by both sets of analyses is probably due to the presence of that substance in the coal used for fuel. In regard to the change in the carbon a word must be said. The castings before annealing, containing 3½ per cent of combined carbon, showed, on breaking, a white fracture, and were too hard to be cut by a drill; after the first annealing an interesting change showed itself in the fracture; a whitish surface extended in about $\frac{1}{16}$ of an inch on all sides, surrounding a dark core of dull black color; the line of change from the light to the dark was quite distinct, and the whole was easily cut by a drill. A portion of this white outside layer was filed off and the carbon determined to be present only in traces, while analyses II. and V. show the presence of a considerable amount of carbon, when a sample of the entire cross section was taken. The black core was noticeably smaller in the case of casting No. 2 than in casting No. 1, which accounts for the small amount of total carbon in analysis V. After the second annealing the black core had entirely disappeared in both cases, the whole fracture being of the same appearance as the white border mentioned above, the amount of carbon in a sample of the whole cross section, as shown by the analysis, was reduced to a trace. It would appear from the above that when a casting does not much exceed $\frac{1}{8}$ of an inch in thickness, the carbon is approximately eliminated throughout the whole mass by the ordinary annealing process; when, however, the casting is thicker, the elimination only extends from the surface into the mass for a certain distance, but may be carried farther in by a repetition of the process. It would also seem that in the interior of a thick casting, where the amount of carbon is at all events only partially reduced, that which remains is by the high heat and subsequent slow cooling changed in its state of occurrence from combined carbon to a species of uncombined or graphitic carbon; for where the iron before annealing is white and very hard, after annealing it shows a dark fracture and is quite soft. Its behavior, too, with nitric acid would lead to the same conclusion, for while the white unannealed iron dissolved perfectly in that reagent, upon standing a few hours, and gave to the solution the same clear brown color that is noticed when a high steel is so treated, the annealed "black heart," as it is technically called, gave a dirty green color to the solution and a black carbonaceous residue remained.

The manufacturers of "malleable iron" are occasionally troubled by a lack of toughness in the annealed castings when these are exposed to a sudden blow or to a bending strain. This weakness is at times doubtless caused by the natural rottenness of the iron owing to the presence of an excessive amount of silicon, phosphorus or sulphur; but it also must frequently be due to a crystalline structure which the iron under certain unknown conditions assumes while being annealed. This structure shows itself in the fracture of an annealed casting in the form of bright crystalline faces which occasionally extend entirely across the fracture.

Further analyses were made of another specimen, before and after its annealing, which when, annealed and broken was brittle, and showed the crystalline structure to some extent.

VII. *Before annealing.*

| | 1. | 2. | Average. |
|---|---|---|---|
| Silicon, | ·577 | ·580 | ·579 |
| Phosphorus, | ·425 | ·423 | ·424 |
| Manganese, | ·154 | ·117 | ·165 |
| Sulphur, | ·116 | ·112 | ·114 |
| Total Carbon, | 3·277 | 3·285 | 3·281 |

VIII. *After annealing.*

| | 1. | 2. | Average. |
|---|---|---|---|
| Silicon, | ·560 | - - - - | ·560 |
| Phosphorus, | ·46 | ·44 | ·450 |
| Manganese, | ·136 | ·158 | ·147 |
| Sulphur, | ·113 | - - - - | ·113 |
| Total Carbon, | below 0·10 per cent. | | |

The weakness in this case may perhaps be partially caused by the large amount of phosphorus present, but the next two analyses made of specimens, which when broken after being annealed were very brittle and showed a most decided crystalline structure, go to prove that this phenomenon of crystallization *cannot* be attributed to the presence of an excessive amount of silicon, phosphorus or sulphur.

IX. *Once annealed, large crystalline faces in fracture.*

| | 1. | 2. | Average. |
|---|---|---|---|
| Silicon, | ·44 | ·46 | ·450 |
| Phosphorus, | ·267 | ·266 | ·266 |
| Manganese, | ·264 | ·182 | ·223 |
| Sulphur, | ·145 | ·133 | ·139 |
| Carbon, | below 0·10 per cent. | | |

X. *Twice annealed, crystalline faces extended entirely across the fracture.*

| | 1. | 2. | Average. |
|---|---|---|---|
| Silicon, | ·585 | ·593 | ·589 |
| Phosphorus, | ·213 | ·212 | ·212 |
| Manganese, | ·149 | ·158 | ·153 |
| Sulphur, | ·092 | ·118 | ·105 |
| Carbon, | none or slight trace. | | |

The above analyses to seem afford no explanation of this crystalline structure, and the cause of it can only be determined by careful experimenting and by the comparison of a large number of trustworthy analyses.

The next analysis was made of an annealed casting which when bent showed a greater degree of toughness than common.

It was of circular section ½ inch in diameter, and was bent cold through an angle of 90° without showing fracture.

| XI. | 1. | 2. | Average. |
|---|---|---|---|
| Silicon, | ·717 | ·722 | ·719 |
| Phosphorus, | ·206 | ·202 | ·204 |
| Manganese, | ·273 | ·268 | ·270 |
| Sulphur, | ·035 | ·037 | ·036 |
| Total Carbon, | 1·840 | 1·844 | 1·842 |

From this analysis it may be inferred that the silicon may run as high as 0·7 per cent without effecting the toughness of the annealed product, while it also tends to show, what might certainly be expected, that an iron low in phosphorus and sulphur is most suitable for making malleable iron.

In regard to the chemical processes used in making the above analyses, in most of the important points I followed the methods for the analysis of iron and steel, given in the last American edition of Fresenius, and I departed from these methods only in such details as Prof. Allen, of the Sheffield Scientific School Laboratory, kindly recommended. All the specimens examined except, No. XI, were obtained from Messrs. O. B. North & Co., of New Haven, whose courtesy in specially preparing and re-annealing the iron for this investigation is gratefully acknowledged.

## ART. XXXVI.—*Descriptions of a few new species, and one new genus, of Silurian Fossils, from Ohio*;* by F. B. MEEK.

### PROTASTER? GRANULIFERUS Meek.

DISK small, apparently circular; rays rather slender, and of unknown length. Dorsal surface of disk and rays covered by an integument composed of innumerable minute grains of calcareous matter. Ventral side of disk not well exposed in the specimen but apparently provided, in the interradial spaces, with minute spines directed outward. Oral pieces not well exposed in the specimen. Arm-pieces regularly alternating, but apparently rectangular at their inner ends, and not interlocking along the minute mesial impressed line, longer transversely than in the direction of the length of the rays; each largely excavated at its anterior outer end so as to form a large pore, or pore-like depression, and divided transversely by a furrow into two parts, the anterior one of which is very short, and the posterior longer and marked by a minute pit at its

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* These fossils are to be fully illustrated and described in the report of the Ohio Geological Survey.

inner end; about eight or nine of these pieces in each range of each ray included within the margin of the disk. Outer arm-pieces (adambulacral of some) smaller than those of the inner ranges, and placed edge upward, with an oblique outward direction so as to imbricate outward or toward the extremities of the rays; each bearing one or more minute articulating spines.

Breadth of disk, about 0·43 inch: breadth of arms at their inner ends, 0·10 inch.

The only specimen I have seen that is certainly known to belong to this species is very imperfect, being merely an incomplete disk, and the inner ends of the rays. It does not conform to the characters of *Protaster* given in Prof. Forbes's diagnosis, in all respects, since its disk, especially on the upper side, is covered by an integument composed of a vast number of very minute grains of calcareous matter, instead of distinct imbricating scales. It is therefore not improbable that perfect specimens would show other characters that would warrant the establishment of a new genus or sub-genus for such forms, in which case the name *Alepidaster* might be applied to the group, which would probably also include *Protaster gregarius* of Meek and Worthen.

I have intentionally avoided, in the foregoing description, the use of the terms ambulacral and adambulacral pieces, applied by some in describing the arms of species of *Protaster* and similar forms, because it seems doubtful whether these terms can be properly applicable to such types. I should certainly think not, *if these types belong to the Ophiuroidea* (in which no ambulacral furrows exist) instead of to the *Asteroidea.* According to Dr. Wright, however (see Brit. Foss., *Echinodermata*, p. 32), *Protaster Miltoni* of Salter has a well developed madreporiform body, and hence would belong to the *Asteroidea.* Yet it is very curious that these types seem to have no proper ambulacral canals, and we have apparently no positive evidence that the viscera of the animal were not confined to the disk, as in the *Ophiuroidea.*

*Locality and position.*—Middle part of Cincinnati group of the Lower Silurian, Moore's Hill, in Indiana; Mr. Dyer's collection.

### PALÆASTER INCOMPTUS M.

Small; rays rather short, or only about once and a half as long as their breadth at the inner ends, and rapidly tapering to their outer extremities, which are more or less angular. Disk equaling in breadth the length of the rays. Dorsal side of rays composed each of three rows of about nine pieces* each, that

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* In some of the rays these appear, but this is probably due to the exposure of one of the marginal rows of the rays by oblique pressure.

are wider than long, and increase rather rapidly in size inward to the margin of the disk, which is made up of smaller pieces; a few very minute pieces apparently sometimes occur between the rows on the dorsal side of the rays. Surface of dorsal pieces a little roughened, but apparently without spines. Madreporiform piece rather small, a little oval, or almost circular, nearly flat, and marked by very fine, irregularly interrupted radiating striæ. Ventral side unknown.

Greatest breadth across, between the extremities of rays on opposite sides, 0·90 inch; length of rays, 0·35 inch; breadth of same at inner ends, about 0·22 inch; length of madreporiform pieces, 0·08 inch; breadth of same, 0·07 inch.

This species seems to be related to *P. matutinus* Hall, but has proportionally shorter and broader rays, that are also less pointed at the extremities. Its dorsal pieces are also less close-fitting, and appear to have at some points a few very minute intercalated pieces between the rows of the rays. This latter character, however, is not clearly visible in the specimen. A more important distinction is the absence, in the species under consideration, of the circle of stellate pieces seen on the dorsal side of the disk of *P. matutinus*, which, according to Prof. Hall's figure, published in the 20th Regents' Report, seems to want the well-defined madreporiform pieces seem in the species under consideration, or at least not to have it distinguishable from four other radiately marked pieces that alternate with the stellate pieces mentioned. As in that species, this has the two inner marginal pieces, that connect with each other at the axis of the rays, rather decidedly larger than those next to them, but of different form, being wider than long instead of the reverse, or with their greater diameters arranged transversely, and each provided with a kind of mesial protuberance on the inner side.

The only specimen of this species I have seen is firmly attached to a foliated expansion of coral, so as to conceal the ventral side entirely. It was evidently lying dead on its back, on the bottom of the sea, when the coral commenced growing upon its ventral side; and afterward the coral not only covered this side entirely, but grew some distance in all directions, even beyond the extremities of its rays. This gives it much the appearance of having firmly attached itself to the coral parasitically; but this is evidently not the case, because it is its entire *ventral* side that is attached out to the very extremities of its rays.

*Locality and position.*—Cincinnati group of the Lower Silurian, at Cincinnati, Ohio. Mr. Dyer's collection.

*Note.*—A fine star-fish recently sent to me by Mr. Dyer, from the Cincinnati rocks, presents many features indicating close relations to *Palæaster granulosus* of Hall; and yet a critical comparison with his description (he has not yet published a figure of that species), leads me to think it most probably distinct. It has about the same *proportional* length and breadth of rays; but instead of being "obtusely rounded at the extremities," the rays are acutely angular at the ends. Again, instead of having the marginal and adambulacral pieces numbering, in a space of one inch and a quarter from the apex of a ray, twenty-five of the first, and forty-two to forty-three of the latter, on each side of the ambulacral furrow, it shows in the same space *twenty-eight* marginal and *thirty-two* adambulacral pieces on each side — the number of adambulacral pieces being about ten less on each side in the same space from the apex—a rather decided difference, thus also showing the inner rows to be composed of proportionally larger pieces. Its two rows of ambulacral ossicula are, as in *P. granulosus*, composed of short wide pieces (numbering about thirty-two in the space given above), with each a linear-ridge running across its entire breadth as in that species, but this ridge does not show the obliquity mentioned in Prof. Hall's description.

On the upper side there are near the middle, of each ray, about fourteen rows of small alternating convex pieces. All of the pieces are granular, and each of the marginal and adambulacral pieces has a minute tubercle for the articulation of a small spine, the largest of which are about 0·10 inch in length. Those of the dorsal side also seem to have supported much more minute spines. The specimen is not in a condition to show the oral pieces, or the madreporiform piece.

It differs materially from *P. Dyeri*, not only in being much smaller, but in the entirely different form and more numerous rows of its dorsal pieces, which are also decidedly more close-fitting. If this should prove to be a new species, I would propose to call it *Palæaster speciosus.*

Its dimensions are as follows: length of rays 1·40 inch; breadth of do. as flattened somewhat by pressure, 0·62 inch; breadth of disk, about 0·80 inch.

### RHYNCHONELLA NEGLECTA, *var.* SCOBINA.

Shell rather small, sub-trigonal, compressed, or sometimes in large specimens quite gibbous, with the mesial fold very prominent and narrow. Dorsal valve bearing on the mesial fold four plications, the middle two of which are more prominent and larger than the other two, one of which latter occupies each slope of the fold; lateral slopes each provided with about six angular plications; beak incurved. Ventral valve with mesial sinus corresponding to the fold of the other valve, and bearing three plications, the middle one of which is usually somewhat larger than the others; lateral slopes each with about six plications; beak small, pointed and arched, but not strongly incurved, projecting moderately beyond that of the other valve. Surface ornamented by fine marks of growth, and numerous minute, distinct, regularly disposed granulations.

Length of one of the largest, and most gibbous specimens, 0·55 inch; breadth of do., 0·50 inch; convexity of same, 0·53 inch. Other individuals of near the same length and breadth have a convexity of only 0·25 inch.

This form agrees almost *exactly* in general outline, as well as in the number and arrangement of the plications, with *A. neglecta* Hall, from New York, Niagara and Clinton groups. But if that shell has been correctly figured and described, from well-preserved specimens, it must be distinct from this, as there are no surface granulations illustrated in the figures or mentioned in the description of the New York species; while they are quite *distinctly and beautifully* defined on those before me from Ohio. As the New York Niagara fossils are usually found in a good state of preservation, it is very improbable that such a character could have escaped attention in *R. neglecta.* I therefore feel strongly inclined to regard the shell under consideration as a distinct species; but as it agrees so closely in other characters with *R. neglecta*, have concluded to view it, for the present, as a variety of that shell, under the name *scobina*, which can be retained for it should it prove, as I think it will, to be a distinct species.

It is possible that the internal characters of the species will be found to differ generically from those of *Rhynchonella*, a distinctly granulated surface being unusual in that genus.

*Locality and position.*—Clinton group, Dayton, Ohio. Found by Prof. Orton.

### PLEUROTOMARIA (SCALITES?) TROPIDOPHORA M.

Shell rather small, obliquely rhombic in general outline, as seen in a side view; height somewhat greater than the breadth; spire conical, with an apical angle varying from 70° to 90°; volutions four to four and a half; each flattened, or sometimes slightly concave above, with an outward slope from the suture to a prominent angle that passes around the middle of the body-turn, and below the middle of those of the spire, to which it imparts a somewhat turreted appearance; suture moderately distinct, but not channeled; lower side of body-volution sloping rapidly inward from the mesial angle, a little below which there usually revolves an obscure, undefined ridge; aperture rhombic subquadrate; surface nearly smooth, but sometimes showing under a magnifier very obscure lines of growth, that curve very strongly backward as they approach the angle around the middle of the body-volution, both above and below —thus indicating the presence of a deep sinus in the lip, that widens rapidly forward, though there is no defined revolving band at the angle.

Length or height, 0.55 inch; breadth about 0.50 inch.

This shell possesses some of the characters of both *Pleurotomaria* and *Scalites.* In general appearance it is most like some types of the former; but it seems to be entirely without the revolving band seen on the species of that genus. Its lines of growth, however, have the very strong, oblique backward curve seen in those of *Scalites* (in which group there is no revolving band), thus showing that its lip, when entire, must have had a deep notch at the termination of the angle of the body-whorl. This notch, however, does not appear to terminate in a deep sharply cut slit, as we most generally see in *Pleurotomaria*; but it seems to have terminated at, and widened rapidly forward from, the angle of the volutions. Specifically, this shell is related to *Pleurotomaria selecta* of Billings, from which it differs in having its striæ of growth nearly obsolete, and in wanting the revolving angle just below the suture, seen in that species.

*Locality and position.*—Cincinnati group, at Cincinnati, Ohio. Mr. A. S. Miller's collection.

### Genus DICRANISCUS[*] M.

Shell inequivalve, hinge line straight, rather long, hinge provided with teeth and sockets. Ventral valve with a well developed cardinal area, divided in the middle by a triangular fissure sometimes partly closed above by a very small false-deltidium; beak imperforate; interior without dental or other laminæ, or processes of any kind; muscular impressions unknown. Ventral valve without a well developed area, but provided with an extremely prominent, bifurcating cardinal process, and on each side of the base of this with a prominent brachial process directed obliquely outward; while just outside of each of the latter there is a socket for the reception of the teeth of the other valve; muscular impression small, and placed in the bottom of the valve some distance in advance of the hinge. Shell substance very thick about the hinge, and showing an imperfectly fibrous structure when broken, the fibers being arranged at right angle to the surface of the valves.

I have had specimens of this remarkable shell under consideration for nearly a year; but have been waiting for others to be found that would show its characters more clearly, all of those seen being fragmentary. Somewhat better specimens have recently been found by Prof. Orton, but none nearly entire have yet been discovered. Those now at hand, however,

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[*] Dimin. of *δίκρανος*, a two pronged fork; in allusion to the long bifid cardinal process.

give the means of making out its generic and specific characters with some degree of detail, though we yet want specimens showing the exact form of the entire shell, and the muscular impressions of the ventral valve.

With regard to its family affinities, it would be unsafe to venture an opinion without better material for study. I know of no other brachiopod with so long a cardinal process excepting *Stringocephalus ;* but it differs entirely from that genus in wanting the prominent mesial internal septum in the ventral valve, as well as in having the beak of this valve very short—even less produced than that of the other valve, and the foramen very small with no perforation through the minute false-deltidium sometimes covering the upper part of the foramen.

It is probably more nearly related to *Stricklandinia* of Billings; but on comparing some of the specimens sent to him by Prof. Orton, Mr. Billings writes that he thinks it entirely distinct from his genus, which he says has no such cardinal process. The ventral valve of our shell also differs in having no trace of the triangular internal chamber, seen under the beak of *Stricklandinia.*

### DICRANISCUS ORTONI M.

Shell truncato-suboval, or suborbicular, with front narrowly rounded; hinge-line less than the greatest breadth. Ventral valve less convex than the dorsal, and provided with a moderately deep mesial sinus not extending to the beak; cardinal area rather low, well defined, and standing nearly at right angles to the plane of the valves, but slightly arched backward; beak very small and scarcely projecting beyond the margin of the area, with its point sometimes slightly arched. Dorsal valve moderately convex, and rising into an undefined mesial fold or prominence on the anterior slope; beak rather prominent, or at least more so than that of the other valve, and more incurved; area wanting or very narrow and obscure; cardinal process very long, a little curved, rounded, and rather slender below, and divided above for about half way down, the divisions being moderately diverging, slender, and somewhat furrowed on their posterior sides above. Surface smooth.

As I have seen no entire specimens, I cannot give measurements, though the shell probably attained a length of one inch and a quarter, with a breadth of about one inch, and a convexity of perhaps three quarters of an inch.

It was some time before I could believe that the separate ventral valves of this shell really belonged to the same species as the dorsal; because I could not understand how so very prominent a cardinal process as that possessed by the latter could possibly be received into a ventral valve with no deeper concavity than is presented by these specimens. It was, therefore, not until Prof. Orton found a broken specimen with portions of the two valves united, and showing this process in place, that I was aware that the dorsal valve has its beak so incurved as to give this cardinal process an oblique forward direction within the other valve. Even then, however, it seems to touch the bottom of the ventral valve.

The specific name is given in honor of Prof. Edward Orton, who discovered the only specimens of the species known.

*Locality and position.*—Summit of the Clinton group, near Dayton, Ohio.

Note.—Among the specimens sent to me from Cincinnati, Ohio, by Mr. A. S. Miller of that city, there are two examples of an *Orthis*, agreeing in form and general appearance with *O. plicatella*, but differing in being considerably larger, and in having a *decidedly* lower ventral area, with the beak of the same valve more incurved than in any authentic specimens of that shell I have ever seen. It also presents a curious triple arrangement of the costæ, caused by each of them giving off a smaller one on each side at about half the distance between the beak and free margins, the main rib always continuing larger and much more prominent than the others to the margin; while the spaces between each bundle thus formed are proportionally wider and deeper than we see in *O. plicatella* as usually found. These characters give this shell a peculiar appearance that leads me to think it will probably be found to belong to an undescribed species. As I know nothing of its internal characters, however, and the species of this type of *Orthis* are known to be quite variable, I feel some doubts about the propriety of describing it as a new species. It seems to me, however, to differ from the *O. plicatella* and *O. fissicosta* in more important characters than they differ from each other. Should other collections show this form to be a distinct species, I would propose for it the name *O. triplicatella*.

The larger of the two specimens seen measures 0·70 inch in length, 1·40 inch in breadth, and 0·40 inch in convexity.

### Art. XXXVII.—*Discovery of a New Planet*; by C. H. F. Peters. From a letter, dated Litchfield Observatory, Hamilton College, August 24.

Last night another new planet came into my view. It is rather bright, and about the 10th magnitude; the following positions were determined:

| 1872. | H. C. m. t. | α (124) | δ (124) |
|---|---|---|---|
| | h m s | h m s | ° ′ ″ |
| Aug. 23. | 12 28 6 | 22 21 23·59 | −7 18 28·8 (10 comp.) |
| " | 13 22 49 | 22 21 21·64 | −7 18 42·3 ( 5 comp.) |

Whence its daily motion is inferred to be about $51^s$ in right ascension, and 6′ in declination toward the south.

## ART. XXXVIII.—*Address before the American Association at its recent meeting in Dubuque, Iowa;* by Prof. ASA GRAY.

THE session being now happily inaugurated, your presiding officer of the last year has only one duty to perform before he surrenders the chair to his successor. If allowed to borrow a simile from the language of my own profession, I might liken the president of this association to a biennial plant. He flourishes for the year in which he comes into existence, and performs his appropriate functions as presiding officer. When the second year comes round he is expected to blossom out in an address and disappear. Each president, as he retires, is naturally expected to contribute something from his own investigations, or his own line of study; usually to discuss some particular scientific topic.

Now, although I have cultivated the field of North American botany with some assiduity for more than forty years, have reviewed our vegetable hosts and assigned to no small number of them their names and their place in the ranks, yet, so far as our own wide country is concerned, I have been to a great extent a closet botanist. Until this summer I had not seen the Mississippi, nor set foot upon a prairie.

To gratify a natural interest, and to gain some title for addressing a body of practical naturalists and explorers, I have made a pilgrimage across the continent. I have sought and viewed in their native haunts many a plant and flower which, for me, had long bloomed unseen, or only in the *hortus siccus.* I have been able to see for myself what species and what forms constitute the main features of the vegetation of each successive region, and record—as the vegetation unerringly does—the permanent characteristics of its climate.

Passing on from the eastern district, marked by its equably distributed rain-fall, and therefore naturally forest-clad, I have seen the trees diminish in number, give place to wide prairies, restrict their growth to the borders of streams, and then disappear from the boundless drier plains; have seen grassy plains change into brown and sere desert—desert in the common sense, but hardly anywhere botanically so: have seen a fair growth of coniferous trees adorning the more favored slopes of a mountain range, high enough to compel summer showers; have traversed that broad and bare elevated region shut off on both sides by high mountains from the moisture supplied by either ocean, and longitudinally intersected by sierras which seemingly remain as naked as they were born; and have reached at length the westward slopes of the high mountain barrier, which, refreshed by the Pacific, bears the noble forests of the

Sierra Nevada and the Coast Range, and among them trees which are the wonder of the world. As I stood in their shade, in the groves of Mariposa and Calaveras, and again under the canopy of the commoner redwood, raised on columns of such majestic height and ample girth, it occurred to me that I could not do better than to share with you, upon this occasion, some of the thoughts which possessed my mind. In their development they may perhaps lead us up to questions of considerable scientific interest.

I shall not detain you with any remarks (which would now be trite) upon the size or longevity of these far-famed Sequoia trees, or of the sugar pines, incense cedar, and firs associated with them, of which even the prodigious bulk of the dominating Sequoia does not sensibly diminish the grandeur. Although no account and no photographic representation of either species of the far-famed Sequoia trees gives any adequate impression of their singular majesty—still less of their beauty—yet my interest in them did not culminate merely nor mainly in considerations of their size and age. Other trees in other parts of the world may claim to be older. Certain Australian gum trees (*Eucalypti*) are said to be taller. Some, we are told, rise so high that they might even cast a flicker of shadow upon the summit of the pryamid of Cheops. Yet the oldest of them doubtless grew from seed which was shed long after the names of the pyramid builders had been forgotten. So far as we can judge from the actual counting of the layers of several trees, no Sequoia now alive can sensibly antedate the Christian era.

Nor was I much impressed with an attraction of man's adding. That the more remarkable of these trees should bear distinguishing appellations seems proper enough. But the tablets of personal names which are affixed to many of them in the most visited groves—as if the memory of more or less notable people of our day might be made more enduring by the juxtaposition, do suggest some incongruity. When we consider that a hand's breadth at the circumference of any one of the venerable trunks so placarded has recorded in annual lines the lifetime of the individual thus associated with it, one may question whether the next hand's-breadth may not measure the fame of some of the names thus ticketed for adventitious immortality. Whether it be the man or the tree that is honored in the connection, probably either would live as long in fact and in memory without it.

One notable thing about these Sequoia trees is their isolation. Most of the trees associated with them are of peculiar species, and some of them are nearly as local. Yet every pine, fir and cypress in California is in some sort familiar, because it has near relations in other parts of the world. But the redwoods have none. The redwood—including in that name the two species of "big trees"—belongs to the general cypress family, but is *sui generis.* Thus isolated systematically, and extremely isolated geographically, and so wonderful in size and port, they more than other trees suggest questions.

Were they created, thus local and lonely, denizens of California only; one in limited numbers in a few choice spots on the Sierra Nevada, the other along the Coast Range from the Bay of Monterey to the frontiers of Oregon? Are they veritable Melchisedecs, without pedigree or early relationship, and possibly fated to be without descent?

Or are they now coming upon the stage (or rather were they coming but for man's interference) to play a part in the future?

Or are they remnants, sole and scanty survivors of a race that has played a grander part in the past, but is now verging to extinction? Have they had a career, and can that career be ascertained or surmised, so that we may at least guess whence they came, and how and when?

Time was, and not long ago, when such questions as these were regarded as useless and vain, when students of natural history, unmindful of what the name denotes, were content with a knowledge of things as they now are, but gave little heed as to how they came to be so. Now, such questions are held to be legitimate, and perhaps not wholly unanswerable. It cannot now be said that these trees inhabit their present restricted areas simply because they are there placed in the climate and soil of all the world most congenial to them. These must indeed be congenial or they would not survive. But when we see how Australian Eucalyptus trees thrive upon the California coast, and how these very redwoods flourish upon another continent; how the so-called wild oat (*Avena sterilis* of the Old World) has taken full possession of California; how that cattle and horses, introduced by the Spaniard, have spread as widely and made themselves as much at home on the plains of the La Plata as on those of Tartary, and that the cardoon thistle seeds, and others they brought with them, have multiplied there into numbers probably much exceeding those extant in their native lands; indeed, when we contemplate our own race, and our own particular stock, taking such recent but dominating possession of this New World; when we consider how the indigenous flora of islands generally succumbs to the foreigners which come in the train of man; and that most weeds (i. e., the prepotent plants in open soil) of all temperate climates are not "to the manor born," but are self-invited intruders, we must needs abandon the notion of any primordial and absolute adaptation of plants and animals to their habitats which may stand in lieu of explanation, and so preclude our inquiring any further. The harmony of Nature and its admirable perfection need not be regarded as inflexible and changeless. Nor need Nature be likened to a statue, or a cast in rigid bronze, but rather to an organism, with play and adaptability of parts, and life and even soul informing the whole. Under the former view, Nature would be "the faultless monster which the world ne'er saw," but inscrutable as the Sphinx, whom it were vain, or worse, to question of the whence and whither. Under the other, the perfection of nature, if relative, is multifarious and ever renewed; and much that is enigmatical now may find explanation in some record of the past.

That the two species of redwood we are contemplating originated as they are and where they are, and for the part they are now playing, is, to say the least, not a scientific supposition, nor in any sense a probable one. Nor is it more likely that they are destined to play a conspicuous part in the future, or that they would have done so, even if the Indian's fires and the white man's axe had spared them. The redwood of the coast (*Sequoia sempervirens*) has the stronger hold upon existence, forming as it did large forests throughout a narrow belt about three hundred miles in length, and being so tenacious of life that every large stump sprouts into a copse. But it does not pass the Bay of Monterey, nor cross the line of Oregon, although so grandly developed not far below it. The more remarkable *Sequoia gigantea* of the Sierra exists in numbers so limited that the separate groves may be reckoned upon the fingers, and the trees of most of them have been counted, except near their southern limit, where they are said to be more copious. A species limited in individuals holds its existence by a precarious tenure; and this has a foothold only in a few sheltered spots, of a happy mean in temperature and locally favored with moisture in summer. Even there, for some reason or other, the pines with which they are associated (*Pinus Lambertiana* and *P. ponderosa*), the firs (*Abies grandis* and *A. amabilis*), and even the incense-cedar (*Libocedrus decurrens*) possess a great advantage, and, though they strive in vain to emulate their size, wholly overpower the Sequoias in number. "To him that hath shall be given." The force of numbers eventually wins. At least in the commonly visited groves *Sequoia gigantea* is invested in its last stronghold, can neither advance into more exposed positions above, nor fall back into drier and barer ground below, nor hold its own in the long run where it is, under present conditions; and a little further drying of the climate, which must once have been much moister than now, would precipitate its doom. Whatever the individual longevity, certain if not speedy is the decline of a race in which a high death-rate afflicts the young. Seedlings of the big trees occur not rarely, indeed, but in meagre proportion to those of associated trees; and small indeed is the chance that any of these will attain to "the days of the years of their fathers." "Few and evil" are the days of all the forest likely to be, while man, both barbarian and civilized, torments them with fires, fatal at once to seedlings, and at length to the aged also. The forests of California, proud as the State may be of them, are already too scanty and insufficient for her uses. Two lines, such as may be drawn with one sweep of a small brush over the map, would cover them all. The coast redwood,—the most important tree in California,—although a million times more numerous than its relative of the Sierra, is too good to live long. Such is its value for lumber and its accessibility, that, judging the future by the past, it is not likely, in its primeval growth, to outlast its rarer fellow-species.

Happily man preserves and disseminates as well as destroys. The species will probably be indefinitely preserved to science, and for ornamental and other uses, in its own and other lands; and the more remarkable individuals of the present day are likely to be sedulously cared for, all the more so as they become scarcer.

Our third question remains to be answered: Have these famous Sequoias played in former times and upon a larger stage a more imposing part, of which the present is but the epilogue? We cannot gaze high up the huge and venerable trunks, which one crosses the continent to behold, without wishing that these patriarchs of the grove were able, like the long-lived antediluvians of Scripture, to hand down to us, through a few generations, the traditions of centuries, and so tell us somewhat of the history of their race. Fifteen hundred annual layers have been counted, or satisfactorily made out, upon one or two fallen trunks. It is probable that close to the heart of some of the living trees may be found the circle that records the year of our Saviour's nativity. A few generations of such trees might carry the history a long way back. But the ground they stand upon, and the marks of very recent geological change and vicissitude in the region around, testify that not very many such generations can have flourished just there, at least in an unbroken series. When their site was covered by glaciers, these Sequoias must have occupied other stations, if, as there is reason to believe, they then existed in the land.

I have said that the redwoods have no near relatives in the country of their abode, and none of their genus anywhere else. Perhaps something may be learned of their genealogy by inquiring of such relatives as they have. There are only two of any particular nearness of kin; and they are far away. One is the bald cypress, our southern cypress, Taxodium, inhabiting the swamps of the Atlantic coast from Maryland to Texas, thence extending into Mexico. It is well known as one of the largest trees of our Atlantic forest-district, and, although it never (except perhaps in Mexico, and in rare instances) attains the portliness of its western relatives, yet it may equal them in longevity. The other relative is Glyptostrobus, a sort of modified Taxodium, being about as much like our bald cypress as one species of redwood is like the other.

Now species of the same type, especially when few, and the type peculiar, are, in a general way, associated geographically, *i. e.*, inhabit the same country, or (in a large sense) the same region. Where it is not so, where near relatives are separated, there is usually something to be explained. Here is an instance. These four trees, sole representatives of their tribe, dwell almost in three separate quarters of the old world; the two redwoods in California, the bald cypress in Atlantic North America, its near relative, Glyptostrobus, in China.

It was not always so. In the Tertiary period, the geological botanists assure us, our own very Taxodium, or bald cypress, and a Glyptostrobus, exceedingly like the present Chinese tree, and more than one Sequoia, co-existed in a fourth quarter of the globe, viz., in Europe! This brings up the question: Is it possible to bridge over these four wide intervals of space and the much vaster interval of time, so as to bring these extraordinarily separated relatives into connection. The evidence which may be brought to bear upon this question is various and widely scattered. I bespeak your patience while I endeavor to bring together, in an abstract, the most important points of it.

Some interesting facts may come out by comparing generally the botany of the three remote regions, each of which is the sole home of one of these three genera, *i. e.*, Sequoia in California, Taxodium in the Atlantic United States, and Glyptostrobus in China, which compose the whole of the peculiar tribe under consideration.

Note then, first, that there is another set of three or four peculiar trees, in this case of the yew family, which has just the same peculiar distribution, and which therefore may have the same explanation, whatever that explanation be. The genus Torreya, which commemorates our botanical Nestor and a former president of this association, Dr. Torrey, was founded upon a tree rather lately discovered (that is, about thirty-five years ago) in northern Florida. It is a noble, yew-like tree, and very local, being known only for a few miles along the shores of a single river. It seems as if it had somehow been crowded down out of the Alleghanies into its present limited southern quarters; for in cultivation it evinces a northern hardiness. Now another species of Torreya is a characteristic tree of Japan; and the same, or one very like it indeed, inhabits the Himalayas, —belongs, therefore, to the Eastern Asiatic temperate region, of which China is a part, and Japan, as we shall see, the portion most interesting to us. There is only one more species of Torreya, and that is a companion of the redwoods in California. It is the tree locally known under the name of the California nutmeg. In this case the three are near brethren, species of the same genus, known nowhere else than in these three habitats.

Moreover, the Torreya of Florida has growing with it a yew tree; and the trees of that grove are the only yew trees of Eastern America; for the yew of our northern woods is a decumbent shrub. The only other yew trees in America grow with the redwoods and the other Torreya in California, and more plentifully farther north, in Oregon. A yew tree equally accompanies the Torreya of Japan and the Himalayas, and this is apparently the same as the common yew of Europe.

So we have three groups of trees of the great coniferous order which agree in this peculiar geographical distribution; the redwoods and their relatives, which differ widely enough to be termed a different genus in each region; the Torreyas, more nearly akin, merely a different species in each region; the yews, perhaps all of the same species, perhaps not quite that, for opinions differ and can hardly be brought to any decisive test. The yews of the Old World, from Japan to Western Europe, are considered the same; the very local one in Florida is slightly different; that of California and Oregon differs a very little more; but all of them are within the limits of variation of many a species. However that may be, it appears to me that these several instances all raise the same question, only with a different degree of emphasis, and, if to be explained at all, will have the same kind of explanation. But the value of the explanation will be in proportion to the number of facts it will explain.

Continuing the comparison between the three regions with which we are concerned, we note that each has its own species of pines, firs, larches, etc., and of a few deciduous trees, such as oaks and maples; all of which have no peculiar significance for the present purpose, because they are of genera which are common all round the northern hemisphere. Leaving these out of view, the noticeable point is that the vegetation of California is most strikingly unlike that of the Atlantic United States. They possess some plants, and some peculiarly American plants, in common,—enough to show, as I imagine, that the difficulty was not in the getting from the one district to the other, or into both from a common source, but in abiding there. The primordially unbroken forest of Atlantic North America, nourished by rainfall distributed throughout the year, is widely separated from the western region of sparse and discontinuous tree-belts of the same latitude on the western side of the continent, where summer rain is wanting or nearly so, by immense treeless plains and plateaux of more or less aridity, traversed by longitudinal mountain ranges of a similar character. Their nearest approach is at the north, in the latitude of Lake Superior, where, on a more rainy line, trees of the Atlantic forest and that of Oregon may be said to interchange. The change of species and of the aspect of vegetation in crossing, say on the forty-seventh parallel, is slight in comparison with that on the thirty-seventh or near it. Confining our attention to the lower latitude, and under the exceptions already specially noted, we may say that almost every characteristic form in the vegetation of the Atlantic States is wanting in California, and the characteristic plants and trees cf California are wanting here.

California has no Magnolia nor tulip trees, nor star-anise tree; no so-called Papaw (Asimina); no barberry of the common single-leaved sort; no Podophyllum or other of the peculiar associated genera; no Nelumbo nor white water-lily; no prickly ash nor sumach; no loblolly-bay nor Stuartia; no basswood or linden trees: neither locust, honey locust, coffee trees (Gymnocladus), nor yellow wood (Cladrastis); nothing answering to Hydrangea or witch-hazel, to gum trees (Nyssa and Liquidambar), Viburnum or Diervilla; it has few asters and golden rods; no lobelias; no huckleberries and hardly any blueberries; no Epigæa, charm of our earliest eastern spring, tempering an icy April wind with a delicious wild fragrance; no Kalmia nor Clethra, nor holly, nor persimmon; no Catalpa tree, nor trumpet-creeper (Tecoma); nothing answering to sassafras, nor to benzoin tree, nor to hickory; neither mulberry nor elm; no beech, true chestnut, hornbeam, nor ironwood, nor a proper birch tree; and the enumeration might be continued very much further by naming herbaceous plants and others familiar only to botanists.

In their place California is filled with plants of other types, trees, shrubs, and herbs, of which I will only remark that they are, with one or two exceptions, as different from the plants of the eastern Asiatic region with which we are concerned (Japan, China and Mandchuria) as they are from those of Atlantic North America. Their near relatives, when they have any in other lands, are mostly southward, on the Mexican plateau, or many as far south as Chili. The same may be said of the plants of the intervening great plains, except that northward and in the subsaline vegetation there are some close alliances with the flora of the steppes of Siberia. And along the crests of high mountain ranges the arctic-alpine flora has sent southward more or less numerous representatives through the whole length of the country.

If we now compare, as to their flora generally, the Atlantic Uhited States with Japan, Mandchuria, and Northern China, *i. e.*, Eastern North America with Eastern North Asia—half the earth's circumference apart—we find an astonishing similarity. The larger part of the genera of our own region which I have enumerated as wanting in California are present in Japan or Mandchuria, along with many other peculiar plants divided between the two. There are plants enough of the one region which have no representatives in the other. There are types which appear to have reached the Atlantic States from the South, and there is a larger infusion of subtropical Asiatic types into temperate China and Japan; among these there is no relationship between the two countries to speak of. There are also, as I have already said, no small number of genera and some species which, being common all round or partially round the northern temperate zone, have no special significance because of their occurrence in these two antipodal floras, although they have testimony to bear upon the general question of geographical distribution. The point to be remarked is that many or even most of the genera and species which are peculiar to North America as compared with Europe, and largely peculiar to Atlantic North America as compared with the Californian region, are also represented in Japan and Mandchuria, either by identical or by closely similar forms. The same rule holds on a more northward line, although not so strikingly. If we compare the plants, say of New England and Pennsylvania (lat. 45°—47°) with those of Oregon, and then with those of Northeastern Asia, we shall find many of our own curiously repeated in the latter, while only a small number of them can be traced along the route even so far as the western slope of the Rocky Mountains. And these repetitions of Eastern American types in Japan and neighboring districts are in all degrees of likeness. Sometimes the one is undistinguishable from the other; sometimes there is a difference of aspect, but hardly of a tangible character; sometimes the two would be termed marked varieties if they grew naturally in the same forest or in the same region; sometimes they are what the botanist calls representative species, the one answering closely to the other, but with some differences regarded as specific; sometimes the two are merely of the same genus or not quite that, but of a single or very few species in each country,—when the point which interests us is that this peculiar limited type should occur in two antipodal places and nowhere else.

It would be tedious and except to botanists abstruse to enumerate instances, yet the whole strength of the case depends upon the number of such instances. I propose, therefore, if the Association does me the honor to print this discourse, to append in a note a list of the more remarkable ones. But I would here mention two or three cases as specimens.

Our *Rhus Toxicodendron* or poison ivy, is very exactly repeated in Japan, but is found in no other part of the world, although a species much like it abounds in California. Our other poisonous Rhus (*R. venenata*), commonly called poison dogwood, is in no way represented in Western America, but has so close an analogue in Japan that the two were taken for the same by Thunberg and Linnæus, who called them *R. vernix.*

Our northern fox-grape, *Vitis Labrusca*, is wholly confined to the Atlantic States, except that it reappears in Japan and that region.

The original *Wistaria* is a woody leguminous climber, with showy blossoms, native to the Middle Atlantic States. The other species which we so much prize in cultivation, *W. Sinensis*, is from China, as its name denotes, or perhaps only from Japan, where it is certainly indigenous.

Our yellow wood (*Cladrastis*) inhabits a very limited district on the western slope of the Alleghanies. Its only and very near relative (*Maackia*) is in Mandchuria.

The *Hydrangeas* have some species in our Alleghany region. All the rest belong to the Chino-Japanese region and its continuation westward. The same may be said of *Philadelphus*, except that there are one or two mostly very similar in California and Oregon.

Our blue cohosh (*Caulophyllum*) is confined to the woods of the Atlantic States, but has lately been discovered in Japan. A peculiar relative of it, *Diphylleia*, confined to the higher Alleghanies, is also repeated in Japan, with a slight difference, so that it may barely be distinguished as another species. Another relative is our twin leaf, *Jeffersonia*, of the Alleghany region alone. A second species has lately turned up in Mandchuria. A relative of this is *Podophyllum*, our mandrake, a common inhabitant of the Atlantic United States, but found nowhere else. There is one other species of it, and that is in the Himalayas. Here are four most peculiar genera of one family, each of a single species in the Atlantic United States, which are duplicated on the other side of the world, either in identical or almost identical species, or in an analogous species, while nothing else of the kind is known in any other part of the world.

I ought not to omit ginseng, the root so prized by the Chinese, which they obtained from their northern provinces and Mandchuria, and which is now known to inhabit Corea and Northern Japan. The Jesuit Fathers identified the plant in Canada and the Atlantic States, brought over the Chinese name by which we know it, and established the trade in it which was for many years most profitable. The exportation of ginseng to China has probably not yet entirely ceased. Whether the Asiatic and the Atlantic American ginsengs are exactly of the same species or not is somewhat uncertain, but they are hardly if at all distinguishable.

There is a shrub—*Elliottia*—which is so rare and local that it is known only at two stations on the Savannah river in Georgia. It is of peculiar structure, and was without near relative until one was lately discovered in Japan (in Tripetaleia) so like it as hardly to be distinguishable, except by having the parts of the blossom in threes instead of fours, a difference which is not uncommon in the same genus or even in the same species.

Suppose *Elliottia* had happened to be collected only once, a good while ago, and all knowledge of the limited and obscure locality was lost; and meanwhile the Japanese form came to be known. Such a case would be parallel with an actual one. A specimen of a peculiar plant, *Shortia galacifolia*, was detected in the herbarium of the elder Michaux, who collected it (as his autograph ticket shows) somewhere in the high Alleghany mountains more than eighty years ago. No one has seen the living plant since, or knows where to find it, if haply it still flourishes in some secluded spot. At length it is found in Japan; and I had the satisfaction of making the identification.* One other relative is also known in Japan; and another still unpublished has just been detected in Thibet.

Whether the Japanese and the Alleghanian plants are exactly the same or not, it needs complete specimens of the two to settle. So far as we know they are just alike. And even if some difference were discerned between them, it would not appreciably alter the question as to how such a result came to pass. Each and every one of the analogous cases I have been detailing—and very many more could be mentioned—raises the same question and would be satisfied with the same answer.

These singular relations attracted my curiosity early in the course of my botanical studies, when comparatively few of them were known, and my serious attention in later years, when I had numerous and new Japanese plants to study in the collections made (by Messrs. Williams and Morrow) during Commodore Perry's visit in 1853, and, especially, by Mr. Charles Wright, in Commodore Rodgers' expedition in 1855. I then discussed

* This Journal, 1867, p. 402. Proc. Am. Acad., viii, p. 244.

this subject somewhat fully, and tabulated the facts within my reach.*

This was before Heer had developed the rich fossil botany of the arctic zone, before the immense antiquity of existing species of plants was recognised, and before the publication of Darwin's now famous volume on the Origin of Species had introduced and familiarized the scientific world with those now current ideas respecting the history and vicissitudes of species, with which I attempted to deal in a tentative and feeble way.

My speculation was based upon the former glaciation of the northern temperate zone, and the inference of a warmer period preceding (and, perhaps, following). I considered that our own present vegetation, or its proximate ancestry, must have occupied the arctic and sub-arctic regions in pliocene times, and that it had been gradually pushed southward as the temperature lowered and the glaciation advanced even beyond its present habitation; that plants of the same stock and kindred, probably ranging round the arctic zone as the present arctic species do, made their forced migration southward upon widely different longitudes, and receded more or less as the climate grew warmer; that the general difference of climate which marks the eastern and the western sides of the continents—the one extreme, the other mean—was doubtless even then established, so that the same species and the same sorts of species would be likely to secure and retain foothold in the similar climates of Japan and the Atlantic United States, but not in intermediate regions of different distribution of heat and moisture; so that different species of the same genus, as in *Torreya*, or different genera of the same group, as Redwood, *Taxodium*, and *Glyptostrobus*, or different associations of forest trees, might establish themselves each in the region best suited to their particular requirements, while they would fail to do so in any other. These views implied that the sources of our actual vegetation, and the explanation of these peculiarities, were to be sought in and presupposed an ancestry in pliocene or still earlier times occupying the high northern regions. And it was thought that the occurrence of peculiarly North American genera in Europe in the tertiary period (such as *Taxodium*, *Carya*, *Liquidambar*, *Sassafras*, *Negundo*, &c.), might be best explained on the assumption of early interchange and diffusion through North Asia, rather than by that of the fabled Atlantis.

The hypothesis supposed a gradual modification of species in different directions under altering conditions, at least to the extent of producing varieties, sub-species, and representative species, as they may be variously regarded; likewise the single

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* Mem. Amer. Acad., vol. vi.

and local origination of each type, which is now almost universally taken for granted.

The remarkable facts in regard to the Eastern American and Asiatic floras, which these speculations were to explain, have since increased in number, more especially through the admirable collections of Dr. Maximowictz in Japan and adjacent countries, and the critical comparisons he has made and is still engaged upon.

I am bound to state that in a recent general work * by a distinguished botanist, Professor Grisebach of Gottingen, these facts have been emptied of all special significance, and the relations between the Japanese and the Atlantic United States floras declared to be no more intimate than might be expected from the situation, climate, and present opportunity of interchange. This extraordinary conclusion is reached by regarding as distinct species all the plants common to both countries between which any differences have been discerned, although such differences would probably count for little if the two inhabited the same country, thus transferring many of my list of identical to that of representative species, and then by simply eliminating from consideration the whole array of representative species, i. e., all cases in which the Japanese and the American plant are not exactly alike. As if, by pronouncing the cabalistic word *species*, the question were settled, or rather the greater part of it remanded out of the domain of science; as if, while complete identity of forms implies community of region, anything short of it carries no presumption of the kind; so leaving all these singular duplicates to be wondered at, indeed, but wholly beyond the reach of inquiry.

Now the only known cause of such likeness is inheritance; and as all transmission of likeness is with some difference in individuals, and as changed conditions have resulted, as is well known, in very considerable differences, it seems to me that if the high antiquity of our actual vegetation could be rendered probable, not to say certain, and the former habitation of any of our species, or of very near relatives of them in high northern regions could be ascertained, my whole case would be made out. The needful facts, of which I was ignorant when my essay was published, have now been for some years made known, thanks mainly to the researches of Heer upon ample collections of arctic fossil plants. These are confirmed and extended through new investigations by Heer and Lesquereux, the results of which have been indicated to me by the latter.

The *Taxodium*, which everywhere abounds in the miocene formations in Europe, has been specifically identified, first by Goeppert, then by Heer, with our common cypress of the

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* Die Vegetation der Erde nach ihrer klimatischen Anordnung, 1871.

Southern States. It has been found fossil in Spitzbergen, Greenland and Alaska, in the latter country along with the remains of another form, distinguishable, but very like the common species; and this has been identified by Lesquereux in the miocene of the Rocky Mountains. So there is one species of tree which has come down essentially unchanged from the tertiary period, which for a long while inhabited both Europe and North America, and also at some part of the period the region which geographically connects the two (once doubtless much more closely than now), but has survived only in the Atlantic United States and Mexico.

The same *Sequoia* which abound in the same miocene formations in Northern Europe has been abundantly found in those of Iceland, Spitzbergen, Greenland, Mackenzie river, and Alaska. It is named *S. Langsdorfii*, but is pronounced to be very much like *S. sempervirens*, our living redwood of the Californian coast, and to be the ancient representive of it. Fossil specimens of a similar, if not the same, species have been recently detected in the Rocky Mountains by Hayden, and determined by our eminent paleontological botanist, Lesquereux; and he assures me that he has the common redwood itself from Oregon, in a deposit of tertiary age. Another *Sequoia* (*S. Sternbergii*), discovered in miocene deposits in Greenland, is pronounced to be the representative of *S. gigantea*, the big tree of the Californian sierra. If the *Taxodium* of tertiary time in Europe and throughout the arctic regions is the ancestor of our present bald cypress, which is assumed in regarding them as specifically identical, then I think we may, with our present light, fairly assume that the two redwoods of California are the direct or collateral descendants of the two ancient species which so closely resemble them.

The forests of the arctic zone in tertiary times contained at least three other species of *Sequoia*, as determined by their remains, one of which, from Spitzbergen, also much resembles the common redwood of California. Another, "which appears to have been the commonest coniferous tree on Disco," was common in England and some other parts of Europe. So the Sequoias, now remarkable for their restricted station and numbers, as well as for their extraordinary size, are of an ancient stock; their ancestors and kindred formed a large part of the forests which flourished throughout the polar regions, now desolate and ice-clad, and which extended into low latitudes in Europe. On this continent one species at least had reached to the vicinity of its present habitat before the glaciation of the region. Among the fossil specimens already found in California, but which our trustworthy paleontological botanist has not yet had time to examine, we may expect to find evidence of the early arrival of these two redwoods upon the ground which they now, after much vicissitude, scantily occupy.

Differences of climate, or circumstances of migration, or both, must have determined the survival of *Sequoia* upon the Pacific, and of *Taxodium* upon the Atlantic coast and still the redwoods will not stand in the east, nor could our *Taxodium* find a congenial station in California.

As to the remaining near relative of Sequoia, the Chinese Glyptostrobus, a species of it, and its veritable representative, was contemporaneous with Sequoia and Taxodium, not only in temperate Europe, but throughout the arctic regions from Greenland to Alaska. Very similar would seem to have been the fate of a more familiar gymnospermous tree, the gingko or *Salisburia.* It is now indigenous to Japan only. Its ancestor, as we may fairly call it, since, according to Heer, "it corresponds so entirely with the living species that it can scarcely be separated from it," once inhabited northern Europe and the whole arctic region round to Alaska, and had even a representative further south in our Rocky Mountain district. For some reason, this and *Glyptostrobus* survived only on the shores of Eastern Asia.

*Libocedrus*, on the other hand, appears to have cast in its lot with the Sequoias. Two species, according to Heer, were with them in Spitzbergen. Of the two now living, *L. decurrens*—the incense-cedar—is one of the noblest associates of the present redwoods; the other is far south in the Andes of Chili.

The genealogy of the Torreyas is more obscure; yet it is not unlikely that the yew-like trees, named *Taxites*, which flourished with the Sequoias in the tertiary arctic forests, are the remote ancestors of the three species of Torreya, now severally in Florida, in California, and in Japan.

As to the pines and firs, these were more numerously associated with the ancient Sequoias of the polar forests than with their present representatives, but in different species, apparently more like those of eastern than of western North America. They must have encircled the polar zone then, as they encircle the present temperate zone now.

I must refrain from all enumeration of the angiospermous or ordinary deciduous trees and shrubs, which are now known by their fossil remains to have flourished throughout the polar regions when Greenland better deserved its name, and enjoyed the present climate of New England and New Jersey. Then Greenland and the rest of the north abounded with oaks, representing the several groups of species which now inhabit both our eastern and western forest districts; several poplars, one very like our balsam poplar or balm of gilead tree; more beeches than there are now, a hornbeam, and a hop hornbeam, some birches, a persimmon, and a planer-tree, near representatives of those of the Old World, at least of Asia, as well as of Atlantic North America, but all wanting in California; one *Juglans* like the walnut of the Old World, and another like our black walnut; two or three grape vines, one near our Southern fox grape or muscadine, the other near our Northern frost grape; a *Tilia* very like our Basswood of the Atlantic States only; a *Liquidambar;* a *Magnolia*, which recalls our *M. grandiflora;* a *Liriodendron*, sole representative of our tulip tree; and a sassafras, very like the living tree.

Most of these, it will be noticed, have their nearest or their only living representatives in the Atlantic States, and when elsewhere, mainly in Eastern Asia. Several of them, or of species like them, have been detected in our tertiary deposits west of the Mississippi by Newberry and Lesquereux.

Herbaceous plants, as it happens, are rarely preserved in a fossil state, else they would probably supply additional testimony to the antiquity of our existing vegetation, its wide diffusion over the northern and now frigid zone, and its enforced migrations under changes of climate.

Concluding, then, as we must, that our existing vegetation, as a whole, is a continuation of that of the tertiary period, may we suppose that it absolutely originated then? Evidently not. The preceding Cretaceous period has furnished to Carruthers in Europe a fossil fruit like that of the *Sequoia gigantea* of the famous groves, associated with pines of the same character as those that accompany the present tree; has furnished to Heer, from Greenland, two more *Sequoias*, one of them identical with a tertiary species, and one nearly allied to *Sequoia Langsdorfii*, which in turn is a probable ancestor of the common Californian redwood; has furnished to Lesquereux in North America, the remains of another ancient Sequoia, a Glyptostrobus; a Liquidambar which well represents our sweet-gum tree; oaks analogous to living ones; leaves of a plane tree, which are also in the tertiary, and are scarcely distinguishable from our own *Platanus occidentalis;* of a Magnolia and tulip tree; and "of a sassafras undistinguishable from our living species." I need not continue the enumeration. Suffice it to say that the facts will justify the conclusion which Lesquereux—a very scrupulous investigator—has already announced, "That the essential types of our actual flora are marked in the cretaceous period, and have come to us after passing, without notable changes, through the tertiary formations of our continent.

According to these views, as regards plants at least, the adaptation to successive times and changed conditions has been maintained, not by absolute renewals, but by gradual modifications. I, for one, cannot doubt that the present existing species are the lineal successors of those that garnished the earth in the old time before them, and that they were as well adapted to their surroundings then as those which flourish and bloom around us are to their conditions now. Order and exquisite adaptation did not wait for man's coming, nor were they ever stereotyped. Organic Nature,—by which I mean the system and totality of living things, and their adaptation to each other and to the world,—with all its apparent and indeed real stability, should be likened, not to the ocean, which varies only by tidal oscillations from a fixed level to which it is always returning, but rather to a river so vast that we can neither discern its shores nor reach its sources, and whose onward flow is not less actual because too slow to be observed by the ephemeræ which hover over its surface or are borne upon its bosom.

Such ideas as these, though still repugnant to some, and not long since to many, have so possessed the minds of the naturalists of the present day that hardly a discourse can be pronounced or an investigation prosecuted without reference to them. I suppose that the views here taken are little if at all in advance of the average scientific mind of the day. I cannot regard them as less noble than those which they are succeeding.

An able philosophical writer, Miss Frances Power Cobbe, has recently and truthfully said :*

"It is a singular fact that when we can find out how anything is done, our first conclusion seems to be that God did not do it. No matter how wonderful, how beautiful, how intimately complex and delicate has been the machinery which has worked, perhaps for centuries, perhaps for millions of ages, to bring about some beneficent result, if we can but catch a glimpse of the wheels, its divine character disappears."

I agree with the writer that this first conclusion is premature and unworthy ; I will add deplorable. Through what faults or infirmities of dogmatism on the one hand and scepticism on the other it came to be so thought, we need not here consider. Let us hope, and I confidently expect, that it is not to last ; that the religious faith which survived without a shock the notion of the fixity of the earth itself, may equally outlast the notion of the absolute fixity of the species which inhabit it ; that, in the future even more than in the past, faith in an *order* which is the basis of science will not (as it cannot reasonably) be dissevered from faith in an *Ordainer*, which is the basis of religion.

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## ART. XXXIX.—*Preliminary Description of New Tertiary Reptiles;* by O. C. MARSH. PART I.

THE remains described in this paper are from the early Tertiary deposits of the Rocky Mountain region, and were discovered by the Yale College party during their explorations in the summer and autumn of last year. The localities are nearly

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* Darwinism in Morals, in Theological Review, April, 1871.

all in the Eocene beds of the Green River basin, first examined by the Yale party, in 1870, and found to contain so many new and interesting forms of vertebrate life.* In this extinct fauna, Reptiles were particularly abundant, and among them were numerous Lizards, several species of which are here described.

### *Thinosaurus paucidens*, gen. et sp. nov.

This genus includes a number of large carnivorous Lizards, which resemble, in some respects, the Varanidæ, or Monitors. The vertebræ are similar in form to those of *Varanus*, but differ in being joined together, especially in the dorsal region, by a peculiar modification of the zygosphenal articulation. This is essentially a repetition of the anterior zygapophyses by an intermediate and diminutive pair of wedge-shaped processes, which replace the usual zygosphene seen in the vertebræ of Iguanas and Serpents. In the caudal vertebræ, this articulation gradually subsides to a low roof, projecting in front over the neural canal. The zygantral cavities are distinct, but not so deep as in the Iguana. They are separated in the anterior vertebræ by a median ridge, the sides of which fit against the articular faces of the small intermediate zygapophyses. Some of the cervical vertebræ had free articulated hypapophyses, as in the Mosasauroid Reptiles. The bones of the skull are smooth, and were evidently without osseous dermal scutes, which was doubtless the case also with the rest of the body. There was a parietal foramen. The teeth are pleurodont, and, in the specimens at present known, have broad expanded bases. All the species appear to have had a long tail, and were probably good swimmers. This genus may be readily distinguished from *Glyptosaurus* Marsh, by the absence of the ornamented dermal plates, which protected all the species of that group. The genus *Saniva* of Leidy is probably more nearly related, but so far as now known the vertebræ appear to have been united only by the usual articulation.

The present species is based upon the greater part of a skeleton found, by the writer, in place. Portions of several other skeletons were discovered in the same region by other members of the Yale party. In the former specimen, the teeth were not numerous, but appear to have been long and sharp. The bases of the crowns attached to the inner wall of the jaw are much expanded, and their sides furrowed, as in *Heloderma*. The dorsal vertebræ have the articular ball and cup transversely elliptical, and much inclined. The centra have their inferior surface very slightly concave longitudinally, and convex transversely. The articular faces for attachment of the ribs have

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* This Journal, vol. i, 1871, pp. 192, 322, and 447.

a round tubercle on their upper portion. The sacral vertebræ are short, and are quite concave below longitudinally. They have no ridge between the zygantral cavities, as in the anterior vertebræ. The anterior and medial caudals are elongated, but less so than the dorsals. The distal caudal vertebræ are slender, but not materially compressed. The limb bones preserved resemble those of the Iguanas. The remains preserved of this species indicate an animal about four feet in length.

*Measurements.*

| | |
|---|---|
| Space occupied by three lower teeth, | 13· mm. |
| Length of dorsal vertebra from edge of cup to end of ball, | 16· |
| Width of articular cup, | 11· |
| Width of ball, | 10·2 |
| Expanse of anterior zygapophyses, | 19· |
| Expanse of posterior zygapophyses, | 17· |
| Expanse of small intermediate processes, | 4·5 |
| Length of first sacral vertebra, | 13· |
| Width of cup, | 8·6 |
| Length of first caudal, | 13·2 |

The type specimen of this species was found by the writer, last September, at Grizzly Buttes, Wyoming. The geological horizon is Eocene, or possibly Upper Miocene.

### *Thinosaurus leptodus*, sp. nov.

This species, which was somewhat smaller than the one above described, is indicated by the more important parts of two skeletons, and some isolated remains of other individuals. The teeth preserved are of unequal size, with the crowns slender, compressed, and pointed; and curving backward and inward. The grooves at the base are deep, and pass upward over the lower half, at least, of the crown. There is a distinct cutting edge in front, but none behind on the lower part of the teeth observed. The vertebræ are very similar to those of *T. paucidens.* The articular cup is transversely elliptical, and is faintly depressed above for the neural canal. The two sacral vertebræ are *ankylosed.* Both are short, and have a deep groove on the lower surface of the expanded diapophyses. The pelvic arch is very similar to that in the Iguanas, but the ilium is pointed at its upper extremity. The caudal vertebræ have the chevrons situated about one-third the length of the centrum from the end of the articular ball. The tail was long and slender.

*Measurements.*

| | |
|---|---|
| Space occupied by three lower teeth, | 11· mm. |
| Antero-posterior diameter of crown of lower tooth, | 2·5 |
| Transverse diameter, | 1·5 |

| | |
|---|---|
| Length of dorsal vertebra on lower surface, | 13·6$^{\mathrm{mm.}}$ |
| Width of cup, | 9· |
| Expanse of anterior zygapophyses, | 16· |
| Length of two united sacral vertebræ, | 18·6 |
| Length of ilium, | 42·7 |

The skeleton on which this description is mainly based was found in September last, at Grizzly Buttes, Wyoming, by Mr. J. F. Quigley. Another fine specimen was found at the same locality by Mr. G. G. Lobdell, Jr.

### *Thinosaurus crassus*, sp. nov.

A third and still larger species of the same genus is represented in our Wyoming collections by a number of dorsal vertebræ, and a few other less characteristic remains, all parts of one skeleton. The vertebræ preserved differ considerably from those of the preceding species, in being much more massive in proportion to their length, which is about the same as in *T. paucidens.* Those of the dorsal series have the inferior surface of the centrum nearly straight longitudinally, and flat or slightly concave transversely. The unarticular surface of the vertebræ is everywhere irregularly striated. The known remains of this species indicate a reptile about five feet in length.

*Measurements.*

| | |
|---|---|
| Length of dorsal vertebra on lower surface, | 16·$^{\mathrm{mm.}}$ |
| Transverse diameter of articular cup, | 11·2 |
| Transverse diameter of ball, | 11· |
| Vertical diameter, | 5·5 |
| Expanse of anterior zygapophyses, | 22· |
| Expanse of posterior zygapophyses, | 20·2 |

The specimen on which the present species is based was found by the writer, last September, in the Tertiary shale near Henry's Fork, Wyoming.

### *Thinosaurus grandis*, sp. nov.

A gigantic Lizard, the largest yet discovered in the Green River basin, and exceeding in size any living species, is indicated by fragmentary portions of several individuals. These remains agree so nearly with those of the species above described that they may be referred, provisionally at least, to the genus *Thinosaurus.* The vertebræ, so far as known, resemble in their proportions those of *T. crassus*, but the dorsals have the centra convex transversely on the inferior surface. The rudimentary zygosphene, moreover, in the type specimen of the present species, has its sides inflected, forming a shelf beneath the cavities thus enclosed, a peculiarity not seen in the vertebræ of the other species of the genus. The posterior sacral vertebra has a groove on the lower side of its diapophyses. This species was probably not less than seven feet in length, and three or four times the bulk of *Iguana tuberculata.*

*Measurements.*

| | |
|---|---|
| Transverse diameter of cup of dorsal vertebra, | 13· mm. |
| Transverse diameter of same vertebra between articular faces of diapophyses, | 29· |
| Transverse diameter of neural arch between zygapophyses, | 16· |
| Expanse of posterior zygapophyses, | 22· |
| Length of posterior sacral vertebra, | 18· |
| Transverse diameter of ball, | 11·5 |
| Length of first caudal vertebra, | 18·1 |
| Antero-posterior diameter of acetabular cavity, | 21· |

The type specimen of this species was found, in September, 1870, at Grizzly Buttes, Wyoming, by Mr. C. W. Betts, of the Yale party of that year.

### *Thinosaurus agilis*, sp. nov.

The smallest species of the genus yet discovered is represented in our collections by some characteristic parts of one skeleton, in perfect preservation, and by several isolated vertebræ from the same locality. In the former specimen, the lower teeth are very similar to those of *T. leptodus*, the next larger species, but the basal grooves on the inner side do not extend up so far on the crown. The two species may be readily distinguished, also, aside from the great difference in size, by the anterior caudal vertebræ, which in the present specimen have the articular cup much more depressed. In the dorsal vertebræ, the neural spine is quite short. The middle and distal caudals are much elongated. The remains preserved indicate an animal about two feet long.

*Measurements.*

| | |
|---|---|
| Length of dorsal vertebra on lower surface, | 10·3 mm. |
| Width of articular cup, | 5·5 |
| Width of ball, | 5· |
| Expanse of anterior zygapophyses, | 9· |
| Expanse of posterior zygapophyses, | 8·2 |
| Length of first caudal vertebra, | 7·3 |
| Width of articular cup, | 5· |
| Vertical diameter of cup, | 2·8 |
| Tranverse diameter of distal end of femur, | 8· |

The specimens on which this description is based were found, last autumn, near Henry's Fork, by Mr. G. G. Lobdell, jr.

### *Glyptosaurus princeps*, sp. nov.

In addition to the characters given when the genus *Glyptosaurus* was proposed,* the following, derived from a study of

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* This Journal, vol. i, p. 456, June, 1871.

more complete specimens, may be mentioned. The entire body and tail was covered with ornamented osseous plates, most of them united by suture. The rami of the lower jaw were but loosely attached at the symphysis. There were numerous small teeth, "*dents en cardes*," on the pterygoids. The malar arch was complete. The parietals were thick, and there was a parietal foramen. The pelvic arch and the limb bones resemble those in the Iguanas, but the posterior limbs were proportionally smaller. The caudal vertebræ, in some species at least, were divided transversely by a thin unossified septum, so that the centra break there readily, as in many recent lizards. This genus and its allies evidently represent a distinct family, which may be called *Glyptosauridæ.*

The present species exceeds in size any of the genus hitherto discovered. It is represented at present by the more important parts of a skull, with some portions of a skeleton and numerous scutes of one individual, and by a few other fragmentary remains. The lower jaws have their rami curved in front, like those of *Heloderma.* The lower teeth were close together, and had their bases deeply fluted. The frontal bones are very massive, and the longitudinal groove on their lower surface is proportionally narrow. The pterygoid teeth are numerous, and are closely and irregularly crowded together. They are tubercular, and collectively resemble the patern of some of the dermal plates. The cranial scutes preserved have their tubercles more irregular in size and arrangement than in the other species of the genus. The remains indicate a reptile fully six feet in length.

*Measurements.*

| | |
|---|---|
| Space occupied by anterior twelve lower teeth, | 23· mm. |
| Width of frontals at posterior edge of nasal suture, | 15·6 |
| Width at posterior edge of prefrontal suture, | 19· |
| Greatest thickness of frontals on median line, | 5· |
| Width of cotylus of lower jaw, | 12· |
| Longitudinal diameter, | 8· |

The type specimen of this species was found by the writer, in September last, in the Eocene shale at Grizzly Buttes, Wyoming.

### *Oreosaurus vagans*, gen. et sp. nov.

This genus may be distinguished from *Glyptosaurus*, apparently its nearest ally, by the frontal bones, which are proportionally much narrower at their posterior margin, and broader between the orbits. The superior surface of these bones is, moreover, rough and granular, which would indicate that they were not covered with osseous scutes. The body was thus protected, but the dermal plates preserved, even those evidently from the dorsal region, were united together by beveled edges. The teeth were pleurodont. The pterygoid bones supported minute tubercular teeth, resembling those of *Glyptosaurus.*

In the species here described the teeth are rodlike, with small bases, and obtuse striated summits, which are crowned by a low longitudinal ridge. The frontals are thick, and loosely united by suture. Between the orbits, their sides are nearly parallel. Their upper surface is evenly marked by small irregular tubercles. The dermal scutes preserved have a similar pattern, and some of them are carinate. The species was about three feet in length.

*Measurements.*

| | |
|---|---|
| Space occupied by eight teeth near middle of lower jaw, | 11·2 mm. |
| Space occupied by four anterior teeth of upper jaw, | 5· |
| Width of band of small teeth on pterygoid, | 4·6 |
| Width of frontals at posterior margin, | 13· |
| Width between orbits, | 11. |

The known remains of this species were found by the writer, last autumn, at Grizzly Buttes, Wyoming.

*Tinosaurus stenodon*, gen. et sp. nov.

A small carnivorous Lizard is indicated among our Wyoming fossils, by part of a lower jaw, with two teeth, in excellent preservation, and by some other fragmentary specimens. The teeth preserved are from near the middle of the lower jaw. Their crowns are short, much compressed, pointed, and curved backward. They are separated from each other about half the diameter of the crown. The anterior tooth is the larger, and has on its front edge a small cusp. The posterior tooth is tricuspid. On the outside of the jaw, in front of each tooth, is a groove cut by the opposing upper tooth, which was likewise compressed and pointed. The bases of the lower teeth are smooth and swollen. The animal thus represented was probably less than two feet in length.

*Measurements.*

| | |
|---|---|
| Space occupied by three lower teeth, | 4·5 mm. |
| Height of crown of lower tooth above jaw, | 1· |
| Antero-posterior diameter at base, | 1·8 |
| Transverse diameter of jaw below teeth, | 2· |

The remains which can now with certainty be referred to this species are from Henry's Fork, Wyoming, and were found by Mr. J. F. Page, in September last.

## PART II.

### *Glyptosaurus brevidens*, sp. nov.

The present species is well represented by the greater portion of a skeleton in remarkable preservation. The reptile appears to have been covered up, soon after death, in the soft mud of the lake, and thus the bones, and even many of the dermal scutes, were preserved in their natural position. The remains indicate a species about as large as *Glyptosaurus ocellatus* Marsh, and probably a nearly related species. The frontals are proportionally thicker than in *G. sylvestris* Marsh, but are covered with similar scutes. The malar arch was massive. The teeth are rod-like, close together, and unusually short, projecting but slightly beyond the jaw. The summits are obtuse, and marked by irregular striæ. The pterygoid teeth are minute, and arranged in a narrow band. The dermal scutes on the malar region are very thick, and have their tubercles in concentric rows, forming an ocellated pattern. The dorsal plates are large, quadrilateral in form, with the lateral margins united by suture, and the ends imbricate. The exposed parts of these scutes are covered with small tubercles, arranged near the margin in rows. The center is more or less carinate longitudinally. The cervical vertebræ have a keel below, which gradually subsides in the dorsal region. The articular ball is surrounded by a deep groove.

*Measurements.*

| | |
|---|---|
| Width of frontals between orbits,...................... | 19· mm. |
| Space occupied by five posterior upper teeth,........... | 7·5 |
| Width of occipital condyle, ......................... | 8· |
| Depth of lower jaw at cotylus,......................... | 12· |
| Length of centrum of anterior dorsal vertebra,.......... | 11· |
| Width of articular cup,............................. | 6.8 |
| Expanse of anterior zygapophyses,.................... | 15· |
| Length of dorsal scute,............................. | 13·5 |
| Width of same,................................... | 8· |

This specimen was found by the writer, last September, at Grizzly Buttes, Wyoming.

### *Glyptosaurus rugosus*, sp. nov.

This species may be readily distinguished from *Glyptosaurus sylvestris*, and other allied forms, by the osseous scutes on the frontals, which are smaller than in any known species of the genus. These plates are prominently convex, and have their tubercles nearly all of equal size, and without definite arrangement. The prefrontal and postfrontal bones, moreover, approach each other, above the orbit, much more nearly than in the species hitherto described. The remains preserved of this species indicate an animal about three or four feet in length.

*Measurements.*

| | |
|---|---|
| Width of both frontals at posterior margin, | 31. mm. |
| Width between orbits, | 22. |
| Extent of postfrontal suture on frontal, | 13·5 |
| Distance between prefrontal and postfrontal, | 3.2 |
| Thickness of frontals on median line between orbits, | 3·4 |

The only known remains of the present species were found, in September last, at Grizzly Buttes, by Mr. T. G. Peck, of the Yale party.

### *Glyptosaurus sphenodon*, sp. nov.

A smaller species, probably belonging to the genus *Glyptosaurus*, is indicated in our collections by some fragmentary remains, among which the most characteristic specimen is a portion of an upper jaw with several teeth. These are peculiar, and differ essentially from any yet discovered in the Green River basin. The crowns are long, cylindrical, separated slightly from each other, and directed obliquely backward. The summits are compressed, and very sharp. The bases of the teeth are rugose, and the crowns smooth. This species was about two or three feet in length.

*Measurements.*

| | |
|---|---|
| Space occupied by four upper teeth, | 6· mm. |
| Height of upper tooth on inner side, | 4·5 |
| Extent beyond jaw, | 2· |
| Antero-posterior diameter of crown, | 1.2 |

The specimens at present representing this species were discovered last autumn, near Henry's Fork, Wyoming, by Mr. T. G. Peck.

### *Glyptosaurus ocellatus* Marsh.

This Journal, vol. i, p. 458, June, 1871.

Among our Eocene fossils are several specimens which evidently belong to *Glyptosaurus ocellatus*, and fortunately afford additional specific characters. One of these specimens has the frontal bones perfect, one postfrontal, and a number of other important parts of the same skull and skeleton. The frontals are slightly sigmoid longitudinally, the posterior margin and the interobital region being elevated. They are closely covered with thick osseous scutes, which have their tubercles so arranged as to form an ocellated pattern. The plates of the middle row on each frontal between the orbits have their length and width nearly equal. The pterygoid bones have a narrow band of teeth near their inner margin, and exterior to this in front a second short band. This species was rather larger than the type specimen of *G. sylvestris*.

*Measurements.*

| | |
|---|---|
| Length of frontals on median line, | 38· mm. |
| Width between orbits, | 19·5 |
| Width at posterior margin, | 32·4 |
| Transverse diameter of distal end of humerus, | 18·4 |

The specimen here described is from Grizzly Buttes, Wyoming, and was found by Mr. J. F. Page.

### *Oreosaurus lentus*, sp. nov.

Another species of lizard, which may be placed provisionally in the genus *Oreosaurus*, is indicated by a number of caudal vertebræ, widely different from any hitherto known. These vertebræ are short, and have the lower surface of the centra deeply excavated, longitudinally, by a broad groove, which makes the inferior border of the articular cup and ball emarginate. The chevrons are thus attached on either side to a prominent ridge, their position being a little behind the middle of the centrum. The reptile represented by the remains preserved was apparently about two or three feet long.

*Measurements.*

| | |
|---|---|
| Length of anterior caudal vertebra on lower surface, | 7· mm. |
| Transverse diameter of articular cup, | 4·4 |
| Vertical diameter, | 2· |
| Distance from chevrons to end of ball, | 3·5 |

The known remains of this species were found by the writer, in September last, near Henry's Fork, Wyoming.

### *Oreosaurus gracilis*, sp. nov.

A somewhat smaller lizard, which may for the present be likewise referred to the genus *Oreosaurus*, is represented by several specimens, nearly all pertaining to different individuals. The most characteristic of these is the greater portion of a right lower jaw in perfect preservation. The teeth in this specimen are numerous, and close together. They are conical in form, with small bases, and smooth sharp summits, slightly compressed. The jaws were slender, and but loosely attached to each other by a small symphysis. The groove for Meckel's cartilage is narrow, and very short. The specimens preserved indicate a reptile about two feet in length.

*Measurements.*

| | |
|---|---|
| Space occupied by the fourteen anterior teeth of lower jaw, | 10·2 mm. |
| Depth of jaw below fourteenth tooth, | 3·5 |
| Thickness of jaw at this point, | 2·4 |
| Length of symphysis, | 2· |

The remains on which this species is based were found, last autumn, by the writer, near Henry's Fork, Wyoming. The geological horizon is Eocene.

### *Oreosaurus microdus*, sp. nov.

Another species, apparently belonging to the genus *Oreosaurus*, and about as large as *O. gracilis*, may be established on some isolated remains which are quite characteristic. One of these is part of a lower jaw with the teeth in excellent preservation. The latter are unusually small and slender, and curve gently outward. The crowns are nearly round; the summits obtuse, somewhat compressed, and marked by irregular striæ. The jaw is stout, and the groove for Meckel's cartilage large.

*Measurements.*

| | |
|---|---|
| Space occupied by four teeth from near middle of lower jaw, | 2·4 mm. |
| Depth of jaw near middle, | 3·5 |
| Width of jaw near middle, | 2· |
| Length of lower tooth including base, | 2· |

The only remains that can now with certainty be referred to this species are from the Eocene beds, near Henry's Fork, where they were found by the writer, last September.

### *Oreosaurus minutus*, sp. nov.

A very diminutive lizard, with teeth closely resembling those of *Oreosaurus*, is indicated among our Wyoming fossils by some fragments of jaws and other less characteristic remains, belonging to several individuals, which differed somewhat in size, but agreed in their more important characters. The teeth in these specimens are cylindrical, slender, with small bases, and sharp summits. They are near together, and curve slightly inward. The rami of the lower jaw were quite slender, and apparently united together only by cartilage. Several vertebræ were obtained by our party, which probably pertain to this species. The remains preserved indicate reptiles not more than six or eight inches in length.

*Measurements.*

| | |
|---|---|
| Space occupied by eight anterior teeth of lower jaw, | 2·2 mm. |
| Depth of jaw below eighth lower tooth, | 1·5 |
| Thickness of jaw at this point, | ·8 |
| Space occupied by four upper teeth of larger specimen, | 2· |

The type specimens of this species were discovered by the writer, last autumn, near Henry's Fork, Wyoming.

### *Tinosaurus lepidus*, sp. nov.

A species of very small lizards, apparently belonging to the genus *Tinosaurus*, may be established on some fragmentary remains among our Wyoming fossils. One of these is the anterior half of a lower jaw in good condition. The teeth in this specimen are compressed, and closely resemble those in the chameleon. The rami of the lower jaw were stout, and not deep, and met each other at a considerable angle. The symphysis is short, and its surface nearly smooth, showing that the rami were but slightly attached. The groove for Meckel's cartilage is unusually large. The animal represented by the remains preserved was probably not more than a foot in length.

*Measurements.*

| | |
|---|---|
| Space occupied by four anterior lower teeth, | 4· mm. |
| Depth of jaw below fourth tooth, | 2· |
| Width of jaw at this point, | 1·6 |
| Length of symphysis, | 2· |

The specimen here described was found by Mr. O. Harger, in September last, near Henry's Fork, Wyoming.

### *Iguanavus exilis*, gen. et sp. nov.

A new and interesting genus of extinct lizards, very different from those already described, may be predicated upon a number of vertebræ, and a few other isolated specimens which were brought to light during our explorations last year in the Eocene of Wyoming. The caudal vertebræ preserved are slender, and much elongated. The articular ball is but little depressed, only slightly inclined, and very convex. The chevrons are attached at the posterior end of the centrum, as in the Iguanas, to which the present species appears to be clearly related. The centra of the caudals, however, are less compressed than in that genus, and also want the unossified transverse septum. The vertebræ appear to have had only a rudimentary zygosphenal articulation. The specimens that can now be placed in this species belonged to animals about two feet in length.

*Measurements.*

| | |
|---|---|
| Length of twelfth caudal vertebra on lower surface, | 7· mm. |
| Transverse diameter of articular cup, | 2·2 |
| Vertical diameter, | 1·8 |
| Transverse diameter of articular ball, | 2· |

The remains above described were found last September, by the writer, near Henry's Fork, Wyoming.

### *Limnosaurus ziphodon*, gen. nov.

*Crocodilus ziphodon* Marsh. This Jour., vol. i, p. 453, June, 1871.

Additional remains of this species, since obtained by the Yale party at the same locality as the type specimen, clearly show that it belongs to a genus quite distinct from the modern *Crocodilus*. The sharp, compressed teeth, with both edges serrated, differ widely from those of any known Crocodilians, and alone afford a good distinctive character. Others will be given in the full description.

Yale College, New Haven, Sept. 21st, 1872.

# SCIENTIFIC INTELLIGENCE.

## I. CHEMISTRY AND PHYSICS.

1. *Water not an Electrolyte.*—The books have long taught that the object of adding an acid, in the electrolysis of water, is to render it a conductor. The fact that compound substances when decomposable conduct only by electrolysis, and hence, that a body not an electrolyte cannot be made so by the addition of another body, has long rendered it probable that it is the acid which is actually decomposed by the current, and that the water suffers decomposition only by a secondary action. BOURGOIN has investigated the subject experimentally, and has proved that water is not itself an electrolyte. His apparatus consists of a cell, divided into two equal compartments by an impermeable septum, which septum is pierced with an opening so minute as to prevent any mixing of the liquids on its two sides, while yet it allows the passage of the current. The cell is so arranged that the gases evolved from the electrodes may be collected and measured. Both compartments are filled with water acidulated with sulphuric acid, and the current is passed for a given time, the hydrogen being collected. When the experiment is concluded, the contents of the compartments are separately analyzed. Under these circumstances it is found that, in the positive compartment, the acid has increased in amount by a certain quantity $\alpha$, while in the negative it has diminished by the same amount. The quantity of sulphuric acid decomposed is then equal to $2\alpha$. But this quantity of acid can furnish only a third of the hydrogen obtained; or, calling P the weight of the hydrogen measured, the acid can yield a quantity of hydrogen equal to $\frac{P}{3}$. It is therefore certain that it is not $H_2SO_4$ which is decomposed, but $H_2SO_4+(H_2O)_2$, or $H_6SO_6$. Two hypotheses may be offered to explain this result: (1) Both the water and the acid are decomposed by the current, but successively:

$$\begin{array}{lll} & \text{Positive electrode.} & \text{Negative electrode.} \\ H_2SO_4 = & (SO_3+O) + \ldots\ldots\ldots & H_2 \\ (H_2O)_2 = & O_2 \quad + \ldots\ldots\ldots & H_4, \end{array}$$

or (2) The current decomposes a definite compound having the formula $SO_3(H_2O)_3$, or $H_6SO_6$:

$$H_6SO_6 = (SO_3+O_3) + \ldots\ldots\ldots H_6.$$

Facts show the second supposition to be the true one. Operating, for example, with currents of varying intensity, upon liquids containing different proportions of acid and water, from $H_2SO_4+3$ aq. to $H_2SO_4+125$ aq., it is found that the ratio of the acid decomposed to the hydrogen evolved is always that above given; which would not be the case, in all probability, were the acid and water separately electrolyzed. Moreover, the compound $H_6SO_6$ is not a hypothetical one, since an acid of this constitution has been rendered probable by the maximum contraction observed when one molecule of $H_2SO_4$ and two of water are mixed. In the case of nitric acid, the action of the current appears to be upon the group $N_2O_5(H_2O)_4$; a body conceded to exist.

Crystallized oxalic acid, fortunately, when in solution, is electrolyzed alone, no water taking part. The hydrogen disengaged corresponds to the equation:

$$C_2H_2O_4(H_2O)_2 = (C_2O_4+O_2) + \ldots\ldots\ldots (H_2)_3.$$

As only carbonic dioxide is set free at the positive electrode, it must be that the oxygen evolved reacts upon and destroys another portion of the acid, thus:

$$(C_2H_2O_4(H_2O)_2)_2+O_2 = (C_2O_4)_2+(H_2O)_6.$$

Moreover, if this interpretation be true, the quantity of acid destroyed should be much greater at the positive than the negative electrode; for:—

| | | |
|---|---|---|
| Acid destroyed | (1) At N electrode. | By current 1 molecule. |
| | (2) At P electrode | By current 1 " |
| | | By oxygen 2 " |

Now, experimentally, the loss of acid at the positive electrode is exactly three times greater than at the negative. Again, in electrolyzing formic acid, only carbonic dioxide is disengaged at the positive electrode. Three hypotheses may be offered to explain the result: (1) The current acts on the acid only, thus:

Positive electrode. Negative electrode.

$$(CH_2O_2)_2 = (C_2H_2O_3+O) + \ldots\ldots\ldots H_2,$$

and then at the positive, the further reactions occur:

$$C_2H_2O_3+O = CO_2+CH_2O_2.$$

(2) Water alone is decomposed:

$$CH_2O_2+O = CO_2+H_2O,$$

and (3) The acid and the water are both decomposed simultaneously:

$$(C_2H_2O_3O)+O = (CO_2)_2+H_2O.$$

If $\alpha$ represent the amount of acid electrolyzed, the loss will be: by the first hypothesis, nothing at the positive and equal to $\frac{\alpha}{2}$ at the negative electrode; by the second, on the contrary, there is no loss at the negative, and the loss is equal to $\alpha$ at the positive electrode; and by the third, it is equal in each compartment, being represented by $\frac{\alpha}{2}$. Now experiment shows that there is no loss of acid at the positive electrode; hence the first hypothesis is the true one, and the water is not decomposed by the current. The same general results were obtained in the electrolysis of alkalies and salts. Bourgoin concludes, therefore, that: "Water is not decomposed by the electric current; it plays the part of a solvent only."—*Bull. Soc. Ch.*, II, xvii, 244, March, 1872.

G. F. B.

2. *Further Examination of the new Platinic chloride.*—Norton has analyzed the new crystallized platinic chloride which he described in December, 1870 (this Journal, III, i, 375). It yields the formula $PtCl_4, 5H_2O$. Of the five water molecules, 16 per cent or 4 molecules, are driven off at 100°. On increasing the heat, platinous chloride is left. No constitutional formula for it is offered.—*J. pr. Ch.*, II, v, 365, May, 1872. G. F. B.

3. *On the Formation of Chloral.*—The action of chlorine upon aldehyde $\begin{matrix}CH_3\\|\\COH\end{matrix}$ produces acetyl chloride $\begin{matrix}CH_3\\|\\COCl\end{matrix}$, as Wurtz some time ago showed. In this case it is not the methylic grouping $CH_3$, but the incomplete group COH, which is attacked. In order to render this latter group more resistant, Wurtz and Vogt load it, as it were, with other groups; hoping thereby to limit the action to the methyl group. For this purpose, they use the compound $CH_3\text{---}CH\left\{\begin{matrix}OC_2H_5\\Cl\end{matrix}\right.$, obtained by Wurtz and Frapolli by acting with hydrochloric acid gas on a mixture of aldehyde and alcohol. Here the COH group is replaced by one of greater complication, $CH\left\{\begin{matrix}OC_2H_5\\Cl\end{matrix}\right.$. Upon submitting this substance to the action of chlorine in presence of a trace of iodine, the predicted tetrachlorinated ether, $CCl_3\text{---}CH\left\{\begin{matrix}OC_2H_5\\Cl\end{matrix}\right.$, is obtained. This it is easy to transform into chloral by the action of water:

$$CCl_3\text{---}CH\left\{\begin{matrix}OC_2H_5\\Cl\end{matrix}\right.+H_2O=HCl+C_2H_5(OH)+CCl_3\text{---}COH.$$

Heated with alcohol, hydrochloric acid and trichloracetal are produced, thus:

$$CCl_3\text{---}CH\left\{\begin{matrix}OC_2H_5\\Cl\end{matrix}\right.+C_2H_5(OH)=HCl+CCl_3\text{---}CH\left\{\begin{matrix}OC_2H_5\\OC_2H_5\end{matrix}\right..$$

Moreover, the hydrochloric acid acting on an excess of alcohol produces, at the same time, ethyl chloride.

By the above reactions, the authors explain the action of chlorine upon alcohol in the production of chloral. The first stage produces aldehyde and hydrochloric acid, as Stas has shown:

$$(1.)\quad \begin{matrix}CH_3\\|\\CH_2OH\end{matrix}+Cl_2=(HCl)_2+\begin{matrix}CH_3\\|\\COH\end{matrix}.$$

This hydrochloric acid acts upon the aldehyde and the excess of alcohol, to form the monochlorinated ether of Wurtz and Frapolli:

$$(2.)\quad C_2H_5(OH)+\begin{matrix}CH_3\\|\\COH\end{matrix}+HCl=CH_3\text{---}CH\left\{\begin{matrix}OC_2H_5\\Cl\end{matrix}\right.+H_2O.$$

This ether then becomes tetrachlorinated by the action of the chlorine, as above shown:

$$(3.)\quad CH_3\text{---}CH\left\{\begin{matrix}OC_2H_5\\Cl\end{matrix}\right.+Cl_6=CCl_3\text{---}CH\left\{\begin{matrix}OC_2H_5\\Cl\end{matrix}\right.+(HCl)_3.$$

And finally, by the action of water produced in the last reaction, the tetrachlorinated ether produces chloral and alcohol:

$$(4.)\ CCl_3\text{-\,-\,-}CH\left\{\begin{matrix}OC_2H_5\\Cl\end{matrix}\right. + H_2O = HCl + C_2H_5(OH) + CCl_3\text{-\,.\,-}COH,$$

or, by the action of alcohol, produces trichloracetal,—a substance already detected by Lieben in the products of the chloral manufacture.

Wurtz and Vogt have also observed the formation of both dichlor- and trichlor-aldehyde (chloral) by the action of chlorine upon a cooled mixture of aldehyde, hydrochloric acid and water; or even upon a solution of aldehyde in water. If, however, the temperature rises, polymerization takes place, and croton-chloral hydrate is found among the products.—*C. R.*, lxxiv, 777, March, 1872. G. F. B.

4. *On a new Organic Base, obtained from Dulcite.*—Bouchardat has succeeded in obtaining an organic base containing oxygen, by acting upon one part of dulcite monochlorhydrin with ten parts of alcohol saturated with ammonia gas, for six hours, at 100°. The chlorhydrate of the new base—which the author calls dulcitamine—is dissolved in absolute alcohol, from which it crystallizes out in long needles on the gradual addition of ether. It is freely soluble in alcohol and water, insoluble in ether; its solution in water is neutral and tastes sweetish. Treated with silver oxide, it yields free dulcitamine, as a powerful base, easily displacing ammonia from its combinations, bluing strongly red litmus paper, and attracting carbonic acid from the air. When concentrated, it becomes an uncrystallizable syrup. With acids, it forms difficultly crystallizable salts. With platinic chloride, it forms a compound crystallizing in long orange-yellow needles, soluble in water and alcohol. Dulcitamine has the formula $C_6H_{15}NO_5$, and resembles glyceramine in many of its properties. Its discovery furnishes new proof of the close relations between the triatomic alcohol, glycerin, and the hexatomic alcohol, dulcite.—*C. R.*, lxxiv, 1406, May, 1872. G. F. B.

## II. GEOLOGY AND NATURAL HISTORY.

1. *Hayden Rocky Mountain Geological Expedition.*—(From a letter by Dr. F. V. Hayden, in charge of the Expedition, to J. D. Dana, dated Madison Valley, Montana Territory, Sept. 1, 1872.)—The following is a brief summary of what the survey under my charge has accomplished up to the present time, and what it proposes to do before the end of the season.

Two large and well-equipped parties have been in the field at work since about the 1st of July. The largest party made Ogden the point of departure. It was under the direction of Mr. James Stevenson, my principal assistant. There are attached to this party a geologist, topographer, astronomer and meteorologist, with the necessary assistants for each. There is also a botanist, who has already collected over 1200 species of plants through that new and interesting region, the Snake River valley. Dr. Josiah Curtis acted as surgeon and microscopist. The party surveyed a route from Ogden to Fort Hall, Idaho, where full preparations were made for a pack train with supplies for a given time. The party passed up the west side of the Snake River valley, forced their way across the mountains, made a careful survey of the Teton range, then passed up the valley of Henry's Fork, entered the Madison valley through the Targee pass, and reached the Geyser Basin of the Madison August 14th.

The party under my charge traveled by stage to Fort Ellis, and there spent about three weeks preparing the outfit; then started up the Yellowstone valley, over about the same route as last year. The party consisted of about thirty persons, among them a chief topographer, astronomer, meteorologist, mineralogist, with their assistants, and a number of others who acted as collectors. A careful examination of the Yellowstone valley was made, and a map in contour lines of 100 feet each constructed.

Both parties met in the Geyser Basin on the same day within a few hours of each other. The two parties numbered about seventy persons. The results of the exploration up to that time proved on examination to be most satisfactory and not less important to science than of practical value to the country. The opening up of that great Snake River valley will prove one of the most important events in American explorations for the year 1872. The barometrical elevations show most feasible routes for railroads, connecting the entire northwest with the Pacific railroads. It opens up to settlement a vast territory of the finest land in the west. A railroad up the Snake River valley from Utah, which is now in contemplation, will bring into market 2500 square miles of pine timber, and an unlimited quantity of arable and pastoral land. The ascent of the Great Teton will be recorded as one of the events of the season. Mr. James Stevenson and Hon. N. P. Langford are undoubtedly the only white men that ever reached its summit. Mr. Stevenson planted the American flag on its summit, and measured its height with an aneroid barometer. It was also measured by triangulation from below. The height was ascertained to be about 13,400 feet. The next important discovery made by this party was the four remarkable passes at the head of Henry's Fork. These passes correspond to the four points of the compass, and are all within a few miles of each other. Henry's Lake is located in the center. The Targee or East pass is about 6,500 feet elevation, and forms one of the great gateways to the Madison valley, and the sources of the Madison and Yellowstone. Henry's or South pass is about 6,000 feet, and opens into the Snake valley; Red Rock or West pass, 6,300 feet, connects the great valley of the Jefferson branch, while the Madison or South pass opens into the lower Madison. All these passes are so smooth and low that one may ride over them in a carriage at full speed. There is probably not a more interesting geographical point on the American continent where there are, within an area of a few miles, four such remarkable passes, linking the Pacific with the Atlantic slopes. There is not the slightest obstruction to a railroad over either of these passes. Emigrants are already following our track of exploration, and within two years the country will be filled with settlers. The railroad company that secures the right of way through this country soon will hold the key to the development of a State.

Observations for latitude and longitude have been made by both parties every night when possible. As nearly all the nights have been clear and the weather fine, these observations have been very numerous, and their value in the location of important points, as well as correcting the old maps, has been great. The Tetons were thirty miles out of place. They were found to be within the boundary of Wyoming territory, and not in Idaho. All these observations will be continued until the 1st of November, without interruption. Each party is supplied with three of Green's best cistern barometers, with a plenty of aneroids, and thermometers of various kinds. Especial regard is paid to the climate, and to elevations for railroad routes. Particular attention is paid to all results of a practical character, on the principle that the money that enables us to make these explorations comes from the people, and should return as far as possible to them in a shape which will be available to them.

The party under Mr. Stevenson is now on its way down the east side of the Snake River valley, having carefully surveyed the sources of that river: every branch will be carefully located. The party will reach Fort Hall about the 13th of October. It will then survey a parallel belt to Salt Lake City, thus connecting all our work in the northwest with the Pacific railroad. The party under my charge will complete the survey of the Madison river and its branches, then the Gallatin to its sources, then pass over the mountains into the valley of the Yellowstone, down to the snowy range, to the mouth of Shields river, then to the three forks of the Missouri, and then down that river to Helena, where our labors will probably close about the 1st of November. The latitude and longitude of Fort Ellis has been quite carefully fixed by an extended series of observations by Mr. Gannett with a transit, and similar observations will be made at Virginia City and Helena.

Besides the two large parties mentioned above, there have been a number of smaller ones operating in various portions of the west, under the auspices of this survey. Prof. Cyrus Thomas has spent the season in the northwest collecting agricultural statistics, and all other information of a practical character. He has been ordered to visit Dakota and Minnesota, and to push his way as far northward into the Red River country as possible. The public may look for a continuation of his agricultural reports, as well as papers on insects, and other subjects.

Prof. E. D. Cope, one of our most distinguished scientists, fitted out quite an extensive party at Fort Bridger at the expense of the survey. He will remain three months in the field. In the first two weeks of his examinations he reported the discovery of over fifty new species of extinct vertebrates. He will close his season's work by an examination of the celebrated Kansas bone deposits. The results of his labors will be of great importance to Geology, Paleontology, and Natural History generally.

Professor Joseph Leidy, the eminent comparative anatomist of Philadelphia, is also exploring the west for fossil vertebrates. He is also making a study of the minute forms of life under the microscope, and will present a report on the minute fauna and flora of the districts he visits.

Mr. F. B. Meek, accompanied by H. M. Bannister, of the Smithsonian Institution, has spent about two months along the Pacific railroad for the purpose of making a critical examination of disputed or obscure points in the geology of that interesting region. Their success has been most satisfactory, and a valuable report may be expected.

Prof. Leo. Lesquereux, our great authority on the Coal formations and the fossil plants found with them, has spent most of the summer, assisted by his son, in the west. He first went to Denver, stopping for a time along the Kansas Pacific railroad, thence along the base of the mountains to Santa Fé, N. M. He then explored the coal beds around Denver, and proceeded to Cheyenne, and made a critical investigation of the Coal formations from Cheyenne to Ogden, along the Union Pacific railroad. Large numbers of fossil plants, new to science, were discovered, all of which will be described in the forthcoming annual report next winter.

These special examinations had for their prime object the determining, by the most overwhelming evidence, the relations of the great group of Tertiary beds of the west to the Cretaceous. It is the purpose of this survey to take nothing for granted, to accept no statement without indubitable proof, and from year to year a certain amount of force will be concentrated on all obscure points of western geology. The amount of new material in all departments of research, to be illustrated in the quarto series of volumes connected with the survey, which has been added this present year, surpasses all previous years.

2. *On the Owen's Valley Earthquake.*—The August and September numbers of the *Overland Monthly* contain articles by Prof. J. D. Whitney on the "Owen's Valley Earthquake" of March 26th, 1872. In pursuing the geological survey of the State, a party found it necessary to pass through the valley, and occasion was taken to make such scientific inquiries and observations as the time allowed. The first paper, of which the following is a brief abstract, describes the geological character of the region and the local phenomena.

Owen's valley is about 70 miles long, and is enclosed on the west by the Sierra Nevada, rising from 10,000 to 11,000 feet above the valley, and on the east by the steep and narrow range of the

Inyo mountains, which rise from 4,000 to 7,000 feet above the plain, this itself being 4,000 feet above the sea level.

On the Sierra side there extends from the base of the precipitous portion of the range a long slope descending at an angle of five or six degrees, made up of detritus from the mountain behind and covered with sage brush. This is a vast bed of boulders, gravel and sand, 2,000 feet thick at the upper end, and spread out at the foot of the range in a bed of varying width. Just where this "sage brush slope" meets the valley bottom the vegetation assumes a somewhat meadowlike character, and here, at the junction of these two formations, all the settlements of the valley are located.

The geology of the valley is also interesting. The Sierra is chiefly one vast mass of granite, which has been elevated since the Jurassic period, and belongs to the Sierra Nevada system of upheaval. The Inyo range, on the other hand, is much more ancient, being a part of the great Paleozoic formation of the Great Basin, and consists of limestones, sandstones, and other stratified material. The two ranges are thus of very different geological ages. Midway in the valley, commencing about thirty miles north of Lone Pine and extending for ten miles, is a region of volcanic cones and lava flows apparently of recent geological date, but now all quiet. Similar cones and lava flows are found in the Coso mountains south of Owen's lake; and on the tableland north of the valley and south of Mono lake there are abundant indications of former volcanic activity, in the form of *solfataras* and hot springs.

After speaking of the effects of the shock in destroying buildings at Little Lake, Haiwee, Lone Pine, Independence, etc., the writer says :—"The almost universal testimony of the residents of Owen's valley was to the effect that the shocks came from that portion of the Sierra Nevada which lies between Owen's lake and Independence. In the region to the south of the lake the vibrations were felt as approaching from the northwest; at Lone Pine they were referred to the high mountains in the immediate vicinity to the west, and as we moved up the valley, the direction assigned was always more to the south of west as we proceeded north." The recurrence of subterranean noises preceding or accompanying the shocks is confirmed, and the noises are referred to the cracking and grinding of the rocks under tension, rather than to the explosion of detonating gases.

Numerous fissures were noticed which in some places may be traced uninterruptedly for miles, and between these fissures the surface had frequently sunk to a depth of sometimes twenty or thirty feet. "In all cases the character of the disturbances of the soil seemed to be pretty much the same; namely, the depression of narrow belts between fissures running nearly parallel to the course of the Sierra, and chiefly limited to the edge of the 'sage brush slope.'" A tidal wave was produced in Owen's lake, but caused no damage beyond the temporary inundation of the shore.

"There are several places in the valley where fissures in the ground have crossed roads, ditches or lines of fences, and where evidence has been left of an actual moving of the ground horizontally as well as vertically. One of these instances of horizontal motion is seen on the road from Bend City to Independence, about three miles east of the latter place. Here, according to a careful diagram of the locality, it appears that the road running east and west has been cut off by a fissure twelve feet wide, and the westerly portion of it carried eighteen feet to the south." Other similar instances were noticed.

The "*General Conclusions*" arrived at in the second paper are, that the impulse by which this earthquake was originated was given somewhere nearly in the axis of the Sierra, at a depth of at least fifty miles, and at the same moment along a line of considerable extent, probably as much as a hundred miles north and south. The resulting waves were propagated in both directions from this mountain axis and nearly parallel with it, and advanced on the surface at a rate of from thirty to thirty-five miles in a minute, if measured in a line at right angles to the axis of the Sierra.

C. G. R., JR.

3. *Bahamas.*—[The following are a few notes from an abstract of a paper which was presented to the Geological Society in 1852, by Captain (now Major General) R. J. Nelson, R. E. (author of the Memoir on the Bemudas), which, through oversight, is not noticed in the writer's recent work on Corals and Coral Islands. The abstract appeared in the Quarterly Journal of the Society for 1853, p. 200.—J. D. D.]

The loftiest land in the Bahamas, according to the maps of the Hydrographical Office, is only 230 feet above the sea. Generally speaking, the hills on the larger islands are much under 100 feet in height, and on the islets from 50 to 10 feet. * * * * The surface generally is occupied by low rocky hills, either surrounding basins or forming parts of what may once have been basins, and rarely by distinct hill and valley of the ordinary character. The bottoms of these basins are usually flat and rocky, only a few inches above the average high-water level, and have a rough and cavernous surface. Water, more or less brackish, rises and falls everywhere throughout the lower parts of these flats, though not contemporaneously with the tide*, or at a uniform rate. The surface is sometimes covered with grass and low bush, and sometimes it consists of the bare rock, full of hollows, which are coated or even arched over with sub-stalagmitic substance. It is in these cavities, locally termed "pot holes," that most of the soil is found; and in the gardens made on such ground, fruit-trees, pine-apples, Indian corn, sugar-cane, etc., grow luxuriantly. Besides these "rock-marshes" there are also ordinary marshes and mangrove swamps, of no great extent or depth, which are more or less in connection with the sea. On the larger islands the rocky surface

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* At Nassau, Bahamas, the tide rises from 4 to 3 feet (spring to neap); but at Bermuda it rises from 6 to 4½.

of the hills is very thinly and partially covered with "red earth," mixed in varying proportions with vegetable matter. This scanty soil is fertile, if well used. When uncleared, it is covered with bush and forest trees. There are also sandy tracks termed "pine-barrens," where the bush suddenly disappears and the palmettos become fewer in number, though enough remain to exhibit an intermixture of pines and palms, respectively typical of the northern and southern floras. The lowest portions of the flat grounds frequently contain small brackish water or salt lakes. In the chalk-marsh of Andros Island, however, there is a freshwater lake, with three streams as its outlets; and it appears that there is no other freshwater lake or stream in the Bahamas. * * * * There are large caverns in Long Cay and Rum Cay; and probably caverns are as numerous in the Bahama Islands as in the Bermudas; but so few extensive excavations have been made, that this cannot be positively affirmed. * * * * One of the most striking objects in the topography of the Bahamas is the very deep submarine valley, forming the gulf known as "the Tongue of the Ocean," which runs into the Great Bahama Bank from its northern end. The color of the water around the islands is usually that of the *aqua-marine* variety of beryl; but the water of the Tongue of the Ocean has the deep blue color of oceanic depths.

The author describes a coral-reef as consisting of masses of numerous species of *Madrepora*, *Astræa*, *Dædalea*, *Oculina*, bases and axes of *Gorgonia*, *Millepora*, *Nullipora*, *Corallinæ*, &c., &c., growing confusedly together without any other apparent order than that of accidental succession and accretion, both laterally and vertically. These are at times aided or even superseded by *Serpulæ*, &c., as seen in the serpuline reefs. * * * *

Capt. Nelson points out a few of the localities that exhibit most clearly the character, source, and mode of aggregation of the materials of the ordinary Bahama rock, such as is formed above the sea level; at the same time referring to the illustrative specimens in the Bahama collection. For instance: the south side of Silver Cay and the beach extending westward from Nassau afford rolled blocks, pebbles, and sand derived from the more massive corals, mixed with remains of turtles, fish, crustaceans, echinoderms, and mollusks. On the beach between Clifton Point and West Bay (specimen No. 1) the shells of *Strombus gigas* more especially accompany the rolled corals. At East Point (specimens Nos. 2 and 3) the sand is derived from corallines and nullipores; the finer sand being often in approximately spherical grains, though not so perfectly as at the White Cay (specimen No. 4) and between Exuma and Long Cay. The beach near Charlotteville Point (specimen No. 5) consists principally of *Lucina Pennsylvanica* in various stages of comminution. At Six Hills (Caicos Group) the mass of Conch shells (*Strombus gigas*) is so great and sufficiently cemented together as to form not only rock, but an island several hundred feet in length. Along the N. W. beach at Gun Cay (specimen No. 8), a hard, coarse, stratified rock is formed of Conch and other shells, together with coral fragments.

The large fragments of corals and shells are never found much beyond the surf-range of high-tide, and therefore always form rock at a low level; whilst, on the contrary, the fine calcareous sand is removed by the wind and deposited in irregularly laminated beds, which, being consolidated in various degrees, are converted into rock of different qualities. * * * * The ordinary Bahama rock everywhere consists of the above-mentioned calcareous sandstone. It is somewhat similar to Portland stone in appearance, but softer and more porous. When first exposed it is quite white, and is inconveniently bright and dazzling under a tropical sun; but it becomes of a dark ashen-gray color along the sea-coast, and more or less so elsewhere, when exposed to the weather. Its average weight, like that of the Bermuda stone, varies from 95 to 145 pounds per cubic foot. Its inferior value as a building material arises from the numerous sand-flaws (specimen No. 7), and consequent ready failure when exposed to the weather. About the south-west of New Providence, for some feet above the sea, the rock is hard and homogeneous, and may be raised in good blocks for building purposes. The looser and softer kinds of rock are found usually on the hill tops. A variety offering a singular counterfeit of true oölitic structure is found at or near White Cay, Exuma, and elsewhere; but the spherules are solid, and have been derived apparently from the stems of corallines. * * * * A chalk-deposit is to be found, by all accounts, in the different basins or lagoon bottoms in every principal group, though nowhere so extensively as along the western coast of Andros Island, where it may almost be termed *a young chalk formation.* * * *

The "red earth" previously mentioned as forming, generally speaking, the scanty soil of the Bahamas, is at times interstratified with the rock, and sometimes it is incorporated with it. It is identical with the "red earth" of the Bermudas (specimen No. 15) which proved a considerable source of embarrassment, especially with reference to Ireland Island, by seeming to point out alternations of aqueous and other deposits, which were contradicted by the presence of the characteristic *Helix* in all the beds. In visiting a cave near Delaport in 1849, Capt. Nelson found the bottom of the cave for many feet in depth covered with a loose dry "red earth," in grains varying in size from coarse sand to fine dust (specimens 14 and 14 *a*, *b*). Under the microscope this appeared as a mass of insect-remains, the *rejectamenta* of bats living in these caverns. Specimens of the earth from another part of the same cave, however, were so much altered in character, that they resembled the Bermuda "red earth," and afforded a complete clue to the characters of this substance. Some of the varieties from the Delaport cave were examined microscopically and chemically by Professor Quekett, of the Royal College of Surgeons, who not only confirmed the above, but announced that all the varieties gave off ammonia, whether retaining organic texture or not. The author thinks it not unlikely that the "red earth," even in the case of the five strata in Ireland Island, has been largely derived from bats inhabiting once-existing caverns; at the same time, he considers it probable that birds, their droppings supplying a sort of guano, have also assisted in the formation of this deposit.

The occurrence of pumice floated ashore at Watling Island, and elsewhere in the Bahamas (as also at Bermuda), is briefly noticed.

4. *Surface Geology of Northwestern Ohio;* by Prof. N. H. WINCHELL.—(Read before the American Association at Dubuque.)—After some preliminary remarks on the uniformity of the geological structure of Northwestern Ohio, Prof. Winchell said that this was a circumstance peculiarly favorable for the preservation of the features of the drift. The whole of the vast tract is a plain with no more unevenness than the prairie region of Illinois. He accepted the glacier theory of Prof. Agassiz to explain nearly all the features of the drift of Northwestern Ohio. There are six long ridges which have received the names of St. Johns, having an elevation above Lake Erie of about 425 feet: Wabash, which is about 375 feet above that lake; the St. Mary's, ranging from 285 to 390 feet above Lake Erie; the Van Wert, ranging from 194 to 240 feet; the Blanchard ridge from 188 to 218 feet; and the Belmore ridge, about 150 feet above Lake Erie. These he regarded as so many terminal moraines left in the retreat of the local glacier which filled the St. Lawrence valley, including the basins of Lakes Ontario and Erie, as well as the valley of the Maumee, about the close of the Glacial epoch. He said all the drift in Northwestern Ohio is of glacier origin, and lies, with very slight exceptions, in a broad sheet evenly spread just as the ice deposited it. It consists from top to bottom of the "boulder clay," the boulders in it being marked by glacier action. He explained by crayon diagrams how the drift, frozen in the ice, or riding on its back, was gently let down on the glaciated surfaces of the rock by the slow thawing of the foot of the glacier. He supposed that the water, which resulted from the thawing of the ice, acted all along the foot of the ice, evenly, without being gathered into streams. Hence, its effect would be seen on the drift in the formation of a ridge of assorted materials, containing gravel, sand and boulders, whenever the climate caused a stand-still in the slow retreat of the ice. In that way he accounted for all the ridges mentioned, rejecting the Beach theory, and all ideas of subsidence below a continental expanse of water.

He admitted that Lake Erie has been higher than at present, but he could see no evidence of it above the height of about 200 feet. At that time it had an outlet by way of Houghton, Ind., through the valley of the Wabash, its outlet by way of the St. Lawrence valley being yet obstructed by the glacier. Above that elevation he said there is no evidence of the presence of the water of Lake Erie. He regarded as evidences of a higher stage of that lake the existence of loose sand knolls and ridges scattered over Northwestern Ohio, running in all directions and having all altitudes up to 200 feet.

These he regarded as *ozars*, or sand-bars thrown up by the action of currents and waves. Besides these sandy deposits there are numerous places known as limestone ridges, where the drift has been denuded from the rock, the boulders found in the drift being left in the immediate vicinity, usually in a belt round the bases of these ridges. The rock in such cases is water-worn and wrought into fantastic shapes, common about rocky shores.

These sandy deposits of Lacustrine origin frequently obscure the true glacier moraines for great distances, and have often been confounded with them.

The superficial lamination of the clay about Defiance, Ohio, he referred to the action of the waters of the St. Joseph and St. Mary's rivers running along the outer periphery of the moraine, which being damned back by the ice foot would form the beginning of Lake Erie, with an outlet by way of Fort Wayne and Huntington, Ind. This action was to carry the fine parts of the glacier drift to some distance away from the ice foot, and spread it in fine horizontal layers over an area of several miles square. He said such lamination occurs, also, farther down the Maumee, at Toledo, and on the Sandusky, at Fremont; and has a thickness of 25 or 30 feet. It is, however, confined to the river valley, and must have been done by the action of those rivers, the waters of which were set back by the prevalence of the ice, so as to form standing water. Below this lamination, the typical unmodified drift, which for the most part covers the whole district, is found as elsewhere.

He regarded the existence of these moraines as a confirmation of the theory of Prof. Agassiz, and read off by proportional numbers the manner of retreat of the ice. The halting places are separated by the following figures; 15; 15; 2; 35; 3½; x.—*E. H. T.*, *Detroit Tribune of Aug.* 31.

5. *Note on* TINOCERAS ANCEPS; by O. C. MARSH.—The large mammal from the Tertiary of Wyoming, described by the writer as *Titanotherium? anceps* (this Journal, vol. ii, p. 35, July, 1871), proves to be a proboscidian, as originally suspected. This fact was stated in a note on page 123 of the present volume. The name *Mastodon anceps*, there provisionally applied to the species, was changed, in the errata, to *Tinoceras anceps*. The limb bones of this animal are similar to those of *Mastodon*, but other parts of the skeleton, especially the skull, differ widely from that genus. The Museum of Yale College has portions of several skeletons, which will soon be fully described.

6. *A Monograph of the Fossil Crustacea belonging to the Order Merostomata.* Part III, *Pterygotus and Slimonea* (pages 71–120, plates xvi–xx); by HENRY WOODWARD, F.G.S., F.Z.S., of the British Museum. 4to. London, 1872. Printed for the Paleontographical Society.—The first part of this memoir was issued in the volume of the Paleontographical Society for 1865 (pub-

lished in 1866), and was noticed in this Journal in volume xliv, p. 116 (1867). This third part treats of several species of *Pterygotus* and of the related *Slimonia acuminata* (Himantopterus acuminatus *Salter*), from the banks of Logan Water, near Lesmahagow, Lanarkshire. The descriptions of these gigantic crustaceans are illustrated with effective lithographs, partly of natural size. The genus *Slimonia* (of Page) approaches *Pterygotus*, and differs from *Eurypterus*, *Stylonurus* and *Dolichopterus*, in having the eyes marginal on the head. Besides the marginal eyes there are two ocelli on the medial line of the head-shield, about a third of its length from the front. Unlike *Pterygotus*, the antennæ are very small and not chelate, and they have the base expanded and fitted to act as jaws; and this is one of several important distinguishing characters. The head shield of one individual, described by Salter, is 6¼ inches broad and 7½ long. Mr. Woodward states that the largest specimen known of the species could not have exceeded *four feet* in length, and the larger part of those found are of individuals less than three feet long. "They cannot therefore be said to equal in size the largest species of the genus *Pterygotus*, which, no doubt, attained a length of at least five feet." One specimen of *Slimonia*, figured natural size on plate xvii, is 26½ inches long.

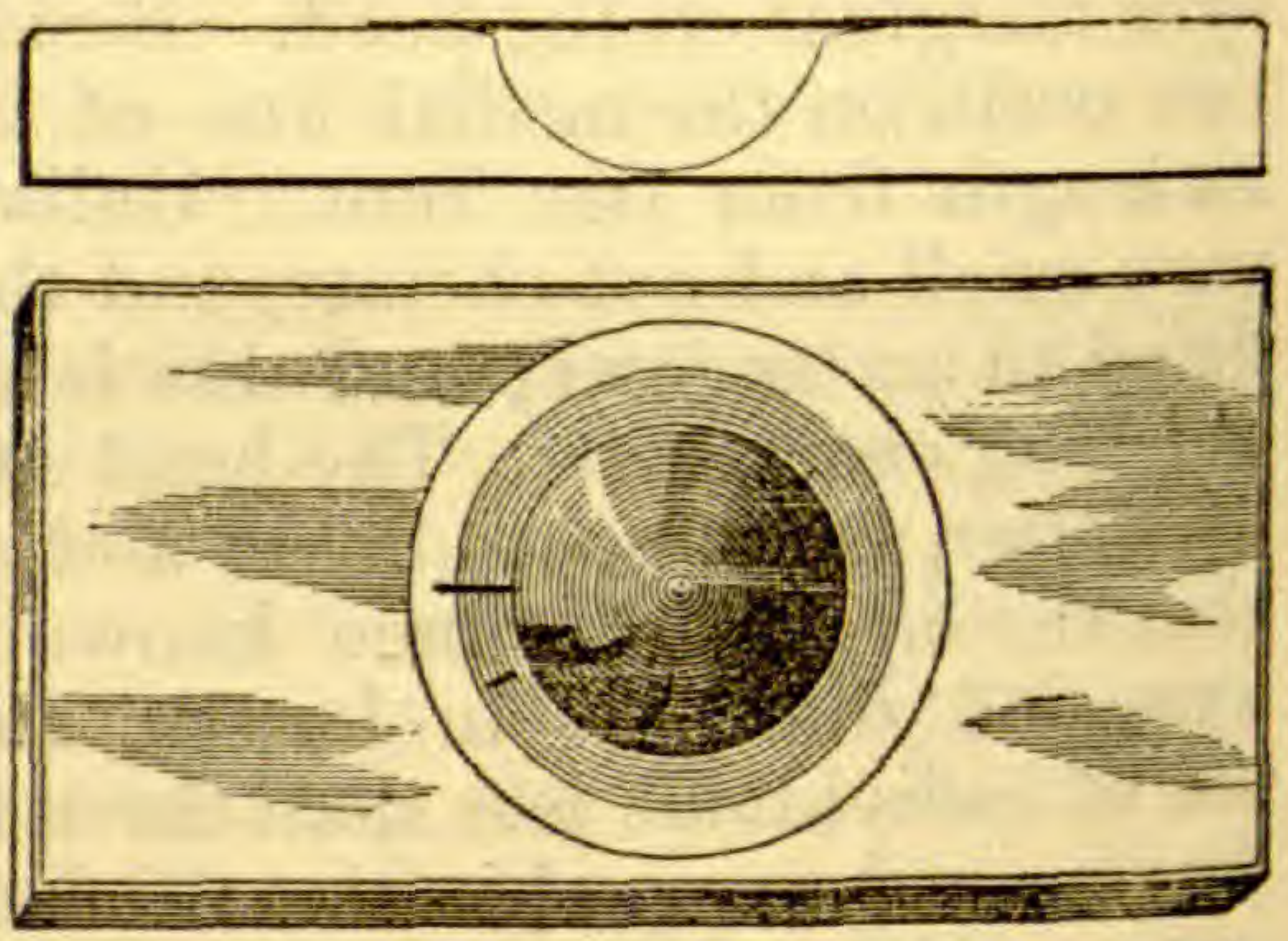
In the following part of this memoir, the author, as he announces, will give in a condensed form the diagnostic characters of each genus of the *Merostomata*.

7. *Notice of a new species of* TINOCERAS; by O. C. MARSH.—A second species of *Tinoceras*, considerably larger than *T. anceps*, Marsh, is represented in the Yale Museum by portions of a skull and teeth, with parts of the same skeleton; and likewise by fragmentary remains of several other individuals, all from the Eocene deposits of Wyoming. The skull is proportionally very small, and indicates one of the most remarkable animals yet discovered. It supports a pair of short horns, and has also two powerful tusks, which in size, shape, and direction resemble the canines of the walrus. The molar teeth are small, the last of the upper series being much the largest. The horn cores are short, somewhat curved, with obtuse compressed summits. They are about $130^{mm.}$ in length. There are apparently but five teeth in the upper molar series, and a long hiatus in front of the premolars. The tusks are compressed longitudinally, regularly curved, and worn near the extremity by the opposing teeth of the lower jaw. The left upper tusk measures $220^{mm}$. in length; $65^{mm.}$ in antero-posterior diameter, and $33^{mm.}$ in transverse diameter at the jaw. The atlas with the odontoid process is $144^{mm.}$ in length, and its posterior face $97^{mm}$ wide. The bones of the limbs and feet resemble those of the living Proboscidea. For this species I propose the name of *Tinoceras grandis ;* and the peculiar group of animals it indicates may be called *Tinoceridæ*.

8. *Microscopical: A Life Slide.*—The accompanying engravings represent front and side views of a form of life slide for the

microscope, designed and used with much success by Mr. D. S. Holman. It is constructed to retain the greatest quantity of material under the smallest cover glass, and is designed to be used with the highest powers of the microscope for studying the Bacteria, Vibriones and other very low forms of life.

The slide consists, as will be seen from the cuts, of a central polished cavity, about which is a similar polished bevel; and from the bevel outward extends a small cut, the object of which is to afford an abundance of fresh air to the living things within, as well as to relieve the pressure, which shortly would become so great, from the evaporation of the liquid within, as to cause the destruction of the cover glass.



No special dimensions are stated for the central cavity. The bevel is usually $\frac{1}{8}$ in. in diameter (the cut is $\frac{2}{3}$ of natural size); the small canal is cut through the inner edge of the bevel or annular space, outward, for the purpose named above.

It is found upon enclosing the animalculæ, &c., that they will invariably seek the edge of the pool in which they are confined, and the beveled edge permits the observer to take advantage of this disposition; for when beneath it, the objects are within range of the high-power glasses.

Another very important feature in the device is the fact that a preparation may be kept within it for days or weeks together, without losing vitality, owing to the simple arrangement for supplying fresh air.

We have repeatedly had the opportunity of witnessing the use of this slide, and are convinced that nothing of the kind has yet been devised which can equal it in excellence, either for observing, or generating the lower forms of life.—*Journal Franklin Institute.*

## III. Astronomy.

1. *Extract from the Address of* Mr. De La Rue *before the Mathematical and Physical Section of the British Association of which he was President.*—Passing to the subject of comets, Mr. De La Rue gave an explanation of Prof. Zöllner's views—a theory which, he said, acquired an additional interest from Schiaparelli's discovery of the identity of the paths of certain comets with great meteor-streams, since the meteoric masses must inevitably be converted into vapor on approaching the sun, with exhibition of the characteristic appearances of the comets. The intimate connection of planetary configuration and solar spots, of the latter and terrestrial magnetism and auroral phenomena, must, the speaker pro-

ceeded, tend to establish also a connection between solar spots and solar radiation. It is demonstrated by the researches of Piazzi Smyth, Stone, and Cleveland Abbe, that there is no connection between the amount of heat received from the sun and the prevalence of spots—a result clearly in harmony with those derived from recent investigations into the nature of the solar atmosphere. Further, in a paper by Mr. Meldrum, of Mauritius, to be read during the meeting, most remarkable evidence is given on the close connection of these phenomena. It appears that the cyclones of the Indian ocean have a periodicity corresponding with the sunspot periodicity; so that if an observer in another planet could see and measure the sunspots and cyclones (earth-spots), he would find a close harmony between them. Such a connection will probably be found to exist over the globe generally; but with reference to the Indian ocean, Meldrum's discussion of twenty-five years' observations, that in the area lying between the equator and 25° south latitude, and between 40° and 110° east longitude, the frequency of cyclones has varied during that period directly as the amount of sun-spots. Mr. Meldrum, in order to place the deductions on a still broader foundation, proposes to investigate these laws on a plan perfectly in agreement with the method of determining the areas of solar disturbances, the results of which have been published from time to time during the last ten years. Moreover, the observations on the periodic changes of Jupiter's appearance, and the observations of Mr. Baxendell, that the convection-currents of our earth vary according to the sun-spot period—all these results, seemingly solitary, but truly in mysterious harmony, point to the absolute necessity for establishing constant photographic records of solar and terrestrial phenomena all over the world. There is every hope of the photographic method as applied to sun-observations being joined to the work of the Greenwich Observatory; but what is further wanted is the erection of instruments for photographic records, and of spectroscopes in a number of observatories throughout the world, so as to obtain daily records of the sun and to observe magnetical and meteorological phenomena continuously in connection with solar activity. Meteorological observation is storing up useful facts; but they can only be dealt with effectually if investigated in close parallelism with other cosmical phenomena. The time has really come, not only for relieving private observers from the systematic observation of solar phenomena, but for drawing close ties between all scattered scientific observations so as to let one grand scheme embrace the whole; and no method seems to be so well adapted to bring about this great achievement as the method of photographing the phenomena of nature, which in its very principle carries with it all extinction of individual bias.

Col. STRANGE, in moving a vote of thanks, complimented the President on his success in rendering photography available for purposes of astronomical measurement, and thus accomplishing what the most eminent astronomers had believed to be impossible.

Sir W. Thomson, in seconding the motion, passed some remarks on Zöllner's theory of comets. It reminded him forcibly of a discussion which took place at a meeting of workmen at Millwall. One of them brought forward a glass tube, drew an iron wire through it, and laid it down, and after a few minutes the glass tube cracked. This is a curious phenomena, and not easily explained. A discussion took place as to its cause, and many explanations were proposed; but the conclusion finally adopted was that it was electrical. *Omne ignotum pro electrico* expresses the whole foundation of Zöllner's theory.—*Proc. Brit. Assoc., Athenæum, Aug.* 24.

2. *Report on Lunar Objects suspected of Change.* (Read before the British Association by Mr. Birt.)—As the last report dealt with the observations of the spots on the floor of the Crater Plato, from which it appeared that changes within the area of the crater had been in progress during the two years of observation, so the report presented to this meeting dealt with the observations of the streaks and the color of the floor. The principal results of the second discussion appeared to be that changes in the appearance and luminosity of the streaks had been detected, and these changes were of such a character that they could not be referred to changes of illumination, but depended upon some agency connected with the moon itself, while the color of the floor was found to vary as the sun ascended in the lunar heavens, being darkest with the greatest solar altitude. The report was accompanied with curves from which the relation of the sun's altitude to the various degrees of that observed on the floor as of cause and effect was readily deducible. These reports on the appearances of the spots and streaks indicate the strong probability that if further observations are undertaken, definite changes of an interesting character on the moon's surface are likely to be discovered.—*Proc. Brit. Assoc., Athenæum, Aug.* 24.

3 *Aurora Australis.*—The Aurora Australis was visible on the evening of April 11th, but could be observed only for a short time on account of clouds. Between 7.30 and 7.50 P. M. streamers were frequent, some of them extending to near the zenith, but by 8 P. M. all streamers had disappeared, and only a deep red glow reaching an altitude of about 60°—over the usual segment of cloud extending on the horizon from S.S.W. to S.E. to a height of 12°-15°—could be seen. It gradually grew fainter, disappearing altogether at the higher altitude, and extending only about 10° above the cloud-segment. By 8.20 P. M. there was only a grayish-white tint visible, growing rapidly fainter, until it was shut out from view by the sky becoming overcast. At 10 P. M., when the clouds partially cleared away, traces of the same could still be seen. On the evening of the 15th another display of this phenomenon took place, but could only be seen very imperfectly on account of the cloudy state of the weather. It lasted all through the evening, but at no time could any streamers be seen.

Coincident with this phenomenon disturbances in the magnetic elements took place, but comparatively of small extent, the range on the 11th being 40 minutes of arc in declination and 0·0312 of the absolute unit of horizontal force, while those commencing on the 15th were of much smaller range, but are remarkable for their long duration.—*Monthly Rec. Melbourne Obs. for April,* 1872.

4. *Colors of the equatorial bands of Jupiter.*—Dr. Cipoletti, of Florence, has attributed the colors of the equatorial bands of Jupiter to an *auroral* cause, and sustains it by comparing the results of observations with the times of auroral display as manifested on the earth,—these times being the same for all parts of the solar system since they depend on the condition of the sun.

5. *The Object-glass of the Equatoreal of the Allegheny Observatory stolen.* (From a letter to the editors, dated Sept. 11, 1872, by S. P. Langley, Director of the Observatory.)—The Allegheny Observatory was, on the 8th of July last, the sufferer by a burglary, of a kind hitherto nearly unprecedented. It was entered at night, after the Director and his assistant had left the building, and the object-glass of the equatoreal (13 inches in aperture), was removed from the telescope and carried away. No other injury was done, and, except some eye-pieces belonging to the transit, nothing else was taken.

I have reason to believe that the thieves hoped to extort a large reward for the return of the glass, which is of course otherwise valueless to them.

Considering that most of the observatories of the country would be probable sufferers by similar spoliation, if a precedent was set which made it worth while for burglars to repeat the theft elsewhere, it has seemed almost a duty to others to refuse to offer a reward and immunity from punishment as the price of its return.

Desiring that those in charge of such valuable and hitherto little guarded instruments should be warned of a danger, I shall be obliged by your giving publicity to this letter.

6. *Erratum to Prof. Kirkwood's Article on page* 225 *of this volume.*—The fraction $\frac{1}{34648}$, should have been printed $\frac{1}{3464.8}$.

## IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Meeting of the American Association for the Advancement of Science, at Dubuque, Iowa.*—The standing committee having failed in their efforts to make satisfactory arrangements for the meeting of the Association in San Francisco, accepted the invitation of the citizens of Dubuque, and convened in the latter city on the 21st of August, Prof. Asa Gray in the chair. After the address of the retiring President, Dr. Gray, which was one of great interest, he yielded the chair to his successor, Prof. J. Lawrence Smith. The following business was transacted in general session.

Prof. Benjamin Peirce was added to the committee appointed at the Indianapolis meeting to memorialize the General Government in regard to establishing an observatory at some suitable point upon the Rocky Mountains.

A committee was appointed to memorialize the General Government in regard to the desirableness of compiling the results of all the geological surveys of the country, and publishing the same, together with suitable maps. This committee is to consist of all the members who are or have been in charge of State or government surveys.

A resolution was passed heartily endorsing a bill now pending in Congress proposing to appropriate the surplus of the so-called Chinese indemnity fund, amounting to about $450,000, for the education of Americans and Chinese in the language and literature of the respective nations, provided said fund shall be found to equitably remain in the possession of the government of the United States.

A resolution was also passed asking of the War Department the establishment of a Signal Service Station at Dubuque.

The association also by resolution expressed approval and appreciation of the published results of the Geological Survey of Iowa, and appointed a committee to memorialize the legislature of Iowa, asking its continuance and liberal support until completed, under the direction of Dr. C. A. White.

The Committee appointed at the Indianapolis meeting to report whether any revision of the constitution is required in relation to membership and greater discrimination in the acceptance of papers to be read and published, reported that they found no change necessary, but that they did find several violations of the constitution in the above regard to have been of common occurrence, and expressed the belief that a strict adherence to its provisions is of vital importance to the association.

A proposition to establish an entomological sub-section of the association was made, which, in accordance with the constitution, will come up for action at the next meeting.

The following officers for the ensuing year were elected: President, Prof. JOSEPH LOVERING, of Cambridge, Mass. (he having resigned the office of Permanent Secretary); Vice-President, A. H. WORTHEN, of Springfield, Illinois; Permanent Secretary, F. W. PUTNAM, of Salem, Mass.; General Secretary, Dr. C. A. WHITE, of Iowa City, Iowa; Treasurer, W. S. VAUX, of Philadelphia, Pa.

The next meeting was appointed to be held in Portland, Maine, on the 3d Wednesday of August, 1873.

One hundred papers in all were presented; but over twenty of these were not accepted for publication. The following are the titles of those which will probably appear in the volume of Proceedings:

A sketch of the Geology of Iowa; by C. A WHITE.
On the Eastern Limit of Cretaceous Deposits in Iowa; by C. A. WHITE.
On the Ancient Mounds of Dubuque and its Vicinity; by H. T. WOODMAN.
Climatic Changes in the Salt Lake Valley; by P. A. CHADBOURNE.
On the Oviducts of Brachiopods; by E. S. MORSE.
On the Embryology of Terebratulina; by E. S. MORSE.
Observations on living Rhynchonella; by E. S. MORSE.
A Discussion of the Forces of Expansion and Contraction; by J. D. WARNER.

The Relation of the U. S. Coast Survey to the Geological and Topographical Survey of the States; by BENJ. PIERCE.

The Physics of the Mississippi River; by C. G. FORSHY.

On Zoological Barriers, with special reference to South America; by JAMES ORTON.

On Sympathetic Vibrations, as exhibited in ordinary mechanical Vibrations, and the Optical Method of showing them; by JOS. LOVERING.

Apparatus for Electric Measurement, with Rules and Directions for its Practical Application; by L. BRADLEY.

Planetary Motion and Solar Heat; by CHAS. E. PHELPS.

On Soil-Analyses and their Utility; by EUGENE W. HILGARD.

Solar and Lunar Photography; by JOS. WINLOCK. [Communicated by Benj. Pierce.]

Glacial Deposits of Northern Ohio; by JOHN B. PERRY.

On a Compensating Clock Pendulnm; by W. L. COFFINBERRY.

On some ancient Carved Stones from New England; by F. W. PUTNAM.

Diagram of a new Cluster of Stars, with remarks; by THOS. BASNETT.

A new Projection of the Sphere, convenient in many Physical Investigations; by THOS. BASNETT.

Microscopical Demonstrations; by T. C. HILGARD.

Demonstration of Magnetic Apparatus; by T. C. HILGARD.

On the relation between Organic Vigor and Sex; by HENRY HARTSHORNE.

Elephas Mississippiensis: a new species of Fossil Elephant; by J. W. FOSTER.

On some Peculiarities in the Crania and Skeletons of the Mound Builders; by J. W. FOSTER.

On the Production of Spiegeleisen, embodying a paper by Hugh Hartmann; by J. W. FOSTER.

Description of the Printing Chronograph at the Dudley Observatory; by G. W. HOUGH.

Refraction Tables modified and expanded from Bessel's formulæ, to be used without logarithms, computed with the Scheutz Tabulating Engine; by G. W. HOUGH.

On the so-called velocity of the electric current over Telegraph Wires; by G. W. HOUGH.

The use of Automatic Instruments for registering Meteorological Phenomena; by G. W. HOUGH.

The use of Lead in the Sulphate of Copper Battery; by G. W. HOUGH.

On Binary Stars; by D. KIRKWOOD.

Origin of Limestone in the Coal Measures; by E. B. ANDREWS.

Coal and some of its special Uses; by E. B. ANDREWS.

Good Wine, a Social and National Good; by G. C. SWALLOW.

Surface Geology of North-Western Ohio; by N. H. WINCHELL.

Hypsometrical Data of some of the North-Western States; by ALEX. WINCHELL.

Recent Geological Discoveries among the White Mountains in New Hampshire; by C. H. HITCHCOCK.

Explanation of new Geological Map of New Hampshire; by C. H. HITCHCOCK.

The Temperature of the Sun; by H. F. WALLING.

A Chemical Theory of Electricity; by H. F. WALLING.

The force at any point of the Surface of a rotating fluid ellipsoid of three unequal axes, under the action of the gravity of its own particles and the accompanying centrifugal force; by R. J. ADCOCK.

The Friction of the Progressive motion of Water in the Tide Wave, being less than friction of the return undercurrent upon the bottom, the Tide Wave does not lengthen the day; by J. D. WARNER.

On a new Genus in the Lepidopterous Family *Tineidæ*, with Remarks on the Fructification of Yucca; by C. V. RILEY.

Compressed Air as a Motor; by WM. JORDAN.

The distribution of the Ruby and Sapphire in the United States, with exhibitions of some specimens from Montana; by J. LAWRENCE SMITH.

On the cause of the Mortality of Fish in Racine River; by P. B. HOY.

On the Dynamical condition of the three states of Aggregation; by G. HINRICHS.

Pyrite on the lateral edges of Calcite scalenohedra; by G. HINRICHS.

Simple Arsenic Apparatus for the certain detection of minute traces of Arsenic in toxicological investigations; by G. HINRICHS.

On the Law of Probability as applied to the determination of mental exertions; by G. HINRICHS.

Simple Apparatus for Students for quantitative demonstrations in the Physical Laboratory; by G. HINRICHS.

An account of an Iron Meteorite that was seen to fall in South Africa; by J. LAWRENCE SMITH.

The conversion of the Sulphates of Potash and Soda into Carbonates in the moist way; by J. LAWRENCE SMITH.

On the ultimate Analysis of Coal; by E. T. COX.

Atmospheric Theory of an ameliorated climate and an Open Sea in the Arctic Regions in opposition to the Gulf Stream theory; by W. W. WHEILDON.

A brief statement of effects of the Thunder Storms of July and August of the present year in the vicinity of Boston; by W. W. WHEILDON.

Some Observations in Topographical Geology in North Carolina; by W. C. KERR.

On the gigantic Mammals of the genus Loxolophodon; by E. D. COPE.

On the Eocene genus Synoplotherium; by E. D. COPE.

On the Geological Age of the Coal of Wyoming; by E. D. COPE.

On the so-called sexual characters of Copepoda; by A. H. TUTTLE.

Remarks on the magnifying powers of objectives; by R. H. WARD.

On a Field-stage for clinical Microscopes; by R. H. WARD.

Respiration in Plants; R. KING.

Circulation in Insects; by R. KING.

Organisms in Drinking Water; by H. W. BABCOCK.

Media for the preservation of Entomostroca; by O. S. WESCOTT.

2. *Papers relating to the Transit of Venus in* 1874, *prepared under the direction of the Commission authorized by Congress.* Part I. Washington: Government Printing Office.—Congress appropriated $2,000 in March, 1871, for experiments upon the best form of instruments to be used in the approaching transit of Venus, and constituted a Commission to expend this appropriation and such others as might be made for observing the transit. The present papers are published by this Commission. There are two quite important ones by Mr. Rutherfurd, in which he expresses his opinion that the best form of movable instrument for photographing the transit would be a five-inch objective, with seventy inches focal distance, in a cell allowing of the application, in front of it, of a flint-glass lens of such curves as would shorten the focal distance to sixty inches, and at the same time correct the object-glass for photographic rays. An enlarging camera should be fitted into the tube of the telescope, enlarging the image about four diameters, or to about two inches diameter. This is upon the method successfully employed by Mr. Rutherfurd in his own observatory in New York.

Prof. Newcomb after discussion of the difficulties of the problem, favors the method employed by Prof. Winlock, which is to throw the image of the sun, by a plane revolving mirror, into a fixed telescope of very long focal distance.

We wish that the Commission could have given us another paper detailing the character and the results of the experiments provided for by the first appropriation of Congress. It would seem that the photographs made in these two ways ought before this time to have been compared, and the capabilities of each method for securing the highest degree of accuracy determined. We fear that the experiments have not been made, and that, if not made very soon, the efficiency of all the American photographic observations of the transit will be sadly impaired.

3. *Volcanic Eruption on Hawaii.*—Sandwich Island papers of August 21 and 28, announce that the summit crater of Mt. Loa is again in eruption. A brilliant light is seen at the summit from all sides of the island, and ejections of columns of lava to a height of several hundred feet take place; but a flow of lava down the mountain is not reported.

4. *Tidal wave at the Sandwich Islands.*—An unusual tidal wave took place at these islands on August 23, at 12 o'clock noon. At Honolulu from $12^h$ to $1\frac{1}{2}^h$ there were five distinct waves of diminishing height, ranging from 12 to 15 minutes. Captain Williams of the British ketch Ino, reports that on August 18, in 18° 55′ N. and 159° W., the sea for twenty-four hours was violently breaking and boiling as if on a bar or reef.—*Honolulu Gazette.*

5. *A General Index to the Contents of Fourteen Popular Treatises on Natural Philosophy*, for the use of Students, Teachers and Artizans, by a Massachusetts Teacher. 108 pp., 8vo, New York, 1872 (Ivison, Blakeman, Taylor & Co.).—This index will be found very useful to all students in physics, and those interested in physical questions. The fourteen treatises here indexed include works of Tyndall, Golding Bird, W. A. Miller, J. Muller, J. P. Cooke, and other valuable treatises.

6. *Pompeii.*—It is only a run of forty-five minutes by rail [from Resina] to Pompeii, so I determined to have another look at it; and a most enjoyable excursion it was. It was the "free day," when the public are permitted to enter without paying a fee, and without being accompanied by a guide—a nuisance from which we were most grateful to be liberated. There are at present 180 men employed in the work of excavation, somewhere outside the Temple of Venus; but as they are breaking new ground of considerable depth, much has to be cleared away before any thing can be discovered. In the general aspect of the old city no changes are, of course, observable; but everywhere I marked the judicious care and attention which have been bestowed by the director, the Commendatore Fiorelli, in preserving the ruins, and rendering a visit one of instruction as well as of enjoyment. The bodies, or forms of bodies, in the museum, held together or filled up by plaster of Paris, after the ingenious design of the Commendatore Fiorelli, had a more than usual interest for me. Their discovery and preparation is an old story now, for I was present at their disinterment and preparation "a long time ago," and sent a detailed report of all to the *Athenæum;* but I repeat they had an especial interest for me now, for they were a lively and painful representation of the sufferings lately inflicted by the same agency. To one of the bodies still adheres a portion of its dress, and in April last

many victims in their agony prayed to have their clothes removed; but it was found to be impossible to do so without flaying them alive. Another body, that of a female, lies apparently with a handkerchief at her nose, reminding me of that terrible 28th of April, when even in the streets of Naples it was impossible to walk without sheltering eyes, nose, and mouth; and when, after gulping a quantity of dust, it was necessary to make a bolt for our houses. The same agency was at work on both occasions, and it is painfully illustrated by the bodies in the museum of Pompeii. —H. W., *Athenæum*, July 20.

3. *Sea-Serpents.*—Regarding "sea-serpents," the following note may be interesting:—The South African Museum, Cape Town, recently received a specimen of the Ribbon fish (*Gymnoterus*) fifteen feet long without the tail. It appears that this fish is known to distant inland fishermen as being forty feet long, and from its slender shape and snake-like movement is probably the "sea-serpent" of late years so minutely described by navigators. From its head there is erected a plume of rose-colored spines, and from head to tail along its back there is a conspicuous mane-like fin. Its general color is like burnished silver. The eye is large and silvery, and the profile of the head comports well with that of the horse. The specimen could not be preserved, but there are two smaller specimens in the Museum.—*Nature*, Aug. 1.

4. *Bass culture in England.*—We learn from the *Field* of July 20, that Mr. Parnaby has succeeded in bringing sixty black bass fry home from America, and that they are safely deposited in the tanks at Troutdale, Keswick, and are feeding heartily, so that they may be considered safe. He found great difficulty in collecting the fry and bringing them safely across the Atlantic on account of the intense heat. Mr. Francis considers this the second greatest feat in pisciculture, the first being the conveyance of salmon to Australia.—*Nature*, Aug. 1.

5. *British Association.*—The meeting of the British Association for 1873 will be held at Bradford, under the presidency of Mr. J. P. Joule. The want of space prevents the reproduction in this Journal of the address of the President, Dr. Carpenter, at the recent meeting of the Association, and abstracts of the papers. These will be found in the numbers of *Nature*, commencing with that for August 15, to be had of Macmillan & Co., New York.

### OBITUARY.

Sir ANDREW SMITH, the author of "Illustrations of the Zoology of South Africa," has recently died at the age of seventy-five.

DELAUNAY, the astronomer and physicist, director of the Observatory at Paris, died on the 4th of August, at Cherbourg, by the upsetting of a boat, while in an excursion on the coast of Normandy. He was born in August, 1816.

# APPENDIX.

## *Euclid's doctrine of Parallels demonstrated.*

By Alex'r C. Twining, LL.D.

"The number of demonstrations proposed on the subject of Parallel Lines is evidence of the anxiety felt by geometrical writers upon the subject." With this remark the author of "Geometry without Axioms"—a work published in 1834 in England*—concludes his extended and critical review of some thirty attempts made successively by Ptolemy the astronomer, by Proclus, Clavius, D'Alembert, Boscovich, Simpson, Playfair, Legendre, and other celebrated or approved authors—not less than twenty in number—to demonstrate Euclid's assumed axiom. Such persistence in abortive endeavors, continued through centuries, marks an inherent reluctance of scientific minds to confide in any tests and confirmations by experience merely for the best attainable proof of those elementary truths which, it is felt, ought to be based upon pure intellection.

The demonstration which follows is changed in form, although not essentially in its principle, from the same as presented by the writer of this at the Salem meeting of the American Association for the Advancement of Science, and which, accordingly, appears in the published proceedings of that body for 1869. Also, having in view a merely imaginary introduction of this proof into his Elements, new Corollaries to three of Euclid's Propositions are here supplied, and also a brief intercalary Proposition, which is thought of sufficient value—and the Corollaries as well—to justify, on the whole, their introduction into a Book of Geometry, independently of the use here made of them. After the example of Playfair and others, the intercalary Propositions are lettered and numbered in a manner to designate their proper place and order in the Book (I.) for which they are thus prepared, as follows:—

### Euclid's Elements, Book I.

Prop. IV. Cor. If two incomplete figures have their sides equal, each to each in the same order, and likewise the contained angles equal, each to each in the same order, then the two sides drawn to complete the figures shall be equal, and the figures shall be equal and alike in every respect.

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* By T. Perronet Thompson, Queen's Col., Cambridge.

PROP. XIX. COR. In a right angled triangle the side subtending the right angle is greater than either of the sides containing the right angle.

For (17. 1.) the right angle must be greater than either of the angles opposite to it, and therefore must subtend a greater side.

PROP. XXIV. COR. In two triangles having unequal angles contained by sides equal each to each, the angle opposite the smaller contained angle and subtended by that one of the containing sides which is not less than the other, is greater than the similarly subtended side of the other triangle.

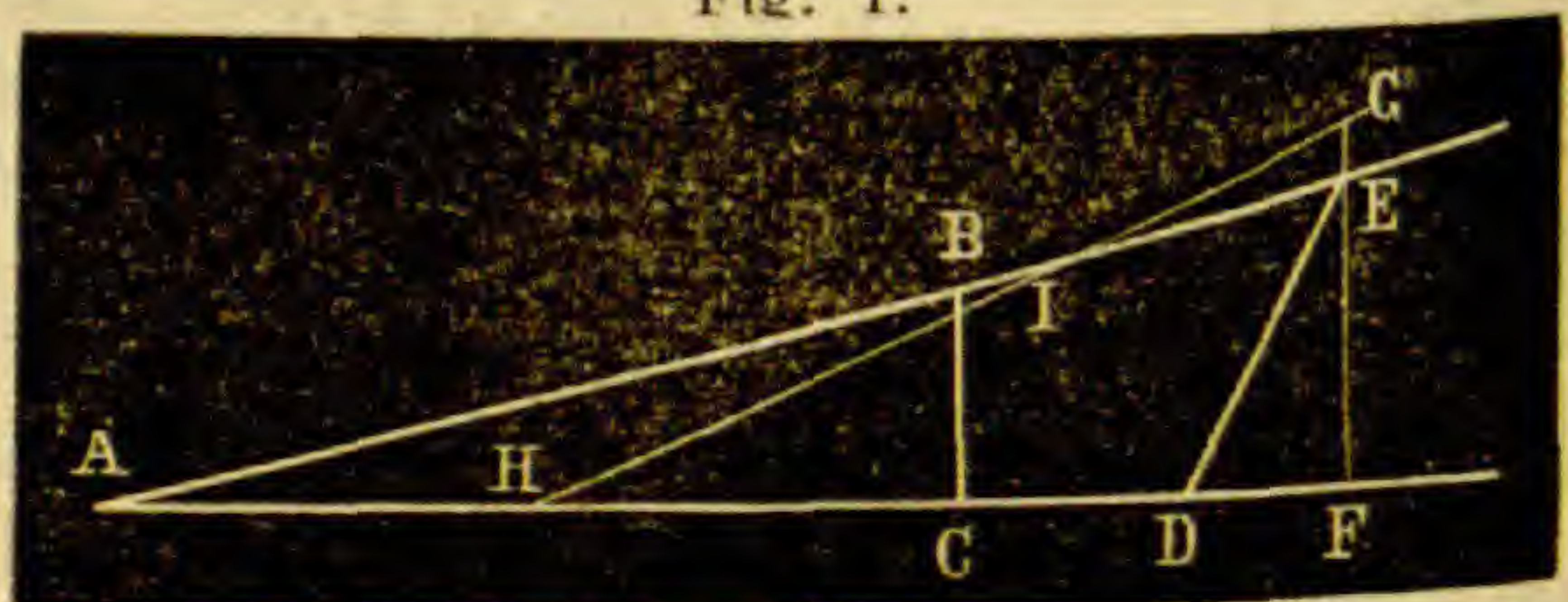
For D E F is greater than D E G or A B C, which has the larger included angle.

### PROP. XXI A. THEOREM.

*If two straight lines intersect, then any third line from one to the other is greater than a perpendicular dropped from any point between the third line and the point of intersection.*

Let the straight lines A E, A F, intersect in A, and let B C be perpendicular to A F, and A E be longer than A B. Then any line E D is longer than B C.

Fig. 1.



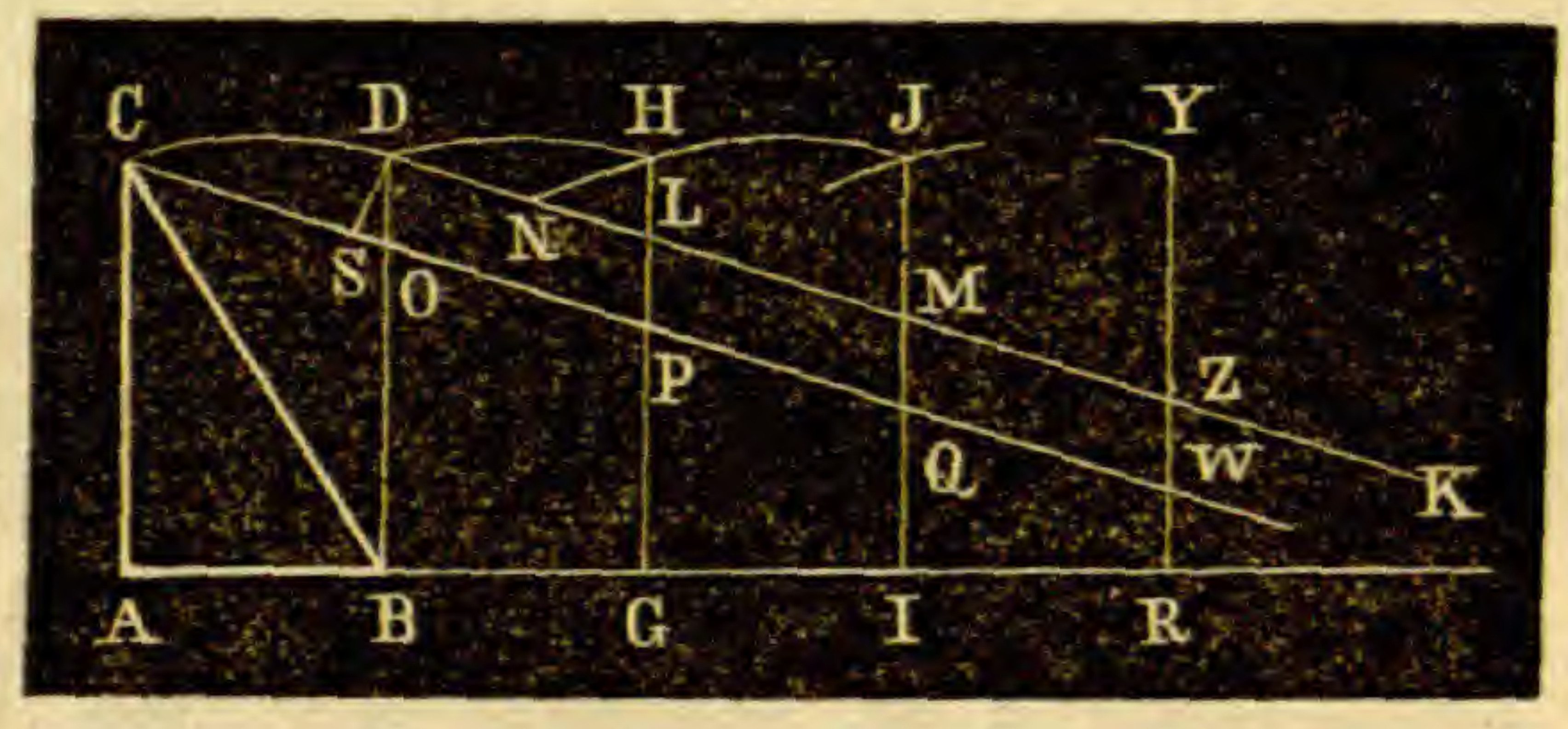
For suppose E D not to be greater than B C; and if it is not perpendicular to A F, let the perpendicular E F be dropped. Then, in the right angled triangle E F D, E F is less (19.1. Cor.) than E D, and therefore less than B C. Produce F E to G, making F G equal to B C. Take F H equal to C A, and join G H, crossing A E in I. Because G F, F H, equal B C, C A, each to each, and contain respectively right angles, the angle G H F or I H F is equal (4.1.) to B A C or I A H,—which is impossible, because (16.1.) the exterior angle I H F must exceed the interior opposite angle of the triangle I A H. Therefore E D must be greater, and E F cannot be less than B C. Neither can E F be equal to B C, for then the point G would coincide with E, and also the point I with the same, and the angle E H F would be proved equal to its interior opposite angle E A H, which is impossible. Therefore E F can neither be less than B C nor equal to it, but must be greater; and much more must any other line E D from E be greater.

## PROP. XXVIII A. THEOREM.

*No triangle can have the sum of its angles greater than two right angles.*

Let A B C be a triangle. The sum of its angles at A, B, C, cannot exceed two right angles.

Fig. 2.



First, let the triangle be right angled at A. From B erect B D perpendicular to A B, and join C D. Suppose the angles at B and C together to exceed a right angle. Then because A B C and B C A exceed a right angle, but A B C and C B D together only equal a right angle, A C B, contained by the sides A C, C B, is greater than C B D contained by the sides D B, B C, equal to the others each to each; and, of these, B C is the greater because it subtends the right angle at A (19.1. Cor.). Therefore (24.1. Cor.) the angle B D C is greater than the right angle at A. And because the three sides C A, A B, BD, containing the right angles A, B, are equal to the same, taken in the order D B, B A, A C (4.1. Cor.), and the right angles equal in the order B, A, the angle A C D equals the angle B D C, and is therefore greater than a right angle. And in like manner may it be shown by taking, in A D produced to R, the bases B G, G I, I R, to any desired number, each equal to A B, and erecting the perpendiculars G H, I J, R Y, and so on, each equal to A C or B D, that the figures B H, G J, I Y, and so on, are each equal and alike in every respect to the figure A D. Complete the figures by joining D H, H J, J Y, and so on.

Produce C D indefinitely to K.* Because C D B is greater than a right angle, its adjacent angle B D K (13.1.) is less than a right angle, and much more, less than B D H, and must fall within it, so that C D produced will cut G H in some point L, making G L less than G H and than its equal A C. So also, for the like reasons, J H produced toward D B will cut D L in some point N; and N L or C L produced will cut J I in some point M, making I M less than I J or its equal A C. And in like manner may it be shown by producing Y J toward G H that C D produced cuts R Y, making R Z less than A C; and so on for every perpendicular drawn as above described. Take in B D, G H, and so on, B O equal to G L, G P equal to I M, I Q equal to R Z, and so on. Join C O, O P, P Q, &c. Because C A, A B, B O, equal D B, B G, G L, each to each, and the included

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* It cannot meet A B produced (17.1).

angles are right angles, the angles A C O, B D L, are equal (4.1. Cor.), and C O B, D L G, are equal. And in like manner it may be shown that B O P equals G L M, also that G P Q equals I M Z, and so on,—also that O P G equals L M I, and P Q I equals M Z R, and so on. But D L G and G L M are together equal to two right angles,—consequently their equals C O B and B O P are the same, and (14.1.) C O P is one straight line—or the line C O produced passes through P. In like manner may it be shown that C O produced passes through Q, W, and so on indefinitely. The line D K can never meet B R* (17.1.), since the angles of the two with A C exceed two right angles. Consequently there can never cease to be a distance R Z, nor, on the same side of A R, an equal distance I Q, through the extremity of which C O produced shall pass. Therefore C O can never meet A R. Because A C less the segment D O equals B O, and B O less L P equals P G, and so on, it follows that any perpendicular, as R W, equals A C less the sum of all the segments D O, L P, M Q, Z W; any number *m* of which cannot together equal A C, for otherwise C O produced could meet A R produced, which is impossible. Drop D S perpendicular to C W, and let *m* be such a number that D S taken *m* times shall exceed A C. Then among that number *m* of the segments there must extend from some point in C D produced at least one—as Z W—which is less than D S,—which is impossible (21 A. 1.). Therefore no right angled triangle can have its oblique angles A B C, A C B, together greater than a right angle: and, because any triangle, as C O D, can be divided into two right angled triangles C S D, D S O, of which the oblique angles D C S, C D S, of one triangle, and the like, S D O, D O S, of the other triangle together make up the three angles of the triangle C O D, those angles can never have their sum greater than two right angles.

Cor. If two perpendiculars A C, B D, to a given base A B, have their extremities joined, the angles A C D, C D B, at the extremities of the perpendiculars shall be equal to one another.

### Prop. XXVIII b. Theorem.

*Through a given point there can be but one parallel to a given straight line.*

Let A B be the given line, and C a given point. Drop C A perpendicular to A B, and through C draw the straight line C R at right angles to A C; then C R is the only parallel to A B through C.

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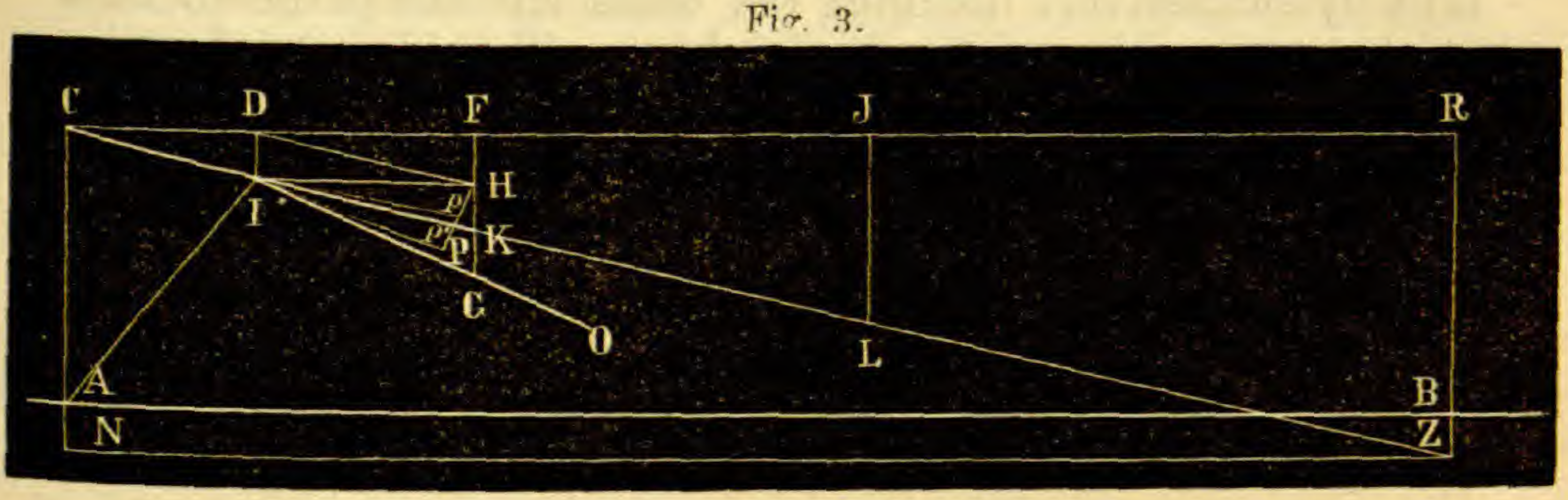
* As above assumed. The demonstration of this proposition in the Proceedings of the American Association, before alluded to, is by a different proess.

For take any line C O making an acute angle A C O with A C. From any point G in the line drop G F perpendicular to C R. Bisect C F in D, and erect D I perpendicular to C R and meeting C O in I. In F G, take F H, H K, each equal to

Fig. 3.



D I, and join D H, I H, I K. Because the sides C D, D I, are equal to the two D F, F H, and the included angles right angles the triangles C I D, D H F, are equal and alike in every respect (4.1.), and the angle C I D equal to D H F. But D I O is adjacent to D I C at the point I in the straight line C O, and similarly D H G is adjacent to D H F,—consequently (13.1.) D I O and D H G are equal angles.

The angles D C I, C I D, of the triangle, right angled at D, must together (28A. 1.), either be equal to or less than a right angle: First, suppose them to be equal to a right angle. Then, because either I D H or D H F together with F D H makes a right angle, those first two angles are equal, and they are also contained by sides D I, D H, and H F, H D, which are equal each to each; wherefore the triangles D I H, D F H, are equal (4.1.), and alike in every respect. Also (28A. 1. Cor.) from the equality of the perpendiculars D I, F H the angle D I H equals I H F, and each is a right angle because equal to D F H, and therefore (13.1.) I H G is a right angle. Since therefore the two sides D I, I H, of one triangle equal the two H K, H I of another triangle, each to each, and the contained angles, are right angles, the triangle H I K is equal and alike in every respect to I H D, and also, therefore, to D C I. Therefore the angles H I K, D I C together equal a right angle, and the three angles C I D, D I H, H I K together equal two right angles. Therefore (14.1.) C I, I K make one and the same straight line. Also the two angles F C K and F K C of the right angled triangle C F K are together equal to a right angle. Therefore it has been shown that if the oblique angles of the right angled triangle C D I are together equal to one right angle, then C I produced will pass through the point K, making a right angled triangle C F K, which, equally with C D I, has its oblique angles together equal to a right angle. Produce C F to J, making

F J equal C F, and erect a perpendicular J L double of F K. It may be proved, in like manner, that C K produced will pass through L, and form the right angled triangle C J L, whose oblique angles are together equal to a right angle. Proceeding thus by successively doubling the bases and the perpendiculars at the extremities of the bases, there will at last be found a perpendicular, as R Z, greater than C A, through the extremity Z of which C I if produced far enough must pass, and form with C R, R Z, a right angled triangle whose oblique angles at C and Z make together a right angle. Produce C A to N making C N equal to R Z, and join N Z. In like manner, as it was shown that the triangles D F H, D I H, are equal and alike in every respect, the same may be shown of the triangles C R Z, C N Z, so that the angle C N Z, being equal to C R Z, is a right angle. Consequently (28.1.) the two lines A B, N Z, making with C N the two interior angles each a right angle, cannot meet, and Z, equally with N, being on the other side of A B from C, the line C I produced has met A B and crossed it, and must so meet and cross in case the angles of C D I are together two right angles as first supposed. But, if C I produced does *not* pass, as above, through K, then the angles of C D I cannot be equal to two right angles, and therefore they must be less, since (28 A, 1) they cannot be greater.

Suppose then that the angles of C D I are together less than two right angles. Then, the construction and proof remaining as before, the angles of the triangle D F H are less than two right angles, and since the angles of D I H cannot be greater than two right angles, the four angles of the quadrilateral I F are less than four right angles, and each of the equal angles at I and H is less than a right angle. Therefore I H K is greater than the angle I H F or its equal H I D. Draw H P equal to I D, or H K, at the angle I H P equal to H I D, which is less than a right angle—and therefore I H P falls within the angle I H K. Join I P; then (4. 1.) the angle H P I equals H D I, and is accordingly acute. It may be supposed either that P shall fall within the triangle I H K, as at $p$, or else in the side I K, as at $p'$, or otherwise outside, as at P. Supposing it at $p$, let H$p$ be produced to meet I K in $p'$. Then H$p'$ is greater than H$p$ or its equal H K, and consequently the angle H$p'$K is less than H K$p'$ (19.1) being subtended by the less side. But H K I is acute (17.1) because I H K is obtuse. Much more, then, is H$p'$K acute and its adjacent angle H$p'$I obtuse; and yet more is the exterior angle (16.1) H$p$I obtuse—which is contrary to the construction, as already proved. Therefore P does not fall within I H K; and, similarly, it cannot fall in I K, as at $p'$. Therefore P must fall outside, and make the angle H I P greater than H I K. But because, by what has been shown, D I G less

D I H, or the angle H I G, is greater than D H G less I H G, or the angle D H I, or its equal H I P, by construction, the angle H I G is greater than H I P, and much more greater than H I K. Therefore the line C I produced to G makes H G greater than H K, if the angles of C D I are together less than two right angles—that is, equally, makes H G greater than H K, if C I produced does not pass through K. But if C I produced passes through K, then is H G by supposition *not greater* than H K, and the angles of C D I accordingly are *not less than but* (28 A. I) *equal to* two right angles, and C I meets A B.

Join I A. It has been shown that if C I produced meets K, or makes with I A the angle A I K, *it must meet* A B, and also that if it does not meet K it makes with I A an angle I A G within I A K, and therefore *much more must it meet* A B. Therefore C I produced must meet A B, whether it does or does not pass through K—that is, *it must meet* A B. And the same may be proved on the other side of A C. Therefore C R is the only line through C which cannot be produced to meet AB.

### *Remarks.*

1*st.* The two principal propositions of the foregoing demonstration are, no doubt, too difficult for beginners. That fact, however, does not militate against the propriety and importance of supplying to the more advanced student, if it can be done, irrefragable proof of what he may, in the outset, have been obliged to take for granted.

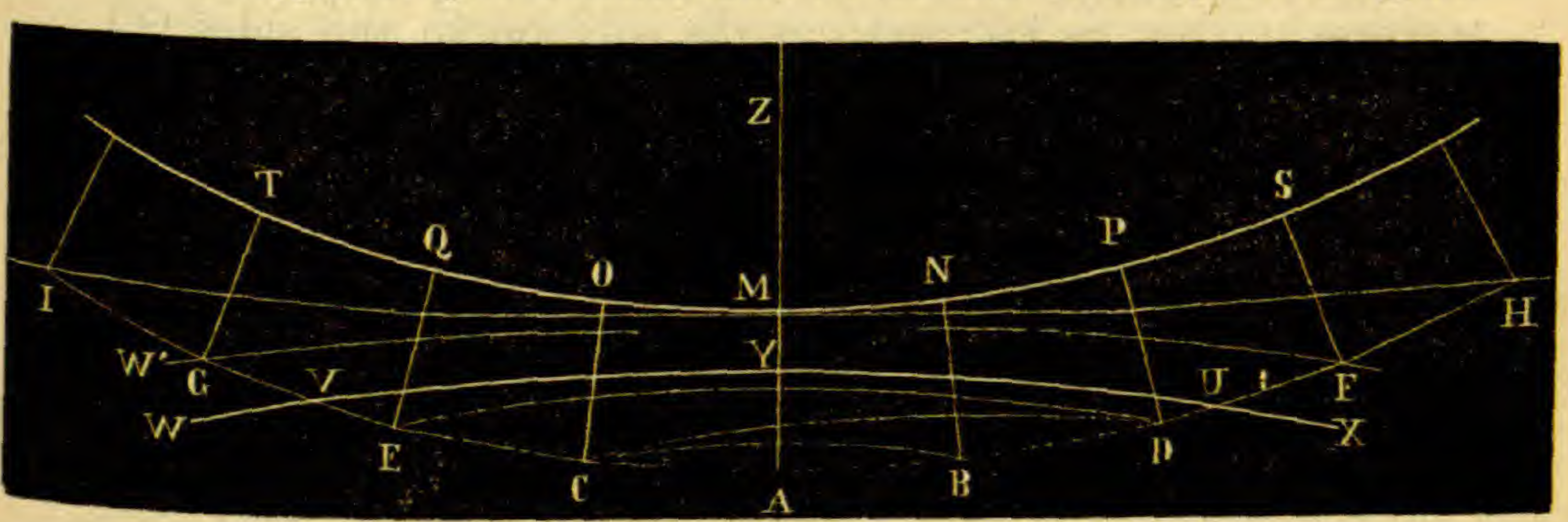
2*d.* Again, while succinctness and simplicity of proof are, in themselves, prime qualities, they do not of necessity counterbalance the advantage of a system like Euclid's, preëminent in its power of disciplining the mental habits, and cultivating a capacity of accurate and penetrative thinking. Thus, also, Legendre's proposed demonstration that the three angles of any triangle are together equal to two right angles—which depends upon the construction of a vanishing series of triangles, each equal to the given triangle in the sum of its angles—although of more than average difficulty, was not on that account refused by its author a place in due order among his elementary propositions; and, though unsatisfactory in its concluding inference, and therefore omitted in subsequent editions, it will ever remain worthy of preservation and of study by reason of the beauty and skill of its conception and conduct.

3*d.* It is quite otherwise, however, with the so-called analytic or *functional* proof by the same author, which has been made the subject of earnest controversy. This, it is familiarly known, depended upon the consideration that in any given triangle the given base and the given angles at the base determine the third angle. Therefore, it was argued, this third angle is a function of the two angles and the side—but a function into which the side cannot in fact enter, because a *line* cannot enter into the composition of an *angle*,—on which account the two angles alone determine the third—which must always be the same, therefore, when the two angles are the same, whatever the side may be. The geometer Leslie's objection to this, that the same reasoning would equally prove that, two sides being given, the third side would be determined by them as a function independent of the included angle, was answered, indeed, but not met, because answered only by ideas, derived from an advanced geometry, itself dependent upon the very doctrine desired to be proved. But there is, besides, in that method, a two-fold assumption of which, apparently, neither Legendre nor his critics were aware. In the first place, it is taken for granted that, because the third angle in the assumed triangle is a certain unknown function of the two angles at its base, and because in any other triangle having the same angles at the base the third angle would likewise be a certain unknown function of those angles, therefore both functions must be one and the same for both triangles, not only in form but likewise in the constants entering them. And a second gross assumption is that those angles must, with their containing lines, when applied to bases greater than the assumed base, be capable of producing a triangle —in other words, that those lines must meet in a third angular point, whatever the bases, because they so meet with the assumed base. On this assumption the entire doctrine would follow apace from the ordinary rudiments of geometry.

*4th.* Mr. T. P. Thompson, the author before alluded to, has himself introduced into his above-mentioned modification of Euclid's first book a proposed proof of the doctrine of parallels. It is too cumbrous for use, even were it valid; for it is a series of seven intercalary Propositions and five Corollaries, covering sixteen octavo pages. This is, however, a minor objection—besides that it might be obviated, as will appear further on. The grave objection lies to the statement under this author's Caption xxviii E, p. 95, in the words, "Let then a straight line W X, of unlimited length both ways, travel along the axis from the vertex A toward Z, till it cuts the axis in M; and it has been shown (28 D, 1 Cor.) that during such travel it cannot cease to cut the series, &c." The fatal objection is that W X is so restricted by the conditions of its cutting that, although ever approaching the point M, it cannot be proved capable of reaching it. The grounds of this objection will be made yet more clear in the recasting of Mr. Thompson's proof, which follows.

### *Thompson's Proof transformed and simplified.*

In our here abbreviated process, Mr. Thompson's chain of proof will be presented unbroken (and in some parts even *à fortiori*) but in propositions involving altogether not one-third the bulk and labor of Mr. Thompson's own method. This statement includes our antecedent demonstration (p. 3) that *no triangle can have the sum of its angles greater than two right angles;* but, aside from this, the proof is reduced in compass to one-tenth. It will be the purport of this our transformed process—equally as it was of Mr. Thompson's process—to show that *no triangle can have the sum of its angles less than two right angles.*



For—looking back to the figure of our xxviii A—if A B C, right angled at A, is supposed to be such a triangle, and the quadrilateral (or *tessera*—so called by Thompson) be constructed as before, then, since the angles of B C D cannot exceed two right angles, the four angles of the tessera are less than four right angles, and the equal angles A C D, B D C, are each less than a right angle. Construct, as below, Thompson's figure under his Caption xxviii E—and with the same designating letters—in which Q M P is a straight line of bases on which the tesseras A N, B P, &c., on one side, and A O, C Q, &c., on the other, are constructed and continued indefinitely, all equal and alike in every respect, and having their angles opposite their bases each less than a right angle. The angles A B D, C A B, &c., are each less than two right angles, wherefore I E A D H is a polygon. Join B C, C D, D E; then A B C, = A C B, must be less than A C D, = B D C, even were A B D one line, and much more, as easily shown, for the angle at B. But B D E, = C E D, is greater than B D C, and much more than A C B. In like manner may it be shown by joining E F, F G, that the equal *cusps* E G F, D F G are greater than the cusps C E D, B D E. In the same manner, also, if A Z is an axis perpendicular to B C—and easily shown to be normal also to E D, G F—it may be proved that if an indefinite line W X moves from A toward Z, keeping at right angles to the axis, it shall make the cusps formed by it at I and H greater than the preceding cusps at G and F, and so on indefinitely. Again, the perpendiculars C O, E Q, &c., and B N, D P, &c., to the straight line Q P, can never meet each other or the axis,—consequently the cusps must each, as E G F, D F G, be less than the angles E G T, D F S, respectively (that is, than half either of the equal angles of the equiangular and equilateral polygon I G E C A B D F H, &c.); for, if otherwise, G F would be one and the same straight line which cuts another straight line T S in two points T and S, or beyond them.

Again, let W X cut the axis at any intermediate position, as Y, within the tessera E D F G. It cannot cut the line E D or G F, since all three are normal to A Z. Therefore it cuts the sides E G, D F, of that tessera in V, U, making angles or cusps V U D, U V E, which cannot be less than E G F, D F G, respectively, because, if so, the four angles of the tessera E F must exceed four right angles. Therefore the cusps formed at V and U are each greater than a given angle A C B. And because T G F is less than a right angle, T G W′ is greater, and the half angle T G I of the polygon will be within it,—so that W X, after passage through any tessera, as E T, of the entire series, may enter and traverse another, as I T, and cut the polygon in the sides G I, F H. Therefore W X, as it approaches to Q P, can never cease to cut the polygon (and at an angle which can be shown, as above, to be greater than a given angle). Let it move on at right angles to the axis till it reaches M. It will then coincide with the straight line of bases Q P, which therefore will somewhere cut the polygon. That is, the base of some one of the tesseras will cut its side opposite the base—which is impossible; consequently the original supposition is also impossible.

This exhibits with fidelity Mr. Thompson's complete process,—only excluding his supposition that the cusps may at length come to equal or exceed half the constant angle of the polygonal series by proving such supposition itself to be impossible. The exceptional point, as already remarked, is the last step of the process—that of W X moving forward to coincide with M. Indeed, beginning back of that step, and at the close of the one preceding it, the really legitimate conclusion would be deduced as follows: "*But* W X *cannot actually reach* M during this consecutive intersection of the tesseras. For suppose it to advance to M while cutting the polygonal series, as in I, H; then I M, which makes the angle I M Q, must also make an equal vertical angle (15·1) on the opposite side of Q P, instead of H M P on the same side,—which last is impossible."

It would, no doubt, be urged by our author that if W X cuts the polygon when in the position Y, but ceases to cut in the position M, it could not but be that the cutting had ceased or was disrupted at some definite position of W X between the two—which, however, has been proved impossible at any definite tessera,—and especially that no such disruption is supposable between lines that never cease to cross one another at an appreciable angle. The infirmity of all this, however, is sufficiently exposed by the inquiry how it appears that W X will not have traversed and cut the polygonal series *throughout its utmost capacity of extension*, while yet at a limiting position short of M,—even as C O (referring back to the figure under our Caption xxviii B) will have traversed and cut A B throughout its utmost capacity of extension when it has reached the limit C R.

This new exemplification, supplied by our author's own labors, and supplementing his own history of fruitless attempts multiplied on this subject, in spite of failures in the past, only affords new evidence how amply any approved demonstration, now or hereafter, of the doctrine of parallels will be recognized and esteemed as having accomplished a scientific desideratum.

## *Notice of some Remarkable Fossil Mammals;* by O. C. MARSH.

The Museum of Yale College has recently received the remains of several fossil mammals, new to science and of great interest. One of these, which is represented by the entire skull and portions of the skeleton, is nearly related to *Tinoceras*, noticed on the preceding pages (322 and 323) and, like the animals of that genus, has the vertebrae and limb bones very similar to those of the recent proboscidians. The skull, however, presents a most remarkable combination of characters. It is wedge shaped, elongated, and quite narrow, especially in front; and was armed with horns, and huge decurved canine tusks. The top of the skull, moreover, is deeply concave, and has around its lateral and posterior margins an enormous crest. On the frontal bones, above the orbits, and in advance of the lateral crest, there is a pair of small, compressed, osseous elevations, which probably supported a pair of horns. The maxillaries have a pair of very large horn cores, just behind and above the canines. These are directed upward and outward, and their summits are obtuse, and nearly round. They are solid, except at the base, which is perforated by the upper extremity of the canine. Near the anterior margin of the nasals there is still another pair of horn cores, which are near together, and have obliquely compressed summits. The nasal opening was small. The premaxillaries are slender, and without teeth.

The upper canines are greatly elongated, slightly curved, and compressed longitudinally. The lower portion is thin and trenchant. Behind the canine is a long diastema, followed by a series of six small teeth. The molars have their crowns composed of two transverse ridges, separated externally, and meeting at the inner extremities.

The skull measures about 28·5 inches (722·$^{\text{mm}}$·), in length; 8·5 inches (202.$^{\text{mm}}$·), in width over the orbits; 6·75 inches (169·$^{\text{mm}}$·) between the summits of the maxillary horn cores; and 2·5 (38·$^{\text{mm}}$·) between the tops of the nasal cores. The maxillary horn cores are about 3 inches, or 75$^{\text{mm}}$· in height. The canine is 9·25 inches (232·$^{\text{mm}}$·) in length below the jaw, 64·$^{\text{mm}}$·, in longitudinal diameter at base, 25·$^{\text{mm}}$· in transverse diameter. The molar teeth occupy a space of 150·$^{\text{mm}}$·, and the last upper molar has an antero-posterior diameter of 36·$^{\text{mm}}$. The species may be be called *Dinoceras mirabilis.* The present animal was nearly as large as an elephant. The remains now known are from the Eocene of Wyoming.

Another species is indicated by portions of a skull with teeth, and some other fragmentary remains. This specimen differs essentially from *Dinoceras mirabilis*, in the upper molar teeth, the last of the series being proportionally much larger than the corresponding tooth in that species, and having, moreover, a broad floor extending backward between the posterior crest and the basal ridge. The length of the upper molar series of six teeth is 163·$^{\text{mm}}$·, the last true molar being 45·$^{\text{mm}}$· in antero-posterior diameter, and also in transverse diameter. This species, equalled *D. mirabilis* in size, and may be called *Dinoceras lacustris.* The remains are also from the Eocene of Wyoming.

The species of *Dinoceras*, and those of *Tinoceras*, represent a distinct order which may be called *Dinocerea.* A full description of these interesting mammals will be given at an early day.

## *Notice of a New and Remarkable Fossil Bird;* by O. C. MARSH.

One of the most interesting of recent discoveries in Paleontology is the skeleton of a fossil bird, found, during the past summer, in the upper Cretaceous shale of Kansas, by Prof. B. F. Mudge, who has kindly sent the specimen to me for examination. The remains indicate an aquatic bird, about as large as a pigeon, and differing widely from all known birds, in having *biconcave vertebræ.* The cervical, dorsal, and caudal vertebræ preserved all show this character, the ends of the centra resembling those in *Plesiosaurus.* The rest of the skeleton presents no marked deviation from the ordinary avian type. The wings were large in proportion to the posterior extremities. The humerus is 58·6$^{\text{mm}}$· in length, and has the radial crest strongly developed. The femur is small, and has the proximal end compressed transversely. The tibia is slender, and 44·5$^{\text{mm}}$· long. Its distal end is incurved, as in swimming birds, but has no supratendinal bridge. This species may be called *Ichthyornis dispar.* A more complete description will appear in an early number of this Journal.

Yale College, Sept. 26th, 1872.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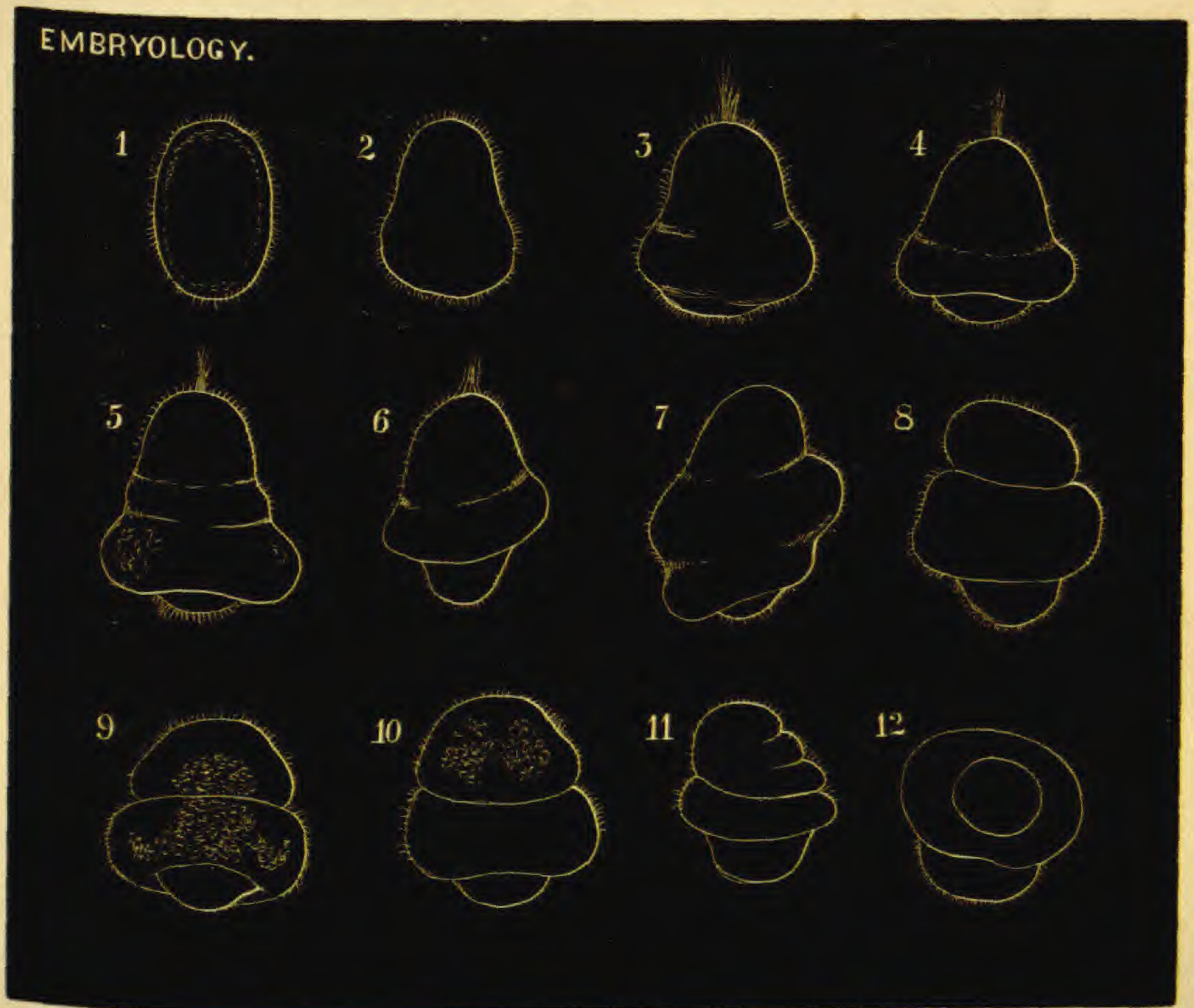
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JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

## ART. XL.—*A Theory of the Formation of the great Features of the Earth's Surface;* by JOSEPH LECONTE, Prof. Geol. and Nat. Hist., University of California.

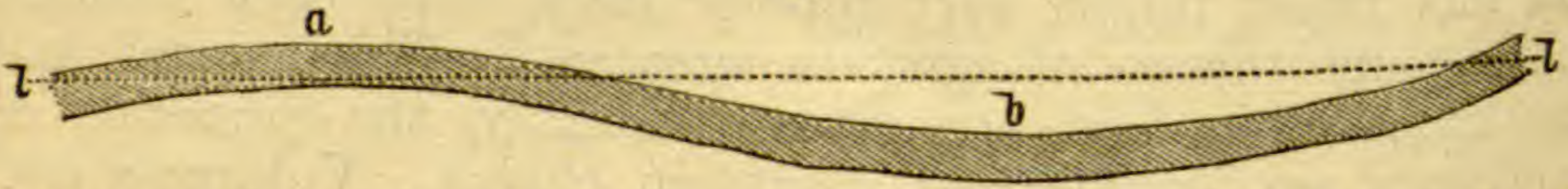
THE connection between the formation of continents and mountain chains on the one hand, and the phenomena of volcanoes and earthquakes on the other, is so evident and close, that this paper might, very appropriately, have been entitled "A general theory of igneous agencies." The general and first effect of igneous agency is, evidently, the formation of continents, sea bottoms and mountain chains. Volcanoes and earthquakes are secondary phenomena—they are but occasional accidents attending the slow march of these grander effects.

According to Humboldt, all the effects grouped under the general head of igneous agency are the result of "*the reaction of the interior on the crust of the earth.*" This formula, although far too vague and general to deserve the name of a theory, must, we believe, form the point of departure of every true theory. But in departing from this vague formula, only the most confused and contradictory notions seem to prevail amongst geologists. We have therefore thought that, on a subject of such vital importance, lying as it does at the very foundation of theoretic geology, any light, or even any more definite statement than now exists, might be considered timely. I have, for many years, thought much on the subject, and striven to emerge from the chaos which now exists into something like clearness of conception. I here present, with some hesitation, the results, hoping that they may serve, at least, as hints in the right direction.

The usual idea in regard to the general constitution of the earth is, that it is even now essentially a liquid, incandescent mass; the fiery liquid being separated from our feet only by a comparatively thin shell of solid matter. This solid shell, being lighter than the subjacent liquid, may be regarded as *floating upon it, and sustained by it.* Vertical upheaval or subsidence of the solid shell is supposed to produce continents and mountain chains on the one hand, or sea-bottoms on the other. Or else (as perhaps many would prefer), the contraction of the liquid interior being greater than that of the solid crust, causes the latter to wrinkle, i. e., to *bend into alternate convex and concave arches*, which form respectively the continents and sea-bottoms. The comparative *thinness* of the solid crust (only 25–50 miles according to these theorists) is supposed to make the process easy and the view plausible. On the contrary, it seems to us that this very supposed thinness of the solid crust renders the formation of continents and sea-bottoms *impossible*, except under very peculiar conditions. These conditions we now proceed to point out.

*Continents and sea-bottoms.*—If we regard the earth as consisting of a solid crust sustained as a floating body upon a liquid, the least reflection suffices to convince us that *the greater inequalities of the surface cannot be produced by alternate convex and concave bendings of the crust* like those shown in fig. 1, in which *a*

1.



is the continental and *b* the oceanic crust, and *ll* the sea level. No such arch as that producing a continent 3000 to 6000 miles across could sustain itself for a single moment: no; not if the crust were 100 or even several hundred miles thick. Still less would it be possible that the inverse arch, or concavity of ocean bottoms *b* (which in the case of the Pacific is 10,000 miles across), should sustain itself. The arch *a* would break *down* and the arch *b*, *up*, and the position of equilibrium *ll* would be assumed. So great is this force tending to the general form of equilibrium that, as shown by Thomson's reasoning referred to farther on, even if the earth as a whole were as rigid as a solid globe of glass, it could not resist it.* The irresistible and only conclusion, therefore, is that continents and sea-bottoms cannot be simple bendings or arches of a solid crust. If there be indeed a solid crust on a liquid interior, in order to sustain itself, the inequalities of the upper surface in contact with the air *must be repeated on the lower surface* in contact with the liquid

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* Phil. Trans., May, 1862.

interior. The accompanying diagram (fig. 2) is an ideal representation of what *must be* the general character of the crust on this view. As before, *a* is the continental and *b* the oceanic crust. For simplicity's sake, in all these diagrams, the crust is represented as spread out on a plane. If we admit that the general constitution of the earth is that of a solid crust covering a liquid interior, I cannot see how the above conclusion can be avoided.

2.

Assuming then a general constitution of the solid crust like that represented in fig. 2, let us see how, on this theory, these inequalities might be formed. To form these, three conditions seem to be necessary. 1. The crust must be *lighter* than the interior liquid, i. e., it must be a floating body. We have assumed this all along, for there could not be a crust otherwise. 2. The material of the crust must *expand in solidifying*, i. e., in becoming crust. 3. Some portions of the crust must cool and thicken faster than others; these more rapidly thickening portions becoming the continents. Under these three conditions we may account for continents and sea-bottoms as follows.

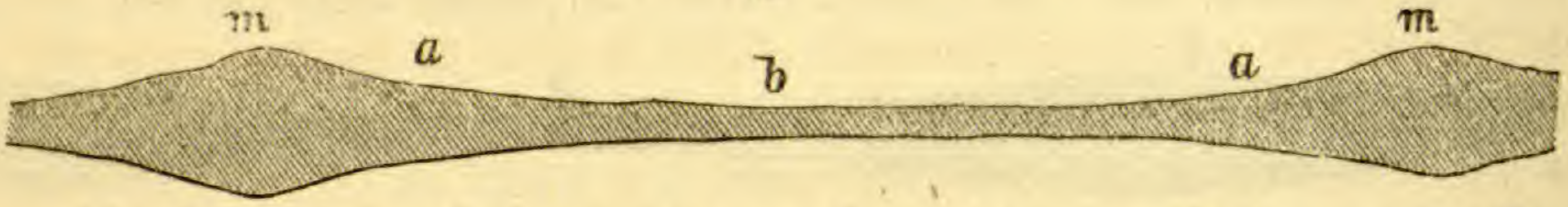
Suppose a liquid earth consisting of heterogeneous materials, covered with a thin crust of solid matter, cooling and the crust thickening everywhere by additions to its lower surface. Evidently the more conductive portions would cool and thicken faster than the less conductive portions. Thus the inequalities would commence on the lower surface, as in fig. 3. But such a condition of things could not continue and increase: for if these areas of thicker and thinner crust be large, by the law of floatation the thicker parts *a a* must rise and the thinner portions *b* sink until equilibrium is established, and the condition of things represented by fig. 2 is assumed. A continuation of the same process, viz: the more rapid thickening of the continental portions *a a*, would cause these to rise higher and higher, and the ocean bottoms to sink deeper and deeper.

3.

Thus, then, by this view, the formation of continents and sea-bottoms, and the increase in height and size of the former, and in depth of the latter, is due to *the unequal thickening of a floating crust by unequal cooling.*

*Mountain chains.*—If mountain chains were only *narrow wrinkles* on the earth's surface, we might suppose it possible that they could sustain themselves as arches. But when we remember that they are in fact great plateaus or bulges a hundred or even a thousand miles wide, it seems impossible to avoid the conclusion that they too, if they rest on a liquid mass beneath, must be sustained by a *similar bulge on the lower surface* of the solid crust. The accompanying diagram (fig 4)

4.



is an ideal section of the crust on this view, *a a* being, as before, the continents, *b* the sea bottom, and *m m* mountain chains. Of course in all the figures the thickness of the crust is enormously exaggerated.

The condition of the solid crust represented by figs. 2 and 4, viz., the existence of inequalities on the lower surface corresponding to the great inequalities of the upper surface, is, we believe, a demonstrably necessary consequence of the interior fluidity theory: there may be difference of opinion as to the *cause* of this condition, but there can be none as to the condition itself, if we admit the interior liquid.

We have given above what seemed on this theory the most probable *cause* of inequalities of crust thickness, viz: unequal surface cooling; but in order to state the interior-fluidity theory fairly, we will give another possible cause. In the gradual cooling of the earth, if the interior liquid should cool and contract faster than the solid shell (as it probably would), then the latter would be subjected to powerful horizontal thrust, to which it must eventually yield, and by which it would be thrown into wrinkles; *not, however, by bending up and down*, as is usually supposed (for this we have already shown is impossible), but *by crushing together and thickening in some places more than others.* By this view *continents and mountain chains and sea-bottoms are due to unequal thickening of the solid crust*, not by unequal cooling, as before supposed, but *by unequal crushing together by lateral pressure.* This mode of viewing has the great advantage of accounting for the foldings of strata so common in mountain chains. But on the other hand, it is difficult, nay impossible, to understand why, on this view, the thicker continental crust (made thicker by yielding), should continue to yield, rather than the thinner oceanic crust. In other words, it is impossible, on this view, to account for the undoubted general tendency of igneous agency to gradually and constantly *increase* the greater inequalities. Their tendency ought to be rather to destroy them as fast as formed, by the yielding and therefore thickening of the thinner portions, viz: the oceanic crust. This difficulty seems to be fatal.

The theory above presented, viz: the formation of continents and sea-bottoms by unequal thickening of a floating crust, by unequal cooling, may, with skillful modification and by adding hypothesis to hypothesis, be made to account for most of the phenomena of igneous agency in a somewhat satisfactory manner. In 1859, at the Springfield meeting of the American Scientific Association, I brought forward in some detail a theory of this kind. I soon became dissatisfied with it, however, as too hypothetical, and therefore never printed it. An abstract of it, however, I believe, appeared in the Canadian Naturalist. Subsequent reflection has induced me not only to reject it, but to doubt more and more the very basis upon which it rests, viz: the interior liquidity of the earth. To all theories based upon the assumption of a liquid interior and a floating crust, there are the following insuperable objections:

*a*. One of the necessary conditions of the formation of continents, &c., by unequal thickening due to unequal cooling, is a lightening of the material in the act of solidification. Now granite and other igneous rocks do not expand in the act of solidifying, but on the contrary, according to the experiments of Bischof, *notably contract.*

*b*. The solid shell of the earth, if there be any such, may be proved to be much thicker than is usually supposed by geologists of the interior-fluidity school. The principal argument for the liquidity of the earth beneath a comparatively thin shell is based upon the increasing temperature of the earth as we go downward into the interior. "The rate of 1° for every 50 feet of descent would give 3000°, the fusing point of iron, at a depth of 28 miles." At this temperature nearly all rocks would melt in our furnaces: therefore many hastily assume that the solid crust cannot be thicker than this. But all the calculations as to the thickness of the solid shell, based upon the rate of increasing interior temperature, neglect two important elements of the calculation, viz: the *increasing conductivity* of the earth as we descend, which would diminish the *rate* of increasing temperature, and the *increasing pressure*, which would elevate the fusing point. I have been accustomed for many years to illustrate the effect of these two distinct causes as follows:

Let S S (fig. 5) be the surface of the earth, and A B depth along any radius. Taking A B as an abciss, let the increasing heat be represented by ordinates. Now, if the density and conductivity of the earth were constant, then the heat would increase at uniform rate, and would therefore be correctly represented by the straight line C D. At the rate of 1° for every 50 feet we would reach the heat ordinate *t t* of 3000° and the melting point of rocks at the depth of 28 miles. But since the density of the earth increases toward the center, it is almost certain that the conductivity increases also. The effect of this would be to diminish the rate of increasing temperature, which would therefore be correctly represented by a curved line C E, and not the straight line C D. Under these conditions the ordinate of 3000° on the curved line would not be reached at 28 miles, but at some point $t'$ farther down, say 40 miles from surface. But having finally reached the temperature of 3000°, which we assume as the fusing point of rocks in our furnaces, we would, by no means, find the rocks in a state of fusion; for the enormous pressure of 40 miles of rock would undoubtedly very greatly elevate the fusing point. How much we know not; but let us suppose to 3500°. To find the ordinate of 3500° on the curve, we must go still deeper to some point $t''$, perhaps 60 or more miles from the surface. But here again we would fail to reach the lower limit of the solid crust, because the fusing point is again elevated above 3500° by the still greater pressure. And thus the fusing point flies before the increasing temperature; and *where* in this chase the former would overtake the latter, or *whether it would ever overtake it at all*, would depend on the rate of increase in the two cases. We have not at present the data to determine these.

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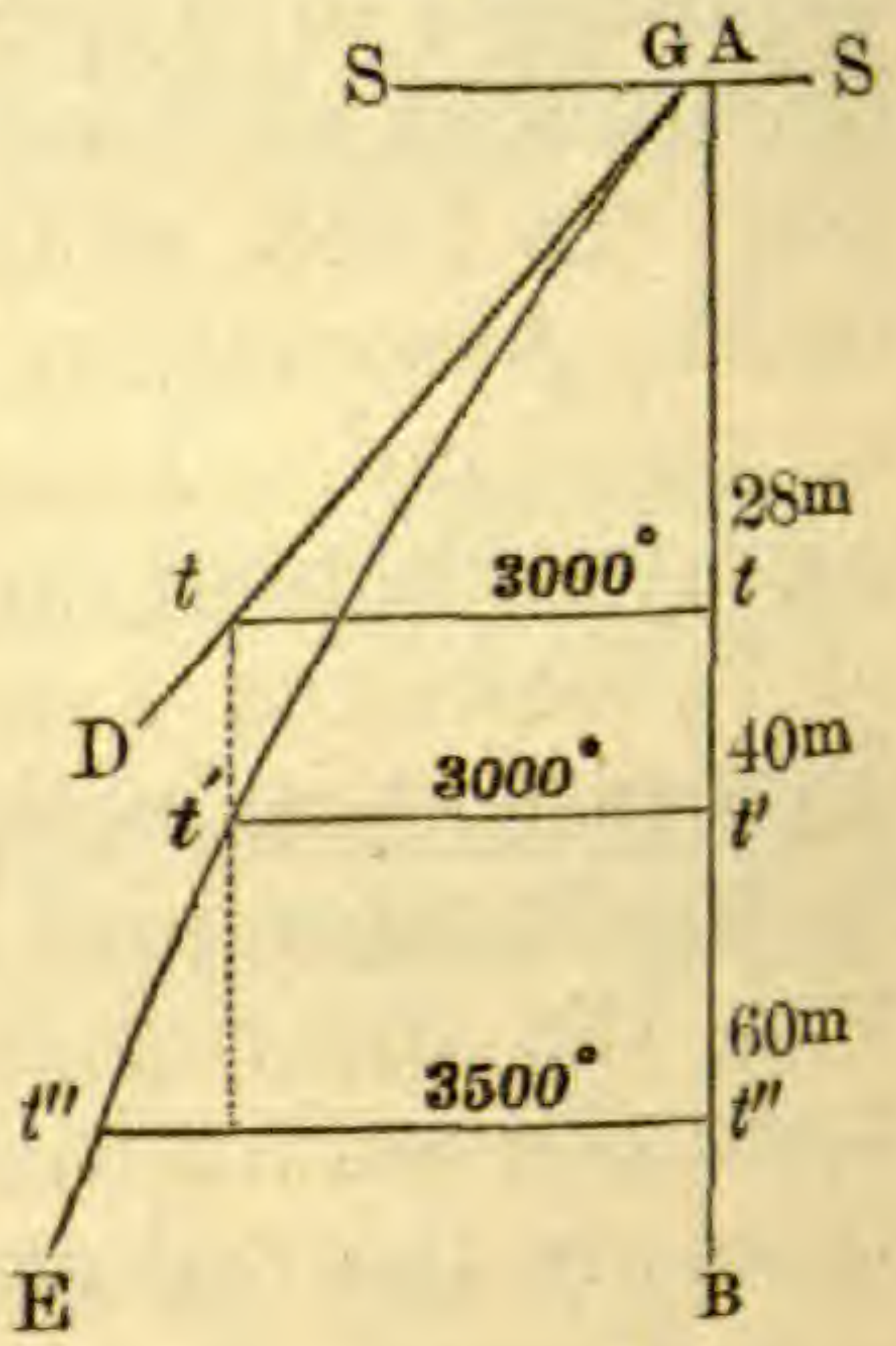

The conclusion from this reasoning is, that the crust is certainly much thicker than is usually supposed; so thick, indeed, that the focus of volcanoes and earthquakes must be *within its thickness;* and it even becomes doubtful whether there be any general fluid interior at all.

*c.* This doubt is entirely confirmed, according to the best physicists and mathematicians, by the effect of the sun and moon on the earth in producing *precession and nutation*, and in producing *tides.* In all these phenomena the earth, even under the most powerful distorting forces, *behaves like a perfectly rigid* solid, and not at all like a liquid or a partly liquid body. The reasoning of Hopkins, based upon the amount of precession and nutation, and his conclusion that the solid shell of the earth cannot be less than 1000 miles thick, is well known. By the majority of physicists and mathematicians it is regarded as sound. Nevertheless, doubts have been thrown upon it by men of high ability. But Thomson's argument, drawn from the behavior of the earth under the tide-generating influences of the sun and moon, is as yet untouched. It seems to be impregnable. The result of Thomson's reasoning is, that the *earth as a whole is certainly more rigid than a globe of solid glass, and probably more rigid than a solid globe of steel.*[*] We have already shown how fatally this extreme rigidity operates on any theory which attributes the inequalities of the earth's surface to bendings of a solid crust.

*d.* Very recently my attention has been directed by my brother, Prof. John LeConte, to another fact bearing strongly in the same direction, viz: the form of the equatorial section of the earth. If the earth were an exact spheroid of revolution, of course the mean surface of the equatorial section would be a perfect circle. Such, however, is not the case; the equatorial section is an ellipse whose major axis is about two miles longer than the minor axis. This, be it observed, is the form of the *equilibrium surface* or water-level; it therefore can be produced only by *unequal distribution of matter*, i. e., irregular density of the interior. Such irregular distribution of dense matter is easy enough to understand in a *solid earth;* for it would be the natural result of unequal cooling, and therefore unequal condensation, after the earth solidified. But it is difficult to understand how such irregular distribution of density could originate or how it could be maintained in a liquid. Humboldt[†] attributes it to *secular currents* in the interior liquid, by which denser matter is slowly transferred from one part to another. To say the least, this is a violent hypothesis, and I think would never have been thought of unless the interior liquidity of the earth was considered certain. Without insisting too far on this objection to the interior liquid, I think I am justified in saying that, on the assumption of a liquid earth, it is difficult to see why the interior density should not be perfectly symmetrically disposed, while on the assumption of a solid earth still cooling, the unsymmetrical disposition of density is almost a necessity.

There is, as far as we know, but one argument of a *general nature*, which has recently been brought forward in favor of the interior liquidity of the earth, viz: the three laws of earthquake occurrence rendered probable by Alexis Perry. The laws are as follows:

1. Earthquakes are more frequent when the moon is on the *meridian* than when she is on the *horizon:* 2. They are a little more frequent at the *syzygies* than at the *quadratures:* 3. They are a little more frequent when the moon is in *perigee* than when she is in *apogee*. If these laws are true, then it is evident that there is a slight tendency for earthquake occurrence to follow the law of *tides*. Many geologists seem to think that

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[*] Phil. Trans., May, 1862. Thomson & Tait, Nat. Phil., p. 689.

[†] Cosmos, vol. iv, p. 19, Sabine's edition.

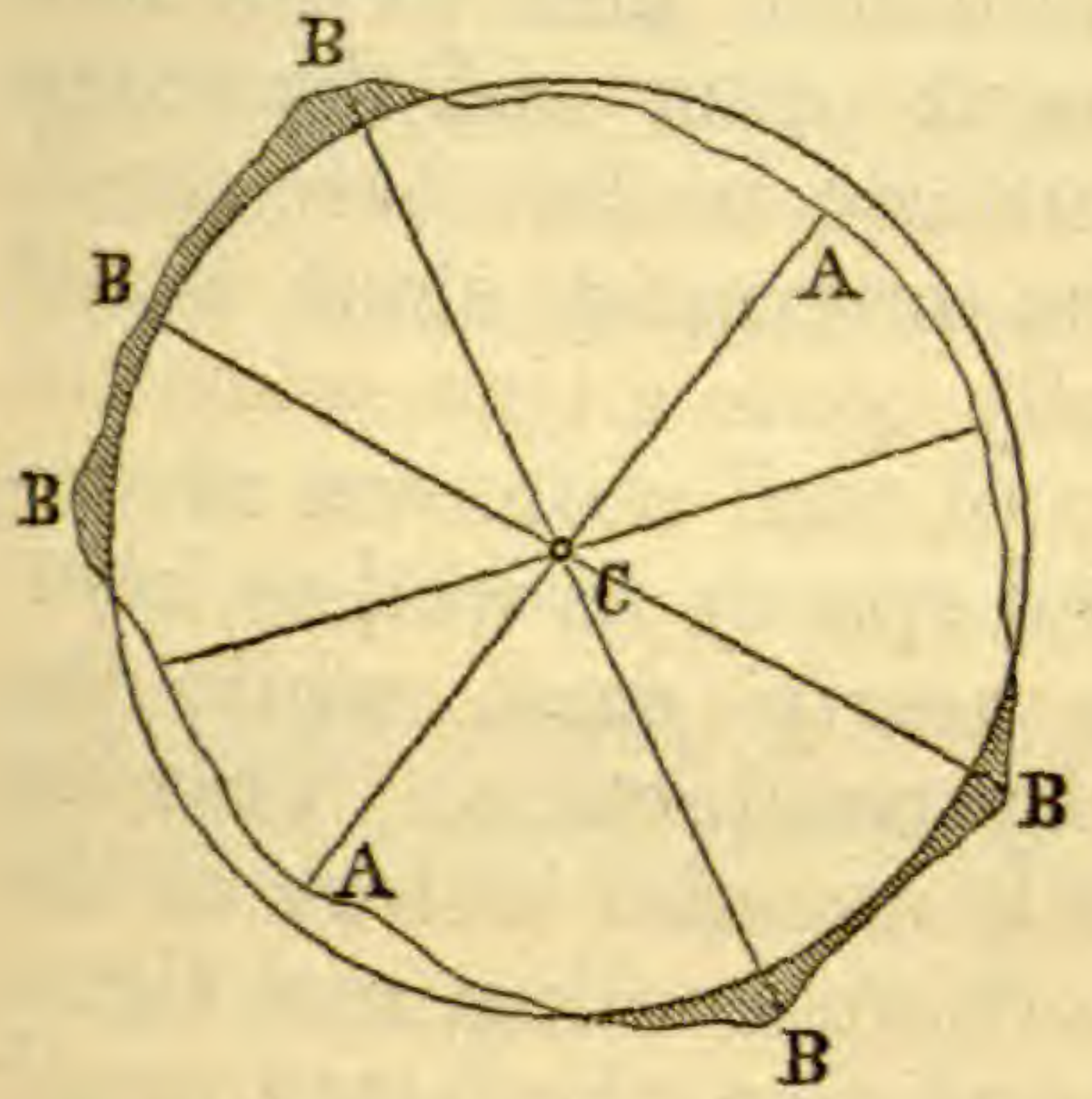
this, if established, is conclusive of the existence of an interior liquid in which tides are generated. On the contrary, it seems to us that, on the supposition of such an interior liquid, the effect of lunation on earthquakes and other igneous phenomena ought to be far more decided than it is. The very slight effect observed is, however, precisely what we would expect in a substantially solid earth. It is evident that in a solid earth, any force, whatever be its nature, tending to lift and break up the surface, would be *assisted by the moon on the meridian* and *repressed by the moon on the horizon;* and that these effects would be most marked at the syzygies and at perigee. Suppose then any force tending to elevate and break up any portion of the earth's surface, but resisted by the weight and the cohesion of the superincumbent strata; is it not evident that if these opposite forces were nearly balanced, the small lifting force produced by the moon in passing over might turn the scale in favor of the upward force, and thus determine the rupture of the strata, and therefore the occurrence of an earthquake?

The foregoing objections have induced many of the best geologists to believe that the earth is either solid or else that the solid crust is so thick that all igneous phenomena have their origin within the limits of that crust: and that therefore the earth may be regarded as *substantially solid.* The liquid ejections of volcanoes, according to this view, must be regarded as having their origin in local accumulations of liquid matter which have no connection with a general fluid interior. I confess, my own sympathies are entirely with this modern school of geology. It seems to me that while much of the prestige of great names, such as Humboldt, Von Buch, Elie de Beaumont, &c., are on one side, most of the substantial argument is on the other. I feel convinced that the whole theory of igneous agencies—which is little less than *the whole foundation of theoretic geology—must be reconstructed on the basis of a solid earth.* What follows is an attempt in this direction.

### 1. *Continents and Sea-bottoms.*

Suppose a solid earth of oblate spheroid form and even surface, covered with a universal ocean and cooling by radiation. If the material were homogeneous and therefore of equal conductivity along every radius, then the cooling and the consequent contraction along each radius would be equal, and, so far as this cause is concerned, the earth, though becoming smaller, would maintain its symmetry of form and its universal ocean. But such homogeneousness could not be expected, nor does it exist. In a heterogeneous earth thus cooling, *areas of* *greater conductivity would* cool more rapidly and therefore *contract more rapidly* in a *radial direction.* These more conductive areas, with shorter radii A C, A C (fig. 6), would form the sea-bottoms, while the less conductive and therefore the less radially contracted portions B B would become land surfaces. The accumulation of water on the shorter or more contracted radii would not check the process; for although water is a bad conductor, yet it conveys heat from bottom to surface by *convection* with great rapidity, and therefore the process of cooling through a stratum of water would go on far more rapidly than through a stratum of any rocky material. The same process therefore continuing, would tend constantly to increase the inequalities thus commenced. In other words, the sea-bottoms would sink and the continents increase in size and height. With many oscillations difficult or impossible at present to account for such has, on the whole, been the progress of change during the whole geological history of the earth.

6.



The law of fluid equilibrium requires that in the pre-existing fluid condition, and therefore also when the earth first became solid, the quantity of matter along each radius was nearly or quite equal. This equality would not be affected by the subsequent unequal contraction. It seems probable, therefore, that the same equality still exists, and that, therefore, the *matter along the shorter oceanic radii is denser* than along the longer continental radii. If, farther, we suppose great mountain plateau-masses also to be formed by unequal radial contraction, then the plateau radii would contain the lightest matter of all. According to Archdeacon Pratt,* the form of the Indian arc indicates unmistakably that continental matter and especially Himalayan matter is less dense than sub-oceanic matter. His view of the origin of the great inequalities of the earth seems to be something like what I have presented, since he states his belief that the absolute quantity of matter along different radii is the same. I have stated this as a *possible* mode of formation of great plateau-masses. As a general fact, however, I account for mountain chains in an entirely different manner, which I now proceed to explain.

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* Phil. Mag., vol. xli, p. 307, 1871.

## 2. *Mountain Chains.*

A cooling earth may be regarded as composed of concentric isothermal shells, each cooling by conduction, and in the case of liquid also by convection to the next outer and the outermost by radiation into space. Furthermore under these conditions, at first and for a long time, the outermost shell would cool fastest; but there would eventually come a time when the surface having become substantially cool, and moreover receiving heat from external sources (sun and space), as well as internal, its temperature would become *nearly fixed*, while the interior would *still* continue to *cool* by conduction. This has probably been the case during the whole *recorded* history of the earth. Now, therefore, the interior portions cooling, and therefore *contracting*, *more rapidly* than the exterior, the latter would be subjected to powerful *horizontal pressure*, which continuing to increase with the progressive interior contraction, the *exterior* portion *must* eventually *yield* somewhere. *Mountain chains are the lines along which the yielding of the surface to horizontal thrust has taken place.* But observe: according to my view, this yielding is not by up-bending into an arch leaving a hollow space beneath, nor such an arch filled and supported by an interior liquid; but *a mashing or crushing together horizontally, like dough or plastic clay, with foldings of the strata, and an up-swelling and thickening of the whole squeezed mass.*

No other view but this will, I think, satisfactorily explain the complex foldings so universal in great mountain chains. We will not stop, however, to discuss this point now, but hasten to give what seems to us nothing less than demonstrative proof of this crushing together and up-swelling of mountain chains. This proof is found in the phenomenon of *slaty cleavage.*

It is now established beyond the possibility of a doubt, by the beautiful observations and experiments of Sharpe, Sorby, Haughton and Tyndall, that *slaty cleavage is produced by* powerful *pressure* perpendicular to the planes of cleavage, by which the whole rock-mass has *been mashed together* and shortened in that direction, and *correspondingly extended* in the direction of the planes of cleavage. As the planes of cleavage are usually highly inclined or even vertical, it is evident that the rock-mass has been crushed together *horizontally* and *swelled up vertically.* As a necessary consequence of this crushing together, we find associated with cleavage the most complex foldings not only of the strata, but of the layers and even of the finest lines of lamination. This *plication is always associated with cleavage*, and, *vice versa*, cleavage, when the rock material is suitable for developing this structure, is always associated with plication; and both are associated with mountain chains.

A mashing together horizontally and an extension vertically, or an up-swelling of the mass, is therefore proved in slaty cleavage, and thus also in mountain chains where slaty cleavage occurs. It only remains to show that the *amount* of mashing in one direction and extension in the other, absolutely proved in these cases, *is fully adequate to account for the upheaval of the greatest mountain chains.*

By the observations of Sorby and Haughton, taking an ideal *cube* of the original *unsqueezed mass*, the ratio of the greatest diameter (in the plane of cleavage), to the least diameter (perpendicular to cleavage), becomes, after squeezing, sometimes 2 : 1, sometimes 4 : 1, 6 : 1, 9 : 1, and even sometimes 11 : 1. According to Sorby, the average is 5 or 6: 1. Now it is evident that of the three rectangular diameters of such a cube, one vertical and two horizontal, one of them, the *horizontal* in the direction of pressure, would be *shortened*, another, the vertical, would be *elongated*, while the third, the horizontal at right angles to pressure, would be *unchanged*, because in the rock-mass yielding could not take place in that direction. It follows then that the *change* of the original diameters in either direction, by compression or elongation, would be the *square roots* of the ratios mentioned above. Thus if a cube of 3 inches diameter be crushed together horizontally, and allowed to extend only vertically, until its previously equal diameters become as 9 : 1, it is evident that the vertical diameter has been increased and the horizontal diameter diminished 3 times. Taking 6 : 1 as the average ratio in cleaved slates of diameters originally equal: we may assert that *in cleaved rocks the whole mass has swelled up* 2½ (2·45) *times its original thickness.* Suppose then a mass of sediments 10,000 feet thick subjected to horizontal pressure and crushing sufficient to develop well-marked cleavage structure; *a breadth of* 2½ *miles would be crushed into* 1 *mile, and* 10,000 *feet thickness would be swelled to* 25,000 *feet, making an actual elevation of the surface of* 15,000 *feet.* Now we actually have strata not only 10,000 but 20,000, and even 40,000 feet thick.

I think, therefore, I am justified in asserting that the *phenomena of plication and of slaty cleavage demonstrate a crushing together horizontally, and an up-swelling of the whole mass of sediments; and that slaty cleavage demonstrates in addition that the up-swelling produced by this cause alone is sufficient to account for the elevation of the greatest mountain chains.*

[To be concluded.]

## ART. XLI.—*Letter to the Superintendent of the U. S. Coast Survey, containing a Catalogue of bright lines in the Spectrum of the Solar Atmosphere, observed at Sherman, Wyoming Territory, U. S. A., during July and August*, 1872; by Prof. C. A. YOUNG, of Dartmouth College.

Prof. B. PEIRCE, Supt. U. S. C. S., &c., &c.

*Dear Sir*—Without waiting to complete my entire report of the spectroscopic work at Sherman, I send for immediate publication, should you think proper, a list of the bright lines observed in the spectrum of the chromosphere during the past summer.

The great altitude of the station (nearly 8,300 feet), and the consequent atmospheric conditions, were attended with even greater advantages for my special work than had been really expected, although I was never quite able to realize my *hope* of seeing all the Fraunhofer lines reversed, unless once or twice for a moment, during some unusual disturbances of the solar surface.

Everything I saw, however, confirmed my belief that the origin of the dark lines is at the base of the chromosphere, and that the ability to see them all reversed at any moment depends merely upon instrumental power and atmospheric conditions.

In this view, a catalogue of the bright lines actually observed is of course less important than it would be otherwise; still it is not without interest and scientific value, since the lines seen are naturally those which are really most conspicuous in the chromosphere spectrum, and this conspicuousness stands in important, but by no means obvious or even entirely simple, relations to the intensity of the corresponding dark lines, when such exist. There can be no doubt that a careful study of these bright lines and their behavior would yield much valuable information as to the constitution and habitudes of the solar atmosphere.

In the catalogue, the first column contains simply a reference number: a ‡ refers to a note at the end of the catalogue.

The numbers in the second column refer to my "Preliminary Catalogue," containing 103 lines, which was published a year ago in the "American Journal of Science." In this column a † indicates that some other observer has anticipated me in the determination and publication of the line. As I have depended for my information almost solely upon the Comptes Rendus and the Proceedings of the Royal Society (which give the observations of Lockyer, Janssen, Rayet and Secchi), it is quite possible that some other lines ought to be marked in the same manner.

The third column, headed K, gives the position of the lines on Kirchoff's scale, the numbers above G being derived from Thalen's continuation of Kirchoff's maps. In this column an asterisk denotes that the map shows no corresponding dark line, a ? that the exact position, not the existence, of the line is for some reason slightly uncertain.

The fourth column, headed A, gives the wave-length of the line in ten millionths of a millimeter, according to Angström's atlas.

The numbers in this and the preceding column were taken, not from the maps themselves, which present slight inaccuracies on account of the shrinking and swelling of the paper during the operation of printing, but from the numerical catalogues of Kirchoff and Angström which accompany their respective atlases. In the preliminary catalogue the numbers were derived from the maps; hence some slight discrepancies in the tenths of division.

The fifth column, marked F, contains a rough estimate of the percentage of frequency with which the lines were seen during the six weeks of observation; and the sixth column, B, a similar estimate of their maximum brightness compared with that of the hydrogen line C.

The variations of brilliance, however, when the chromosphere was much disturbed, were so considerable and so sudden that no very great weight can be assigned to the numbers given. Nor is it to be inferred that lines which have in the table the same index of brightness were always equally bright. On certain occasions one set of lines would be particularly conspicuous; on others, another.

With two or three exceptions, indicated in the notes, no lines have been catalogued which were not seen on at least two different days. In the few cases, where lines observed only on one occasion have been admitted to the list, the observations were at the time carefully verified by my assistant, Prof. Emerson, so as to place their correctness beyond a doubt. Many other lines were "glimpsed" at one time and another, but not seen steadily enough or long enough to admit of satisfactory determination.

The last column of the catalogue contains the symbols of the chemical elements corresponding to the respective lines. The materials at my disposal are the maps of Kirchoff and Angström, Thalen's map of the portion of the solar spectrum above G, and "Watts' Index of Spectra." Since the positions of the lines in the latter work are given only to the nearest unit of "Angström's scale," I have marked the coincidences indicated by it with a (w), considering them less certain than those shown by the maps.

In addition to the elements before demonstrated to exist in the chromosphere, the following seem to be pretty positively indicated—sulphur, cerium and strontium; and the following with a somewhat less degree of probability, zinc, erbium and yttrium, lanthanum and didymium. There are some coincidences also with the spectra of oxygen, nitrogen and bromine, but not enough considering the total number of lines in the spectra of these elements, or of a character, to warrant any conclusion. One line points to the presence of iridium or ruthenium, and only three are known in the whole spectrum of these metals.

No one, of course, can fail to be struck with the number of cases in which lines have associated with them the symbols of two or more elements. The coincidences are too many and too close to be all the result of accident, as for instance in the case of iron and calcium, or iron and titanium.

Two explanations suggest themselves. The first, which seems rather the most probable, is that the metals operated upon by the observer who mapped their spectra, were not absolutely pure—either the iron contained traces of calcium and titanium, or *vice versa.* If this supposition is excluded, then we seem to be driven to the conclusion that there is some such similarity between the molecules of the different metals as renders them susceptible of certain synchronous periods of vibrations—a resemblance, as regards the manner in which the molecules are built up out of the constituent atoms, sufficient to establish between them an important physical (and probably chemical) relationship.

I have prefixed to the catalogue a table showing the number of lines of each substance, or combination of substances, observed in the chromosphere spectrum; omitting, however, oxygen, nitrogen and bromine, since with one exception (line 230), neither of them ever stands alone, or accounts for any lines not otherwise explained.

The instruments and methods of observation were the same employed in the construction of the Preliminary Catalogue. Telescope, $9\frac{4}{10}$ inches aperture—spectroscope automatic, with dispersive force of 12 prisms.

The approximate geographical position of Sherman is Lon. $1^h\ 53{\cdot}2^m$ west of Washington; Lat. 41° 07′; Altitude above sea-level 8,280 feet; mean height of barometer about 22·1 inches.

*Table showing the number of coincidences between the bright lines observed in the spectrum of the chromosphere, and those of the spectra of the chemical elements.*

| | | | | | | |
|---|---|---|---|---|---|---|
| Fe, Ti, $S_{(w)}$ | 1 | | | Unknown. | 52 | Total, |
| " Ba, $S_{(w)}$ | 1 | Ti, $S_{(w)}$ | 3 | Fe, | 14 | 110 |
| " $S_{(w)}$ $Zn_{(w)}$ | 1 | " Ca, | 2 | Ti, | 23 | 43 |
| " Co, Ce, | 1 | " Mn, | 1 | Ca, | 10 | 29 |
| " Ni, $E_{(w)}$ | 1 | " Ce, | 1 | Ba, | 8 | 13 |
| Ca, Cr, Ce, | 1 | " Sr, | 1 | $S_{(w)}$ | 7 | 14 |
| " Li, Zn, | 1 | " Zn, | 1 | Mn, | 6 | 12 |
| Ti, Ba, $S_{(w)}$ | 1 | Ca, Cd, | 1 | Ce, | 5 | 11 |
| Ba, La, $E_{(w)}$ | 1 | " Ce, | 1 | H, | 4 | 4 |

TABLE—continued.

| | | | | | | |
|---|---|---|---|---|---|---|
| Fe, Ca, | 10 | Ca, Co, | 1 | Na, | 4 | 6 |
| " Ti, | 9 | " Cr, | 1 | Cr, | 4 | 10 |
| " Mn, | 4 | " Sr, | 1 | Mg, | 3 | 4 |
| " Cr, | 3 | | | Sr, | 3 | 6 |
| " Ni, | 3 | S$_{(w)}$ E$_{(w)}$ | 1 | Zn, | 3 | 9 |
| " Ba, | 2 | | | E$_{(w)}$ | 2 | 9 |
| " Zn, | 2 | Mn, Zn, | 1 | Ni, | 2 | 6 |
| " E$_{(w)}$ | 2 | | | Co, | 1 | 5 |
| | | Cr, E$_{(w)}$ | 1 | Cu, | 1 | 2 |
| " Ce, | 1 | | | La, | 1 | 3 |
| " Co, | 1 | Ce, Co, | 1 | Ru, Ir, | 1 | 1 |
| " Mg, | 1 | | | Cd, | | 1 |
| " Na, | 1 | Na, Cu, | 1 | Li, | | 1 |
| " S$_{(w)}$ | 1 | | | | | |
| " La, | 1 | lines mark'd with a * | 14 | | | |

The numbers in the last column denote the whole number of times that the symbol of each element appears in the catalogue, either singly or combined with others.

*Catalogue of bright lines in the Spectrum of the Chromosphere.* 1872.

| No. | P. C. | K. | A. | F. | B. | E. | No. | P. C. | K. | A. | F. | B. | E. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1‡ | 1† | 534·0* | 7055·? | 100 | 12 | | 38 | 11† | D$_1$1002·8 | 5895·0 | 50 | 30 | Na, |
| 2 | 2† | 654·3 | 6676·9 | 25 | 50 | Fe, Ba$_{(w)}$ | 39 | 12† | D 1006·8 | 5889·0 | 50 | 30 | Na, |
| 3 | 3† | C.694·1 | 6561·8 | 100 | 100 | H, | 40‡ | † | 1011·2 | 5883·0 | 2 | 1 | Fe, |
| 4 | | 711·4 | 6515·5 | 15 | 4 | | 41‡ | 13† | D$_3$1016·5* | 5874·9 | 100 | 90 | |
| 5 | 4 | 718·7 | 6496·0 | 18 | 5 | Ba, | 42 | | 1031·8 | 5852·7 | 8 | 2 | Ba, |
| 6 | | 731·7 | 6461·7 | 5 | 2 | Ca. | 43 | | 1135·1 | 5708·3 | 1 | 1 | Fe, |
| 7 | 5 | 734·0 | 6453·8 | 10 | 6 | | 44 | | 1151·1 | 5687·2 | 2 | 1 | Na, |
| 8 | | 740·9 | 6438·1 | 5 | 2 | Ca, Cd, | 45 | | 1154·2 | 5683·5 | 5 | 3 | |
| 9‡ | 6 | 744·3 | 6429·9 | 20 | 4 | | 46 | | 1155·8 | 5681·5 | 2 | 1 | Na, Fe, N$_{(w)}$ |
| 10 | | 750·1 | 6415·6 | 5 | 2 | | 47 | | 1165·7 | 5667·8 | 2 | 2 | S$_{(w)}$ |
| 11 | | 756.9 | 6399·0 | 5 | 2 | Fe, | 48 | | 1167·0 | 5666·0 | 1 | 1 | |
| 12 | | 759·3 | 6392·6 | 5 | 1 | Fe, | 49 | | 1170·6 | 5661·5 | 15 | 2 | Fe, Ti, E$_{(w)}$ |
| 13 | | 767·?* | 6373·? | 5 | 2 | | 50 | | 1175·0 | 5656·7 | 8 | 3 | S$_{(w)}$ N$_{(w)}$ |
| 14 | 7 | 768·?* | 6371·? | 5 | 3 | | 51 | | 1176·6 | 5654·4 | 2 | 1 | Fe, |
| 15 | | 778·3 | 6346·1 | 10 | 4 | Ruth, Ir, | 52 | | 1187·1 | 5640·2 | 1 | 1 | S$_{(w)}$ |
| 16‡ | 8 | 823·5 | 6245·4 | 8 | 5 | Fe, | 53 | | 1189·3 | 5637·3 | 1 | 1 | |
| 17‡ | 9 | 827·6 | 6237·3 | 8 | 2 | | 54 | | 1200·6 | 5623·2 | 2 | 1 | Fe, |
| 18 | | 830·2 | 6231·5 | 5 | 1 | Fe, | 55 | | 1207·3 | 5614·5 | 2 | 1 | Fe, |
| 19 | | 836·5 | 6218·3 | 3 | 1 | Ti, | 56 | | 1229·6 | 5587·6 | 2 | 2 | Ca, |
| 20 | | 839·2 | 6214·1 | 3 | 1 | Ti, | 57 | | 1231·3 | 5585·5 | 2 | 1 | Fe. |
| 21 | | 845·7 | 6199·6 | 2 | 2 | Fe, | 58 | 14† | 1274·2 | 5534·1 | 50 | 12 | Ba, Fe, Sr, |
| 22 | | 849·7 | 6190·5 | 10 | 2 | Fe, | 59 | 15 | 1281·3 | 5525·9 | 40 | 5 | Fe, |
| 23 | | 859·7 | 6168·3 | 3 | 1 | Ca, | 60 | | 1287·5 | 5518·7 | 15 | 2 | Ba, |
| 24 | | 863·9 | 6161·2 | 8 | 3 | Ca, | 61 | | 1298·9 | 5505·8 | 2 | 1 | Fe, |
| 25 | | ⎰ 870·9 | 6148·1 | 3 | 2 | Fe, E$_{(w)}$ | 62 | | 1303·5 | 5500·5 | 2 | 1 | Fe, La, |
| 26 | | ⎱ 871·4 | 6146·8 | 3 | 2 | | 63 | | 1306·7 | 5496·6 | 2 | 1 | Fe, E$_{(w)}$ |
| 27 | 10 | 874·3 | 6140·6 | 25 | 10 | Ba, | 64 | | 1320·6 | 5480·2 | 2 | 1 | Ti, Sr, |
| 28 | | ⎰ 876·5 | 6136·1 | 2 | 1 | | 65 | | 1324·8 | 5475·9 | 1 | 1 | Ni, |
| 29 | | ⎱ 877·0 | 6135·6 | 2 | 1 | Fe, | 66 | | 1328·7 | 5472·3 | 3 | 1 | |
| 30 | | 884·9 | 6121·2 | 5 | 3 | Ca, Co, | 67 | | 1337·0 | 5462·3 | 1 | 1 | Fe, N$_{(w)}$ |
| 31 | | 890·2 | 6109·9 | 2 | 1 | Ba, | 68 | 16 | 1343·5 | 5454·7 | 10 | 4 | Fe, |
| 32 | | 894·9 | 6101·7 | 3 | 2 | Ca, Li, Zn$_{(w)}$ | 69 | 17 | 1351·1 | 5445·9 | 10 | 4 | Fe, Ti, Br$_{(w)}$ |
| 33 | | 903·1 | 6083·1 | 3 | 2 | Ti, | 70 | | 1360·9 | 5435·4 | 5 | 2 | Zn, Br$_{(w)}$ |
| 34 | | 912·1 | 6064·5 | 5 | 2 | Fe, Ti, | 71 | | 1362·9 | 5433·0 | 2 | 2 | Fe, |
| 35 | | 933·8 | 6018·0 | 2 | 1 | Ba, | 72‡ | 18 | 1364·3 | 5431·8 | 8 | 5 | |
| 36 | | 949·4 | 5990·0 | 10 | 4 | | 73 | 19 | 1367·0 | 5428·8 | 8 | 3 | Fe, Ti, |
| 37 | | 992·0 | 5913·2 | 2 | 1 | Fe, | 74 | 20 | 1372·1 | 5424·5 | 25 | 6 | Ba, Ti, S$_{(w)}$ |

TABLE—continued.

| No. | P C | K. | A. | F. | B. | E. |
|---|---|---|---|---|---|---|
| 75 | 21 | 1377·4 | 5417·9 | 5 | 2 | Ti, Mn, |
| 76 | | 1380·5 | 5414·4 | 2 | 2 | Fe, |
| 77 | 22 | 1382·5 | 5412·4 | 4 | 2 | Mn$_{(w)}$ |
| 78 | | 1384·7 | 5410·0 | 2 | 1 | Fe, Ni, |
| 79 | | 1385·7 | 5409·0 | 2 | 2 | Cr, |
| 80 | | 1389·4 | 5404·8 | 2 | 1 | Fe, |
| 81 | 23 | 1390·9 | 5403·1 | 5 | 3 | Fe, Ti, |
| 82 | | 1394·2 | 5399·6 | 2 | 1 | Mn, |
| 83 | 24 | 1397·5 | 5396·1 | 4 | 2 | Fe, Ti, |
| 84 | | 1401·6 | 5392·2 | 2 | 1 | Fe, Ce, |
| 85 | | 1412·5 | 5380·2 | 3 | 2 | Ti, |
| 86 | 25† | 1421·5 | 5370·5 | 10 | 3 | Fe, |
| 87 | | 1423·0 | 5369·0 | 1 | 1 | Fe, |
| 88 | | 1425·4 | 5366·5 | 1 | 1 | Fe, |
| 89 | | 1428·2 | 5364·0 | 1 | 1 | Fe, |
| 90 | 26† | 1430·1 | 5361·9 | 20 | 10 | Fe, |
| 91 | | 1438·9 | 5352·4 | 4 | 2 | Fe, Co, Ce, |
| 92 | | 1446·7 | 5345·0 | 1 | 1 | |
| 93 | | 1450·8 | 5340·2 | 1 | 2 | Fe, Mn, O$_{(w)}$ |
| 94 | 27 | 1454·7 | 5335·9 | 5 | 2 | Ti, Zn$_{(w)}$ |
| 95 | | 1461·5 | 5329·1 | 6 | 4 | |
| 96 | 28 | 1462·8 | 5327·1 | 5 | 2 | Fe, |
| 97 | 29 | 1463·3 | 5327·6 | 5 | 2 | Fe, |
| 98 | | 1464·8 | 5325·1 | 6 | 2 | |
| 99 | | 1471·9* | 5318·0 | 1 | 1 | |
| 100‡ | 31† | 1473·9 | 5315·9 | 90 | 50 | Fe,? O$_{(w)}$? |
| 101 | | 1476·8 | 5313·1 | 3 | 1 | |
| 102 | | 1497·3 | 5292·0 | 1 | 1 | Cu, Br$_{(w)}$ |
| 103 | 32 | 1505·3 | 5283·4 | 20 | 10 | Ti$_{(w)}$ |
| 104 | 33† | 1515·5 | 5275·0 | 30 | 15 | |
| 105 | 34 | E$_1$1522·7 | 5269·5 | 15 | 4 | Fe, Ca, |
| 106 | 35 | E$_2$1523·7 | 5268·5 | 12 | 3 | Fe, |
| 107 | 36† | 1527·7 | 5265·8 | 10 | 4 | Fe, Co, |
| 108 | | 1530·2 | 5263·3 | 1 | 1 | Ca, Br$_{(w)}$ |
| 109 | | 1538·5* | 5256·2 | 2 | 1 | Sr, |
| 110 | | 1541·9 | 5254·1 | 1 | 2 | Fe, Mn, |
| 111 | | 1547·7 | 5249·7 | 3 | 1 | Fe,Z$_{(w)}$Br$_{(w)}$ |
| 112 | | 1551·6 | 5246·3 | 3 | 1 | Fe, |
| 113 | 37 | 1561·0 | 5239·0 | 4 | 2 | Fe, |
| 114 | 38 | 1564·2 | 5236·3 | 4 | 2 | |
| 115 | 39† | 1567·5 | 5233·6 | 10 | 8 | Mn, Zn$_{(w)}$ |
| 116 | 40 | 1569·6 | 5232·1 | 1 | 3 | Fe, |
| 117 | | 1575·4 | 5227·5 | 1 | 1 | Sr,? |
| 118 | 41 | 1577·4 | 5226·2 | 10 | 3 | Fe, |
| 119 | | 1578·1 | 5225·5 | 2 | 3 | Sr, Br$_{(w)}$ |
| 120 | 42 | 1580·1 | 5224·3 | 2 | 2 | Ti, |
| 121 | | 1589·1 | 5216·5 | 2 | 1 | Fe, |
| 122 | | 1590·7 | 5215·5 | 3 | 2 | Fe, |
| 123 | | 1592·3 | 5214·4 | 2 | 1 | Fe, |
| 124 | | 1597·9* | 5210·5 | 1 | 1 | |
| 125 | | 1598·9 | 5209·5 | 1 | 2 | Ti, |
| 126 | 43 | 1601·5 | 5207·6 | 10 | 6 | Fe, Cr, |
| 127 | 44 | 1604·4 | 5205·2 | 10 | 6 | Cr, E$_{(w)}$ |
| 128 | 45 | 1606·4 | 5203·7 | 10 | 6 | Cr, Fe, |
| 129 | 46 | 1609·2 | 5201·5 | 5 | 3 | Fe, |
| 130 | 47 | 1611·3 | 5199·7 | 4 | 2 | S$_{(w)}$ E$_{(w)}$ |
| 131 | | 1613·9 | 5197·9 | 1 | 1 | Fe, |
| 132 | 48† | 1615·6 | 5197·0 | 15 | 10 | |
| 133 | | 1617·4 | 5195·0 | 1 | 1 | Mn, |
| 134 | | 1618·9 | 5194·1 | 2 | 2 | Fe, |
| 135 | | 1627·2 | 5188·2 | 10 | 5 | Fe, Ca, |
| 136 | | 1628·2 | 5187·3 | 1 | 1 | Ti, |
| 137 | | 1631·5 | 5185·1 | 5 | 2 | Fe, Ti, |
| 138 | 49† | b$_1$1634·1 | 5183·0 | 50 | 30 | Mg, |
| 139 | 50† | b$_2$1648·8 | 5172·0 | 50 | 35 | Mg, |
| 140 | 51† | b$_3$1653·7 | 5168·3 | 40 | 30 | Fe, Ni, Br$_{(w)}$ |
| 141 | 52† | b$_4$1655·6 | 5166·7 | 30 | 20 | Fe, Mg, |
| 142 | | 1666·? | 5160·? | 1 | 1 | |
| 143 | | 1671·5 | 5154·8 | 3 | 1 | Na, |
| 144 | 53 | 1673·7 | 5152·5 | 3 | 1 | Na, Cu,? |
| 145 | 54 | 1677·9 | 5150·1 | 2 | 2 | Fe, Br$_{(w)}$ |
| 146 | | 1689·5 | 5142·2 | 1 | 2 | S$_{(w)}$ |
| 147 | | 1701·8 | 5133·0 | 1 | 1 | Fe, |
| 148 | | 1704·7 | 5130·8 | 1 | 1 | Fe, |
| 149 | | 1707·9 | 5128·6 | 1 | 1 | Ti, |
| 150 | | 1710·7 | 5126·7 | 1 | 1 | Fe, Ti, |
| 151 | | 1712·2 | 5125·5 | 1 | 2 | |
| 152‡ | | 1713·4 | 5124·4 | 1 | 1 | Fe, |
| 153 | | 1715·2 | 5123·2 | 1 | 1 | Fe, |
| 154 | | 1717·9 | 5121·0 | 1 | 1 | Fe, |
| 155 | | 1719·4 | 5119·9 | 1 | 1 | Ti, |
| 156‡ | | 1727·3 | 5114·9 | 1 | 1 | Ni, |
| 157 | | 1734·6 | 5108·8 | 2 | 2 | Ti$_{(w)}$ |
| 158 | | 1737·7 | 5107·0 | 1 | 1 | Fe, |
| 159 | | 1750·4 | 5098·1 | 1 | 1 | Fe, |
| 160 | | 1752·8 | 5096·5 | 1 | 1 | Fe, S$_{(w)}$ |
| 161 | | 1765·0* | 5087·0 | 2 | 1 | E$_{(w)}$ |
| 162 | | 1771·5 | 5083·5 | 1 | 1 | Zn$_{(w)}$ |
| 163 | 55 | 1778·5 | 5077·9 | 1 | 2 | Fe, |
| 164 | | 1823·6 | 5047·8 | 2 | 2 | Fe,? Zn$_{(w)}$ |
| 165 | | 1833·4 | 5041·2 | 2 | 2 | Fe, Ca, |
| 166 | | 1834·3 | 5040·1 | 2 | 2 | Fe, |
| 167 | | 1848·9 | 5030·1 | 4 | 3 | S$_{(w)}$ |
| 168 | | 1856·9 | 5023·5 | 3 | 1 | S$_{(w)}$ |
| 169 | 56† | 1867·1 | 5017·6 | 30 | 15 | Fe. Ni, |
| 170 | 57† | 1870·6 | 5015·0 | 30 | 10 | Ti$_{(w)}$ |
| 171 | | 1905·1 | 4993·3 | 2 | 1 | Fe, N$_{(w)}$ |
| 172‡ | | c 1961·0 | 4956·7 | 1 | 2 | Fe, |
| 173 | 58† | 1989·5 | 4933·4 | 30 | 8 | Ba, |
| 174 | 59† | 2001·6 | 4923·1 | 40 | 12 | Fe,S$_{(w)}$Zn$_{(w)}$ |
| 175 | 60† | 2003·2 | 4921·3 | 30 | 8 | S$_{(w)}$ |
| 176 | 61 | 2007·2 | 4918·2 | 20 | 3 | Fe, |
| 177 | | 2016·0 | 4911·2 | 3 | 2 | Zn$_{(w)}$ |
| 178 | 62† | 2031·1 | 4899·3 | 30 | 6 | Ba, La, E$_{(w)}$ |
| 179 | 63† | 2052·5?* | 4882·9 | 10 | 4 | Ce, |
| 180 | | 2067·8 | 4869·4 | 5 | 1 | |
| 181 | 64† | F 2080·0 | 4860·6 | 100 | 80 | H, |
| 182 | | 2087·6 | 4854·7 | 5 | 2 | Fe, Ni, E$_{(w)}$ |
| 183 | | 2094·0 | 4848·1 | 3 | 2 | Ca, O$_{(w)}$ |
| 184 | | 2116·?* | 4826·5 | 1 | 1 | |
| 185 | | 2121·2 | 4822·8 | 10 | 2 | Mn, |
| 186 | | 2142·4 | 4804·4 | 3 | 1 | Ti, S$_{(w)}$ O$_{(w)}$ |
| 187 | | 2171·5 | 4778·7 | 3 | 2 | Co, N$_{(w)}$ |
| 188 | | 2229·1 | 4730·8 | 1 | 1 | Fe, |
| 189‡ | | 2251·3* | 4712·5 | 2 | 2 | Ce, O$_{(w)}$ |
| 190 | | 2309·5 | 4666·3 | 3 | 1 | Fe, Ti, |

TABLE—continued.

| No. | P C | K. | A. | F. | B. | E. |
|---|---|---|---|---|---|---|
| 191 | | 2314·3 | 4663·3 | 2 | 1 | |
| 192 | | 2323·0 | 4656·0 | 2 | 1 | Ti, |
| 193 | 65 | 2358·4 | 4629·0 | 15 | 8 | Ti, $N_{(w)}$ |
| 194 | | 2359·5* | 4628·2 | 2 | 1 | Ce, |
| 195 | | 2369·7 | 4620·3 | 1 | 1 | |
| 196 | | 2410·2 | 4589·4 | 1 | 1 | |
| 197 | | 2412·8 | 4587·5 | 2 | 2 | |
| 198 | 66 | 2419·3 | 4583·2 | 15 | 6 | |
| 199 | | 2429·5 | 4576·0 | 4 | 2 | |
| 200 | 67 | 2435·5 | 4571·4 | 10 | 4 | Ti, |
| 201 | 68 | 2443·9 | 4564·8 | 10 | 3 | |
| 202 | 69 | 2446·6 | 4563·2 | 10 | 5 | Ti, |
| 203 | | 2452·1 | 4559·5 | 8 | 2 | |
| 204 | | 2454·1 | 4558·1 | 8 | 1 | |
| 205 | 70 | 2457·9 | 4555·3 | 10 | 5 | Fe, Ti, |
| 206 | 71 | 2461·2 | 4553·4 | 10 | 5 | Ba, |
| 207 | | 2463·4 | 4551·8 | 1 | 1 | Ti, $S_{(w)}$ |
| 208 | 72 | 2467·6 | 4548·9 | 10 | 8 | Ti, |
| 209 | | 2480·8 | 4539·2 | 2 | 1 | Ce, |
| 210 | 73 | 5486·6 | 4535·5 | 2 | 2 | Ti, Ca, |
| 211 | 74 | 2489·4 | 4533·2 | 5 | 5 | Fe, |
| 212 | | 2490·5 | 4532·1 | 3 | 2 | Ti, Ca, |
| 213 | 76 | 2502·2 | 4524·4 | 3 | 2 | Ba, Fe, |
| 214 | 77 | 2505·6 | 4522·0 | 3 | 3 | Ti, $S_{(w)}$ |
| 215 | | 2517·0 | 4514·0 | 2 | 1 | |
| 216 | | 2518·4 | 4513·0 | 1 | 1 | |
| 217 | | 2527·0 | 4506·0 | 2 | 1 | |
| 218 | 78 | 2537·1 | 4500·3 | 15 | 6 | Ti, |
| 219 | 79 | 2552·4 | 4490·9 | 20 | 8 | Mn, |
| 220 | 80 | 2555·0 | 4489·4 | 15 | 3 | Fe, Mn, |
| 221 | 81 | 2566·3 | 4480·9 | 5 | 2 | Mg, |
| 222‡ | 82† | f 2581·2 | 4471·2 | 100 | 25 | Ce, |
| 223 | 83 | 2585·4 | 4468·5 | 20 | 5 | Ti, $O_{(w)}$ |
| 224 | | 2620·8 | 4446·3 | 1 | 1 | Ti, |
| 225 | 84 | 2625·2 | 4443·0 | 10 | 2 | Ti, |
| 226 | | 2633·0 | 4436·7 | 1 | 1 | Mn ? |
| 227 | | 2639·6 | 4433·5 | 1 | 1 | |
| 228 | | 2651·5* | 4426·0 | 2 | 3 | |
| 229 | | 2653·2 | 4425·0 | 2 | 2 | Ca, |
| 230 | | 2664·9 | 4418·0 | 2 | 1 | $O_{(w)}$ |
| 231 | | 2665·9 | 4417·5 | 3 | 1 | Ti, |
| 232 | 85 | 2670·0 | 4414·7 | 1 | 1 | Fe, Mn, $O_{(w)}$ |
| 233 | | 2680·0 | 4407·7 | 1 | 1 | Fe, Ca, |
| 234 | | 2686·8 | 4404·2 | 1 | 1 | Fe, |
| 235 | | 2696·0 | 4398·5 | 1 | 1 | Ti, Ce, $O_{(w)}$ |
| 236 | | 2698·2 | 4396·5 | 1 | 2 | |
| 237 | 87 | 2702·5 | 4394·6 | 15 | 3 | |
| 238 | | 2715·2 | 4388·5 | 1 | 1 | Fe? |
| 239 | 88 | 2718·5 | 4384·7 | 8 | 2 | Ca, Ce, |
| 240 | | 2720·2 | 4383·5 | 1 | 1 | |
| 241 | 89 | 2721·6 | 4382·8 | 1 | 1 | Fe, Cr, |
| 242 | | 2725·8 | 4380·4 | 1 | 1 | |
| 243 | | 2728·0 | 4379·1 | 1 | 1 | Ca, |
| 244 | 90 | 2733·7 | 4375·5 | 5 | 3 | Fe, |
| 245 | 91 | 2736·9 | 4374·2 | 8 | 3 | $E_{(w)}$ |
| 246 | | 2762·0 | 4359·1 | 1 | 1 | Cr, |
| 247 | 92 | 2775·7 | 4351·8 | 3 | 1 | Cr, |
| 248 | 93† | 2795·7 | 4340·1 | 100 | 65 | H, |
| 249 | | 2798·0 | 4338·2 | 10 | 2 | Cr, |
| 250 | | 2805·4 | 4335·1 | 2 | 1 | La, |
| 251 | | 2823·4 | 4324·0 | 1 | 2 | |
| 252 | | 2830·7 | 4320·1 | 1 | 1 | Ti, $O_{(w)}$ |
| 253 | | 2843·0 | 4313·5 | 1 | 1 | Ti, |
| 254 | 94 | G.2854·2 | 4307·2 | 3 | 2 | Ca, Fe. |
| 255 | 95 | 2867·7 | 4302·1 | 3 | 2 | Ca, Fe, |
| 256 | 96 | 2874·2 | 4298·0 | 1 | 1 | Ca, Fe, |
| 257 | 97 | 2894·5 | 4289·4 | 1 | 1 | Cr, Ca, $Ce_{(w)}$ |
| 258 | 98 | 2928·5 | 4274·6 | 2 | 1 | Cr, Ca, |
| 259 | 99 | 2961·2 | 4260·0 | 2 | 1 | Fe, |
| 260 | 100 | 2996·2 | 4245·2 | 30 | 3 | Fe, |
| 261 | | 3018·0 | 4235·5 | 30 | 5 | Fe, |
| 262 | | 3022·8 | 4233·0 | 15 | 5 | Fe, Ca, |
| 263 | 101 | 3040·0 | 4226·3 | 3 | 3 | Ca, Sr, |
| 264 | 102 | 3061·8 | 4215·3 | 40 | 7 | Ca, Sr, |
| 265 | | 3155·5 | 4178·8 | 1 | 1 | |
| 266 | | 3187·0 | 4166·7 | 1 | 1 | Ca, |
| 267 | 103† | h. 3363·5 | 4101·2 | 100 | 50 | H, |
| 268 | | 3431·0 | 4077·0 | 25 | 2 | Ca, |
| 269 | | 3526·0 | 4045·0 | 3 | 2 | Fe, |
| 270 | | 3703·3 | 3990·? | 2 | 1 | |
| 271 | | 3769·5 | 3970·? | 2 | 1 | Fe, |
| 272‡ | | $H_1$3778·5 | 3967·9 | 75 | 3 | Fe, Ca, |
| 273‡ | | $H_2$3882·5 | 3932·8 | 50 | 1 | Fe, Ca, |

NOTES.

1. The position assigned to this line, first observed by Respighi (a fact of which I was ignorant when the Preliminary Catalogue was published), rests upon two series of micrometric measurements, referring it to four neighboring dark lines—the probable error is about $\frac{1}{20}$th of a division of Kirchoff's scale.

9. No. 6 in P C. Position there given, 743?

16 and 17. Nos. 8 and 9 of P C. Position given as 816·8 and 827·6, by a mistake in identifying lines upon the map.

40. I have never myself seen this line reversed. Prof. Emerson, however, saw it several times. It was first reported by Rev. S. J. Perry, in *Nature*, vol. iii, p. 67.

41. The position of this line has been independently determined by three series of micrometric comparisons with neighboring lines. My result agrees exactly with that of Huggins.

72. Erroneously given in P C, as 1363·1, which line does not reverse, or at least was never seen reversed at Sherman.

100. The principal line in the spectrum of the corona. The corresponding line in the spectrum of iron is feeble, and on several occasions when the neighboring lines of iron (1463, &c.), have been greatly disturbed, this has wholly failed to sympathise. Hence I have marked the Fe with a ?. Watts indicates a strong line of oxygen at 5315 A.

152 and 156. Observed only on one day, but verified by Prof. Emerson.

172. Called 'little C' by Mr. Stoney.

179. Given by Lockyer as K 2054. Its position is a little uncertain; it seems to coincide with neither of the dark lines at 2051 and 2054, but lies between them, a little nearer to 2051.

189. Rather a band than a line.

222. The position of this line, which, however, like 189, is rather a band, was determined by two series of careful micrometrical measurements. It was discovered by Rayel in January, 1869; since called 'f' by Lorenzoni.

272 and 273. These lines were both reversed (by a narrow bright stripe running down the center of the broad hazy band) as constantly, whenever the seeing was good, as *h* or C itself. The observation was difficult, however, and required the most scrupulous exclusion of foreign light, and a careful adjustment of the slit in the plane of the solar image formed by these particular rays.

They were also found to be regularly reversed upon the body of the sun itself, in the *penumbra and immediate neighborhood of every important spot.*

## ART. XLII.—*On the Quartzite, Limestone and associated rocks of the vicinity of Great Barrington, Berkshire Co., Mass.;* by JAMES D. DANA. With a map, on Plate IV.

WHATEVER the appearances at Canaan,* the quartzite in Great Barrington (or Barrington, as it is called in the guide-books), a dozen miles north of Canaan, alternates with mica schist, and *both of these rocks for the most part overlie the "Stockbridge" limestone.*† The quartzite is, therefore, as well as the schist, to a great extent a newer rock; and as both occur in some places interstratified with upper beds of the limestone, all are parts of one consecutive series of strata.

If then the western range of limestone in Berkshire is, like that of West Rutland, Chazy‡ in age, the range to the eastward

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* See, on the "Canaan Quartzite," this Journal, III, iii, 179. I shall at another time reconsider the conclusion in that paper with regard to the relations of the quartzite to the other rocks; it was based on general considerations and not on observed sections.

† This relation of the gneiss and quartzite was observed by Prof. Hitchcock, and is stated in his Massachusetts Geological Report (pp. 588–590, 1841), where he says that the quartzite might be considered a member of the gneiss and mica schist formation. Its relation to the associated limestone he speaks of, on p. 590, as "little more than a juxtaposition." But on p. 573 he had stated correctly that inasmuch as the limestone was interstratified with the same schist, it therefore must be also with the quartzite. The areas colored as *quartzite*, on both the Massachusetts and Vermont geological maps, are largely, and some *wholly*, mica schist, gneiss, or hydro-mica slate. This is the fact with the Great Barrington region as well as others.

‡ This volume, page 133, in a note by Mr. E. Billings.

is probably Trenton; and, further, the quartzite and the associated mica schist (and gneiss), since they directly overlie the limestone, and besides are sometimes interstratified with it, are, with little doubt, either of the Trenton, or of the Cincinnati (Hudson River) group. Prof. Hitchcock makes the statement that the limestone of the western range is essentially pure carbonate of lime, or contains very little magnesia if any, while the more eastern, both in Massachusetts and Vermont, is mostly dolomite.*

On the western border of Berkshire (or of Massachusetts) stretch along the Taconic mountains, which consist in these latitudes, on the eastern side, chiefly of a fine-grained mica slate, sometimes chloritic, and occasionally staurolitic as well as garnetiferous. The rocks dip eastward, with local exceptions, and underlie conformably the western range of Berkshire limestone. The limestone adjoining these Taconic slates on the east being Chazy, the slates themselves are almost certainly of the period next preceding, and therefore Quebec schists, as made by Logan.

Hence, in this part of the Green Mountain region, the rocks are probably a continuous series of Lower Silurian strata, commencing with the Quebec group, and ending seemingly with the upper group of this era—the Cincinnati or Hudson River group. These views accord in the main with those of the geological map of Sir Wm. E. Logan, that is, in making the beds Lower Silurian, and the Taconic slates to represent the Quebec group; but differ in referring the other rocks (on the basis of facts since ascertained) to the Trenton and perhaps also the Hudson River period. They sustain as regards the limestones the view put forward as early as 1841, by Professors W. B. and H. D. Rogers.† They are entirely at variance with the views of Prof. Emmons, who made the limestone, schists and quartzite, all older than the Potsdam. They are inconsistent with the view that in Canada and Western New England an epoch of disturbance intervened between the Trenton era and that preceding it; and support the old conclu-

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* How far this difference is connected with difference of geological age has not been ascertained. Hitchcock states that successive layers in the same region are sometimes unlike in this respect, and T. S. Hunt found the same to be true in the Quebec group of Canada. Chemical analyses to settle the question would have, therefore, to be very numerous. In the east-and-west region including North Adams and Williamstown, the marbles of Williamstown, the *western*, are magnesian, according to Hitchcock's Massachusetts Report, while those of North Adams, or the *eastern*, contain little or no magnesia.

† Amer. Phil. Soc., Jan. 1, 1841. This Journ., xlvii, 151, 1844. The Messrs. Rogers made the limestone Trenton, or Chazy and Trenton, but the quartzite they pronounced Potsdam, and the Taconic slates, with those west of them to the Hudson River, Hudson River slates.

sion of Logan, that the great disturbances affecting these Lower Silurian rocks occurred at the close of the Lower Silurian.

I propose to discuss the facts as to the special age of these rocks on another occasion.

I have said that the conformable superposition of the limestone, schists, and quartzite is unquestionable—contrary to the idea with which I entered upon the study of the Green Mountain rocks. The evidence consists in the direct testimony of sections showing unmistakable superposition.

In my discussion of the subject, I first briefly explain the *topography* of the region, in order that the positions and geographical relations of the several localities described may be appreciated. The accompanying map (Plate IV) in its outlines is a reduction of the Berkshire County map of 1858; the topography I have added from such observations as I have been able to make without the use of instruments. I next describe the *kinds of rocks* in order that their variety and their transitions may be understood, and also the bearing of the facts on the value of lithological characters as a criterion in geological chronology. Finally, I shall present the facts connected with the *stratification* of the rocks, as obtained from sections in different parts of the region.

### 1. *Topography.*

In the vicinity of Great Barrington (see the map) there are three principal north-and-south valleys over a region about ten miles wide, each containing strata of crystalline limestone.

(1.) The *western*, that of Egremont (and western part of Great Barrington), is backed, as just stated, by the Taconic mountains.

(2.) The *central* is that of the Housatonic river, on which are situated the villages of Great Barrington; Van Deusenville, two miles north of Great Barrington; Housatonic, two miles farther north; and Stockbridge, three miles northeast of Housatonic, or six from Great Barrington.

(3.) The *eastern*, the valley of Muddy Brook and the Konkapot, lies along the eastern border of the town of Barrington, and has Beartown Mountain on the east.

The two ranges of ridges between these three valleys consist mainly of the metamorphic schists and quartzite which overlie, or are interstratified with, the limestone.

The range bounding the Housatonic valley on the *west* is, by estimate, 100 to 250 feet in height above the river. North of Van Deusenville it consists of two overlapping parts, L and W, and between them flows Williams river.

The range on the *east* of this valley varies from 100 to at least 500 feet in elevation above the Housatonic. To the northward it bends more to the westward toward the Housatonic river and the village of that name, narrowing the valley there to less than a fourth of a mile; in this part the eastern range is a broad elevated region, 300 to 450 feet in height above the river by estimate, and is called Monument Mountain. Southward also the ridge increases in height, and three miles (by the road) east of Great Barrington (2½ miles in a direct line, at T and farther south) becomes "Three Mile Hill," or ridge. The high land of "Three Mile Hill" extends westward to "East Mountain," just east of Great Barrington, reducing abruptly the width of the Housatonic valley in this part to half a mile.

Beartown Mountain is probably full 600 feet above the plain at its western base. It is a ridge chiefly of gneiss, and of the same age with the other ridges just described.

The Egremont plain or region, on passing north of the latitude of Van Deusenville, is divided into three north-and-south valleys. The Tom Ball ridge bounds the town of Alford on the east, and, extending northward into West Stockbridge, divides the southern half of the latter town into an eastern and western section. "Tom Ball," the highest summit of the range, lies directly west of Williamsville, and is by estimate 750 feet in elevation above the plain either side. Its rocks are mica or hydro-mica slate and chloritic mica slate, like those of the Taconic range. Again, the town of Alford is divided longitudinally from north to south by a low ridge of the same slates, 50 to 250 or 300 feet in elevation.

I may also state, as it will be necessary to make use of the fact in the course of this memoir, that a range, similar in rocks and altitude to the Tom Ball ridge, lies farther east along the *eastern* boundary of the town of West Stockbridge, and extends northward between Richmond and Lenox to the southern border of Pittsfield. "Lenox Mountain" is its highest part, and may well give a name to the ridge. All the ridges here mentioned have an approximate parallelism to the Taconic range farther west.

The limestone region extends to the eastward of Beartown Mountain. I propose to continue my investigations of the Taconic and later rocks in that direction another season. How far they spread east, whether anywhere to the Connecticut river valley or not, is yet among the unknown things in American geology.

### 2. *Kinds of Rocks.*

The following rocks occur in the limestone region within 20 miles north or south of Great Barrington. The beds of schist and slate either directly underlie, or are interstratified with, or overlie the limestone; the quartzite rocks all overlie the lower limestone stratum, but are sometimes interstratified with others above.

A. *Limestone.*—The limestone of the region is, in general, a fine-grained crystalline rock, white, bluish-white, or clouded. It graduates in some places into a slate-blue variety, which is but slightly crystalline. At one place along the railroad, a mile west of Stockbridge, I looked, with some hope of success, for fossils; and I think I found one, although the form somewhat like the head of a Cystidean with its stem, is not distinct enough to entitle it to credit. The same bed at times varies in the course of a mile or less, from a blue semi-crystalline rock into a whitish crystalline. Pyrites and minute brown tourmalines are present in some impure beds. Tremolite is a very common mineral in the Canaan limestone, where a massive form is the so-called *Canaanite;* it also occurs in the next town, Sheffield, but is sparingly met with in the limestone farther north. Brown mica in scales is also common in the less pure layers of the Canaan limestone.

B. *Metamorphic schistose rocks and slates.*—The following are among the occurring kinds of schistose and slaty rocks.

(1) *Mica schist*, abounding in mica, the scales largish, their colors white and black intermingled, the latter predominating.

(2) *Arenaceous mica schist*, with minute scales of mica.

(3) *Mica slate*, using this term in a sense distinct from that of mica schist, for a shining slaty rock, smooth in surface, intermediate between mica schist and clay slate, and also between mica schist and hydro-mica slate. It is a prominent constituent of the Taconic Mountains, and also of Tom Ball and other summits in the limestone area. This rock is in some places *staurolitic* and *garnetiferous.*

(4) *Hydro-mica slate*, like the mica slate, but feeling greasy, and owing this peculiarity to the presence of a hydrous mica instead of the ordinary anhydrous. It is not always easy to draw the line between this slate and mica slate, except by chemical analysis. The slate of Tom Ball and that of the Taconic Mountains have in part the external characters of this rock. [Near Rutland, Vt., a variety, very decidedly soapy to the touch, occurs associated with, and graduates into, quartzite, and all *overlie* apparently the limestone of the region.*]

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* I repeat here that the hydro-mica slate was formerly called *talcose slate*, and by Emmons *magnesia slate;* later by T. S. Hunt, in the Canada Geological Reports, *nacreous shale;* and by Hitchcock in the Vermont Geological Report of 1861, *talcoid schist.* It was shown by Hunt (Can. G. Rep. 1854, 1863) and G. F. Barker (Vt. G. Rep., p. 708) to contain no magnesia.

The following are analyses; 1 by Hunt; 2 to 4 by Barker:

| | $\ddot{\text{Si}}$ | $\dddot{\text{Al}}$ | $\dot{\text{Fe}}$ | $\dot{\text{Mg}}$ | $\dot{\text{Ca}}$ | $\dot{\text{Na}}$ | $\dot{\text{K}}$ | vol. |
|---|---|---|---|---|---|---|---|---|
| 1. Ste Marie, Canada, *red*, | 66·70 | 16·20 | 6·90 | 2·65 | 0·67 | [3·78] | | 3·10=100 |
| 2. Irasburgh, Vt., *gray*, | 78·70 | 12·80 | $\dddot{\text{Fe}}$ *tr.* | *tr.* | 1·23 | 5·57 | 0·89 | 0·60= 99 79 |
| 3. Roxbury, Vt., *gnh-gy.*, | 69 90 | 20·00 | | 1·80 | 1·51 | 2·33 | 1·45 | 2·40= 99·39 |
| 4. Pownal, Vt., *bh-gray*, | 42·90 | 42·20 | | 1·98 | 0·78 | 1·33 | 5·24 | 5·60=100·03 |

These analyses indicate that it is often impossible to distinguish the hydro-mica slates; for No. 2, although described in the Vermont Report as "the most unc-

(5) *Chloritic* varieties of the mica or hydro-mica slate occur in Tom Ball and the Taconic Mountains, in some of which there is as much chlorite as mica. They sometimes contain crystals of magnetite. [At the Rutland locality, referred to above, there is a true dark green *chlorite schist*, containing magnetite associated with the hydro-mica slate and quartzite.]

(6) *Fine-grained gneiss*, grayish to bluish gray in color; the feldspar and quartz in small grains and distinguishable from one another with difficulty. The fusibility of the rock shows that it contains feldspar. The rock is sometimes thick-bedded and of uniform color. A bluish gneiss of this kind is quarried for architectural purposes on Monument Mountain. A very common variety is strongly and closely banded with black and white through the alternation of very micaceous and quartzo-feldspathic layers. It affords thick slabs, and is quarried west of Great Barrington and elsewhere. A very *contorted gneiss* of this kind occurs, which, although containing much mica, has lost, through the contorting, all its schistose structure, and almost all traces of its bedding.

(7) *Granitoid gneiss*, a light colored, even-grained granite-like rock breaking into great blocks, the mica in small scales, mostly black. (Overlies mica schist and quartzite).

(8) *Whitish gneiss*, containing very little mica in distinct interrupted streaks and spots.

(9) *Hornblendic schist*, interstratified with the gneiss, of a blackish color.

(10) *Garnet rock*, interstratified with gneiss and gneissic quartzite, a dark gray rock, exceedingly tough, made up largely of pale reddish massive garnet, with quartz, and perhaps some feldspar (though not distinguishable), and few minute oblong whitish crystals of some hornblendic mineral. Contains minute particles of pyrites.

(11) *Tremolite rock.* The Canaanite of Canaan, a coarsely granular rock, white to grayish-white in color, often mixed with quartz and with limestone, and often containing scales of brown mica. Weathered specimens sometimes show the ends of crystals of tremolite.

(12) *Clay-slate* occurs alternating with quartzite and limestone in Williamstown, Massachusetts, and at Rutland and elsewhere in Vermont. That of the Taconic range west of Great Barrington lies mostly to the west of the mica or hydro-mica slate of that range.]

tuous of all the specimens analyzed," evidently contains, chiefly if not solely, an anhydrous mica, and this a *soda* mica, if there is any mica present. The analysis gives too little alumina in proportion to the alkali for any known species. No 4 appears to be almost solely the hydrous mica damourite; just such a rock has been recently described from Salm-Château, and called damouritic schist. Another analysis by Barker, not here cited, is of a chloritic variety.

C. *Quartzite beds.*—The rocks of the quartzite beds differ much in character. The principal kinds are the following.—

(1.) An intensely hard, gray or whitish quartzite, jointed profoundly and in more than one direction, but without distinct traces of bedding. Some beds are a conglomerate of the same hardness, made up of pebbles or stones from the size of a pea to that of cobble stones. Minute particles of pyrites are sparingly disseminated through a large part of this and other varieties of the quartzite.

(2.) A rock equally massive in fracture and almost as firm, but showing the bedding more or less distinctly. (It is often used for the hearths of furnaces.) Cleavable particles of a glassy feldspar are sometimes distributed through it. When thus laminated it often contains, especially over the surfaces of the laminæ, scales of a white mica or hydro-mica, and sometimes minute brown or blackish tourmalines.

A rock of this kind often weathers rapidly on exposure, so as to become very friable, or even fall to sand. (It is used for making glass in the region.)

(3.) *Soft sand-beds*, in thin layers, that change deeply to a dirty sand.

(4.) *Calcareous quartzite*, which graduates on one side into limestone and the other into quartzite. Some hard laminated quartzites are very porous as if they had once contained calcareous material.

(5.) *Gneissic quartzite* and *quartz-conglomerate.* A variety consisting partly of quartz pebbles half an inch to an inch in diameter containing large masses of orthoclase and much mica, and really a variety of gneiss.

(6.) *Feldspathic quartzite*, a quartzite, often very hard, containing much orthoclase through its mass. The orthoclase decomposes easily and becomes removed, leaving the rock cavernous, and thus, as Hitchcock long since explained, is produced the *buhrstone* of Berkshire. Besides the orthoclase, a glassy cleavable less alterable feldspar may often be distinguished in the so-called buhrstone, and sometimes in the walls of the cavities that had been made by the decomposition and removal of the orthoclase; it is probably either albite or oligoclase.*

The transitions between the different kinds of rock in the quartzite formation are often very abrupt. Only a few rods sometimes separate the regularly-bedded fragile quartzite from the hard bedless granular quartz. The soft sand-rock often has within it intensely hard masses made up of the sands of

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* I am indebted for specimens of this and the preceding variety of the quartzite to Dr. Stephen Reed of Pittsfield. They are from the valleys of Roaring Brook and Mill River, east of New Lenox.

many layers solidified together; the bedding stops short off in a mass of structureless quartzite, several yards, it may be, in breadth and height, which lies isolated, so that it might be taken for an intruded boulder were it not for the traces of bedding on some parts of its exterior.

When the rock of a quartzite stratum is the harder quartzite, it is sure to stand out over the region in bold crests, or make the precipitous brow of a mountain ridge. When it is the softer variety, the ridges may have gentle slopes, and lie deeply covered with earth, showing nowhere an outcrop of the beds. But ridges of this latter kind often have vast numbers of quartzite boulders over the surface, which one ignorant of the facts would suppose were all transported boulders, while they are partly at least the hard knots of the rotted sand rock.

These diversities in the quartzite serve to explain much that is mysterious in its apparent distribution. It does not answer the purposes of strict science to set down the plains along the valleys as all limestone areas; for the soil of these plains may rest on soft beds either of the quartzite formation, or of the mica schist; and we cannot infer from an outcrop of hard quartzite only a few yards in breadth that the concealed stratum below to which it belongs has no greater breadth.

Again, *following the direction of the bedding*, there are sometimes changes from quartzite to mica schist or gneiss. This is proved by the fact that the strata of the same north-and-south range, or in the direction of the strike, are mainly quartzite in one place, and in another, two or three miles off, are mainly schist. It is also suggested by finding a limestone stratum overlaid by schist with quartzite above the schist at one point, and in another not far distant by quartzite directly, the schist being wanting. This kind of evidence (to be given in detail in the continuation of this memoir) is not as positive as that from direct observation along exposed strata. Direct proof of the plainest kind exists, as I have seen, in the quartzite ridge northeast of Rutland, Vt. (colored as quartzite in the Vermont Geological map, Rep. of 1861); the schist (here mainly hydro-mica schist) is the prevailing rock of the ridge, and in some places it passes in the course of a hundred yards into true quartzite. The transition is so obvious and complete that no occasion exists for doubting with regard to analogous transitions in Berkshire.

These facts at the first thought seem strange. But we take little note among unaltered stratified rocks of the change along the bedding from a sandstone of purely siliceous sand to an impure sandstone; or, off an existing seashore, from the sands of a sand-flat to the mud of the shallow bottom but a few rods distant; and the difference we find in the Green Mountain rocks is only this small and often unimportant distinction made intensely apparent by metamorphism.

If then a bed of rock may be quartzite in one part and mica schist or gneiss in another, and if these rocks alternate with one another in the way mentioned, *there is not strictly any quartzite formation in the Green Mountains*; for the formation is made up of various rocks, and quartzite is not always the predominant one.

The kinds of rocks in the region under discussion have been here separately described with some detail because the fact is not generally appreciated that gneiss, granitoid gneiss, coarse and fine mica schist, hydro-mica slate, compact garnet rock, hornblende slate, chloritic rocks, as well as quartzite, soft and hard, may belong to the Stockbridge limestone formation, and even overlie it. Many of the rocks are precisely such as belong to the so-called "Green Mountain Series," which series has been pronounced on lithological evidence to be *pre-Silurian* and *Huronian*.

I have collected specimens of chloritic mica slate from the summits of Mt. Washington in the Taconic range; of Tom Ball; of the Graylock ridge, near South Adams, Mass.; of Mt. Mansfield, in the region of the Green Mountain series of rocks in Vermont; and from the ridge two miles west of this city (New Haven, Ct.), which are so closely alike as to appear as if broken from the same block or slab. Are these all of the same age? all Huronian or pre-Silurian? or all Trenton or Hudson River rocks? I have found staurolites (which Hunt makes as good as a fossil for distinguishing one series of pre-Silurian rocks) in the Taconic slates of Salisbury, Conn., underlying directly the Canaan limestone: Is the rock *therefore* of the White Mountain series and pre-Silurian? I have seen a slate abounding in staurolites alternating with hornblende rock, gneiss and quartzite, in Vernon, in southeastern Vermont, but a few miles north of the Bernardston region of either *Lower Helderberg* or *Devonian* quartzite, slate and limestone (crinoids an inch in diameter of stem occurring in the beds), and, as the Vermont Report states, the quartzites of these adjoining towns are probably the same rock: Are *these* beds of the White Mountain series and pre-Silurian?

We learn from the facts how much virtue there is in lithology for determining the equivalency of metamorphic rocks. It may afford a quick answer to hard questions, but its answer is worth very little unless otherwise abundantly fortified.

[To be continued.]

## ART. XLIII.—*On the nature and duration of the discharge of a Leyden Jar connected with an induction coil;* by OGDEN N. ROOD, Prof. of Physics in Columbia College. Part III.

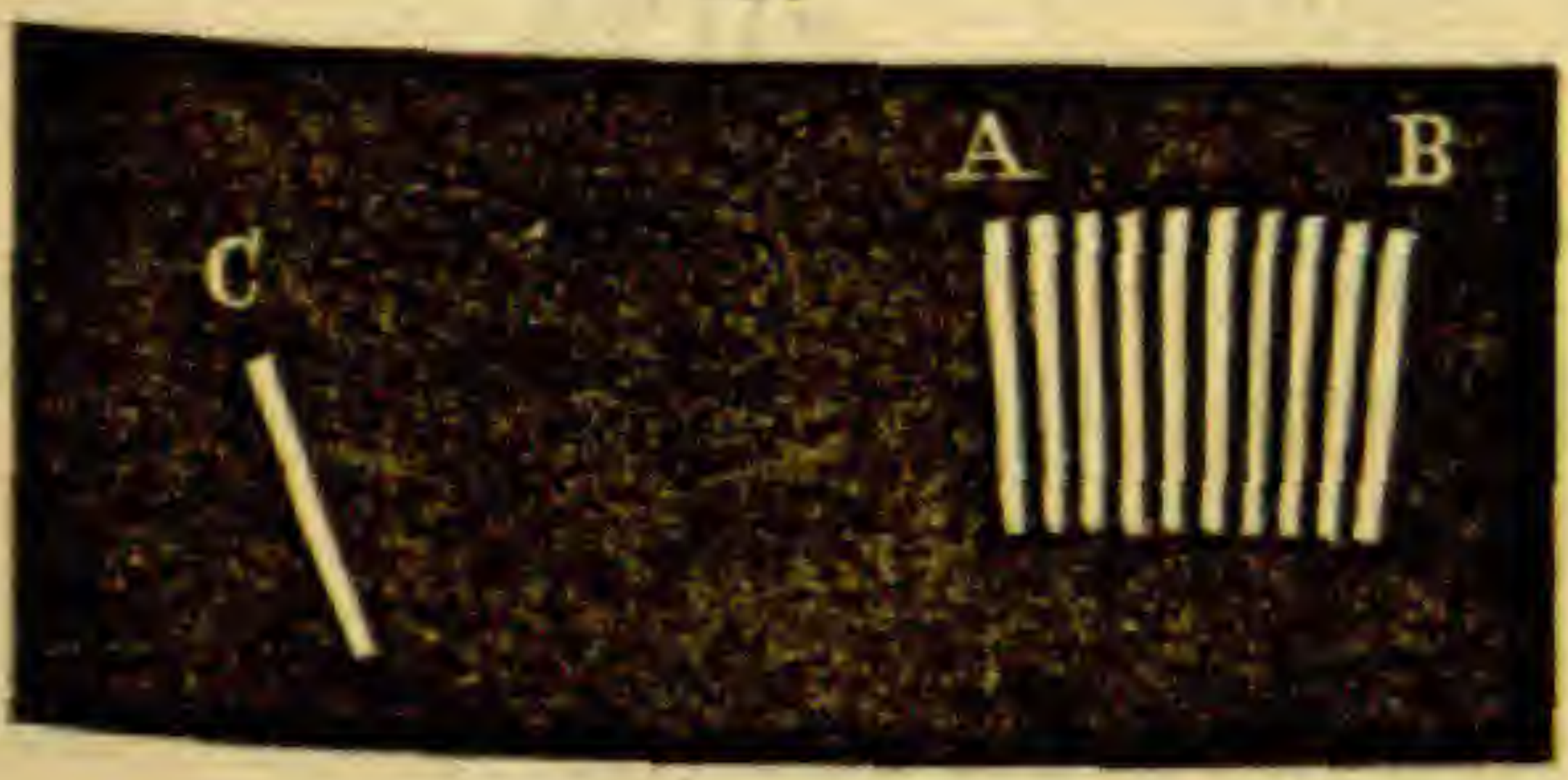
(Concluded.)

### *Form and duration of the discharge of the small jar with brass balls as electrodes.*

The form of the discharge under these circumstances was more complex; the succession of acts being generally as follows: first, an instantaneous spark, which was followed by a pale violet discharge, lasting a small fraction of a second; afterward came a series of instantaneous sparks whose number diminished as the striking distance became greater. Fig. 5 shows about the appearance with the mirror, fig. 6, with the disc, the latter giving more reliable information regarding the actual number of discharges. The length of the simple induction spark in these experiments was 57·6 millimeters.

5.

6.



*Striking distance* 1 *millimeter.*—The total duration was somewhat variable; the average of the experiments were,

| | |
|---|---|
| Average with the mirror, | ·0247 sec. |
| " " " disc, | 0276 " |
| Final average, | ·0261 " |

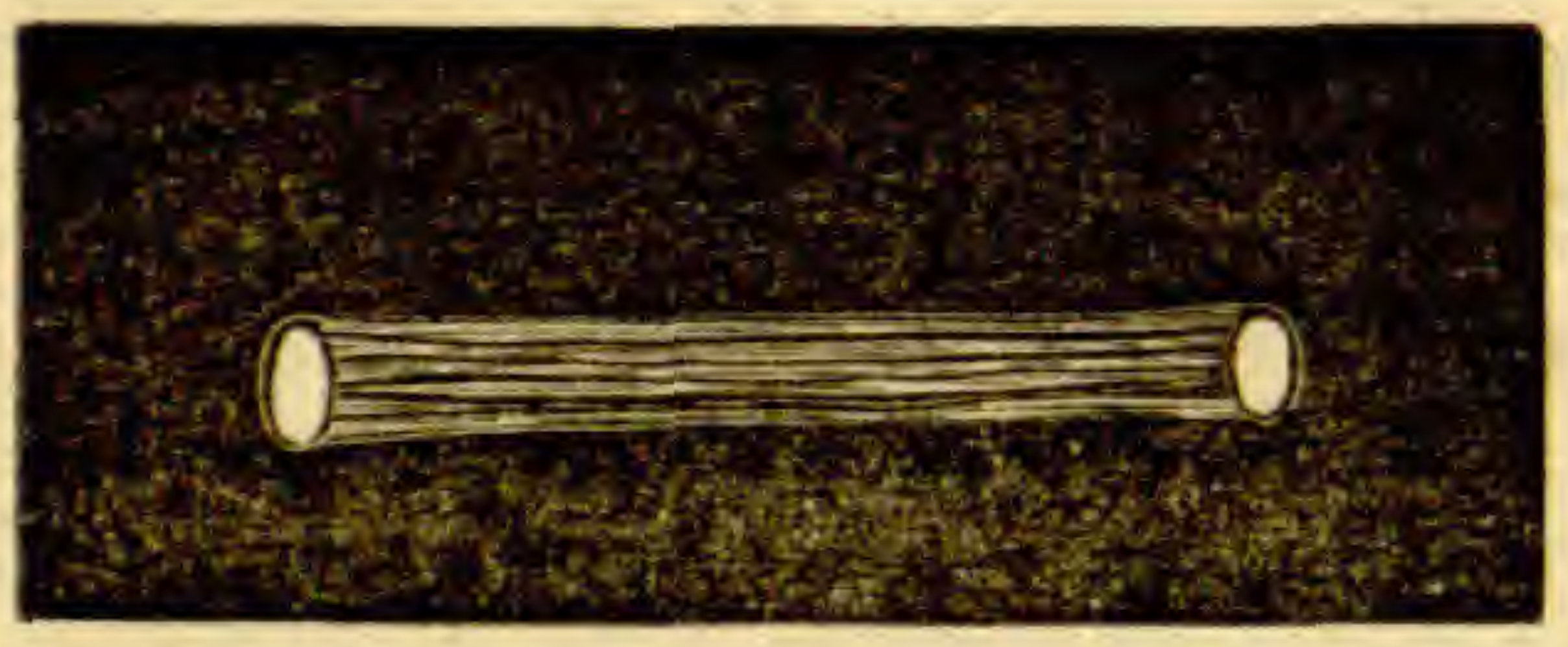
The portion A B, figs. 5 and 6, consisted of from ten to twenty instantaneous sparks, its duration varying with the number of included discharges. Disc observations gave for

| | |
|---|---|
| Maximum duration of AB, | ·0119 sec. |
| Medium " " " | ·0090 " |
| Minimum " " " | ·0053 " |

Taking then ·0090 as the medium duration, and fifteen as the medium number of discharges, we have for the average interval separating the individual sparks ·0006 sec. This will give for the medium duration of the violet portion CA ·017 sec.

*Other forms.*—A quite different form was sometimes present, like that in fig. 7, and consisting of a faint violet streak terminated at each end by an instantaneous spark, the duration being, ·026 sec. to ·030 sec.

7.



These, from their rarity, were a little difficult to study, and I am not sure that in all cases the act was *terminated* by a spark. The violet streak was of course invisible in disc experiments.

*Striking distance* 2 *millimeters.*—The form was the same as fig. 5, but the dimensions were somewhat reduced.

Total duration with mirror,................ ·01830 sec.
" " " disc, ................. ·01588 "

Average, ................ ·01709 "
Average duration of portion AB, .......... ·005 "

The other form (fig. 7) also sometimes occurred with a duration of ·027 sec.

*Striking distance* 3 *millimeters.*—The form was still the same; the portion AB consisting sometimes of only three or four sparks, but often of eight or ten.

Total duration with mirror,.................. ·0124
" " " disc, ................. ·0125

The form of fig. 7 sometimes occurred, apparently with a duration about twice as great as that just given.

*Striking distance* 4 *millimeters.*—The form was sometimes the same as those just given, but more often consisted solely of a compact series of from four to six instantaneous sparks.

Duration with four sparks,................ ·0043 sec. } disc.
" " six " ................ ·0082 " }

*Striking distance* 5 *millimeters.*—Merely two, three or four isolated sparks.

Average total duration,................. ·0035 sec. (disc).

*Striking distance* 6 *millimeters.*—Same form as the last,

Average total duration,.... ............ ·0025 sec. (disc).

*Striking distance* 7 *millimeters.*—Two, three, rarely four sparks.

Average total duration,.................. ·0018 sec. (disc).

*Striking distance* 8·75 *millimeters.*—Only two sparks, one quite faint. Interval between them ·0009 sec. (disc).

*Striking distance* 10 *millimeters.*—One spark; seldom two. 10·75 was the maximum striking distance; at it, and half a millimeter under it, only a single spark was produced.

*Form and duration of the discharge of the small jar with platinum points as electrodes.*

The length of the simple induction spark was 58 millimeters.

*Striking distance* 1 *millimeter.*—The form varied considerably, three or four different kinds being mingled. The simplest form consisted of ten or twelve instantaneous sparks following each other at a pretty regular interval, which increased somewhat toward the close of the act. This kind of discharge produced a short hissing sound like that obtained by thrusting a red hot wire in cold water, and its presence could be detected by the ear alone. The total duration of this form was subject to considerable variation; below I give

| | |
|---|---|
| The greatest, | ·0099 sec. |
| " least, | ·0037 " |
| " medium, | ·0068 " |

The last quantity was not obtained as the mean of the other two, but is the average of eleven distinct experiments with the disc and mirror. These I give as a sample of the variation in the results sometimes obtained in these experiments, the discordance being mainly due, not to the methods, but to the phenomenon itself.

| | sec. | |
|---|---|---|
| | ·0047 | mirror and compass. |
| | ·0099 | " " " |
| | ·0064 | " " " |
| | ·0047 | " " mercury tube. |
| | ·0064 | " " " " |
| | ·0074 | disc. |
| | ·0092 | " |
| | ·0079 | " |
| | ·0037 | " |
| | ·0085 | " |
| | ·0063 | " |
| Average | ·0068 | |

If we take the average number of sparks as ten, we shall have for the average interval separating them ·0007 sec., that is, these sparks were generated at the rate of 1428 per second. I suppose that a combination of ten or fifteen of them would be sufficient to render the ear sensible of the *tone* produced; but owing to the irregularity of their position, instead of furnishing an approximation to a musical tone, the sound resembled rather that of the combination of the consonants, ST. By swiftly drawing a card or piece of thin sheet brass over the edge of an irregularly notched plate of brass, this sound could be imitated.

*Other forms.*—Quite often the discharge was like that given in fig. 7, with a total duration of ·017 sec. More rarely the form in fig. 5 was produced, but I could obtain no good measurement of it. In *one* case its average duration was doubtfully estimated to be ·005 sec.

*Striking distance* 2 *millimeters.*—Form was generally like fig. 4, merely ten or twelve instantaneous sparks; total duration ·0075 sec. Sometimes forms like fig. 7 were seen.

*Striking distance* 3 *millimeters.*—Like the last, with perhaps twenty sparks; total duration ·0088 sec. Forms like fig. 7 were sometimes seen.

*Striking distance* 4 *millimeters.*—Fifteen or twenty sparks; total duration ·0081 sec.

*Striking distance* 5 *millimeters.*—Form same as the last; number smaller; total duration ·0073 sec.

*Striking distance* 7 *millimeters* —Same form; total duration ·0067 sec.

*Striking distance* 9 *millimeters.*—Form the same; total duration ·0060 sec.

*Striking distance* 10 *millimeters.*—Same form; total duration .0046 sec.

*Striking distance* 12 *millimeters.*—Same form still; only four or five sparks; total duration ·0041 sec.

*Striking distance* 15 *millimeters.*—Like the last, consisting of two or three sparks, total duration ·0020 sec.

The maximum striking distance was 24·5 millimeters, when the discharge consisted of only a simple instantaneous spark, and this was true, down to distances as small as 21 millimeters. It will be noticed that the total duration of the discharge in these experiments was not, as formerly, a maximum at the smallest striking distance, but became so at 3 millimeters, which I am disposed to account for by the circumstance, that at the smaller striking distance, many of the heavier discharges assumed the form of figure 7. Some general correspondence between the results with brass balls and platinum points will also be noticed.

These experiments show that a single excitation of the coil supplies sufficient electricity to repeatedly charge jars of moderate or small dimensions, this happening not only on account of the amount of electricity generated, but also because the act of its production is spread out over a certain interval of time. In this connection, then, it becomes of interest to examine and measure this interval in the absence of the jar, and to ascertain its relation to the total duration of the multiple discharges which have just been described. It has long been known that the simple induction discharge consists of an instantaneous spark which is followed by a violet light, the latter lasting during a rather large fraction of a second. This is called the *aureol*, and has been studied by several physicists.

As their results, nevertheless, would have no particular application to the matter in hand, I made a new set of experiments with the same apparatus, using also the same electrodes and battery.

*Duration of the aureol with brass balls as electrodes.*

| Striking distance. | | Duration. | |
|---|---|---|---|
| 1 | millimeter. | ·026 | sec. |
| 2 | " | ·015 | " |
| 3 | " | ·012 | " |
| 4 | " | ·009 | " |
| 5 | " | ·006 | " |

At a striking distance of ten millimeters the aureol was not visible. The light of the aureol corresponding to one of the electrodes was violet, that due to the other had a hue approaching red. With small striking distances the two streaks were in contact, but separated as the distance between the electrodes was increased.

*Duration of the aureol with platinum points as electrodes, the length of the simple induction spark being* 48·7 *millimeters.*

| Striking distance. | | Duration. | |
|---|---|---|---|
| 1 | millimeter. | ·022 | sec. |
| 2 | " | ·020 | " |
| 3 | " | ·018 | " |
| 10 | " | ·011 | " |

With ten millimeters only one of the streaks was visible; as before, they were red and violet.

In both of these experiments the duration of the aureol increases as the striking distance diminishes, and it is evident that with distances under a millimeter still greater durations would probably be obtained. Now while the durations in these experiments for a striking distance of one millimeter do not represent the actual time during which the induction current flowed into the jar, they do prove that it occupied at least as great an interval as ·026 sec., which is the maximum duration of the multiple discharges described in this paper.

The general result obtained in these experiments may then be summed up as follows: if a Leyden jar, of a selected size, with a fixed striking distance, be connected with an induction coil, the act of exciting the coil will charge and discharge it at *once;* as the size of the jar is diminished, it will be charged and discharged oftener, and the aureol will begin to mingle itself with the spark discharges, and as the size still further diminishes, will finally replace all of them except the first; for it is evident that we may regard two brass balls, with an intervening layer of air a millimeter thick, as a minute Leyden jar, representing the last of the series.

The combination of the aureol or violet discharge with the multiple sparks, is, I think, to be explained thus: the first spark heats and rarefies the air between the electrodes; if then the electric current is furnished with sufficient rapidity by the coil, the tension of the electricity in the jar may rise sufficiently for a discharge through the rarified air before it can cool down, and thus produce the violet light. This state of things would continue till the electricity began to be furnished more slowly by the coil, when it would result that, the air between the electrodes having time to cool down would no longer permit the electricity to pass through it in an unbroken stream, but would compel it to discharge itself in sparks. According to this idea the successive sparks ought to be separated by a gradually increasing interval, and this indeed appears to be the case, the rise of the separating interval being particularly strongly marked toward the close of the total act. Brass balls favor this mixed form of discharge, perhaps, by confining the air to a certain extent, and thus preventing it from cooling down. Platinum points can have no such influence, and with them this phenomenon is rare. With the larger jar it never occurred, as the air was always able to cool down before the electric tension had sufficient time to rise in its larger surface, so as to warrant a discharge. With the small jar and longer striking distances, the violet light was not produced for an analogous reason, and also perhaps because the air was less confined.

In using the multiple sparks for investigations where an instantaneous illumination is required, the presence of the aureol-like discharges cannot occasion trouble, owing to their slight luminosity and large duration, the latter being enormous when compared with that of the spark. In the experiments described in the second part of this paper, this violet light was never visible although it must often have been present, and I was enabled to make the determinations here given only by a reduction of the distance between the mirror and the ground glass, and the use of a lens of vastly larger angular aperture. The large number of sparks (15 or 20), given by the small jar with a suitable striking distance, is admirably adapted for certain purposes of investigation, and enables the experimenter to dispense with arrangements for controling the moment at which the discharge take splace. If single, non-multiple sparks are desired, the indications I have given will readily guide to their production, it being always preferable to test with a revolving disc making fifteen or twenty revolutions per second, and provided with a small open sector of ½°.

I will also call the attention to the circumstance that where Leyden jars in connection with induction coils have been used in experiments on binocular vision, etc., the results are not to be relied on as having been obtained with an instantaneous illumination, as is abundantly shown by the large total durations so often met with in this investigation.

Finally, by the use of large rotating discs, these multiple discharges can readily be exhibited to moderate sized audiences.

New York, June 29th, 1872.

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## Art. XLIV.—*On the Allegheny System of Electric Time Signals;* by Prof. S. P. Langley.

The necessity of a uniform standard of time for the railways of the United States is one which is growing into importance with the increasing extent of our railway system; and we are, ere long, in this country, to be called on to settle for ourselves a practical problem which has already been solved in England, and which is beginning to make its demand for solution upon the managers of our railroads.

Since past experience shows that their probable adoption of a new and common standard will introduce it to public notice and discussion, and then to adoption by cities and individuals, it is desirable that this should not be done without the direction which intelligent scientific coöperation will give to a movement originated by the demands of intercontinental traffic. As few are aware how generally this coöperation has already been invoked, nor to what extent the public is indebted to observatories for increased security of transit, it has seemed that an account of what has been done in this direction in any one of them would be of interest.

The earliest introduction of the system of electric automatic transmission of time-signals, on an extended scale, appears to be due to the observatory of Greenwich.

The Astronomer Royal, with Mr. C. V. Walker, commenced their use in 1852, carrying for that purpose special wires on the poles of the South Eastern Railway from Greenwich to London Bridge. The subsequent extension of the use of Greenwich time under this system has been almost universal throughout the United Kingdom, the observatories of Glasgow and Liverpool, under the direction respectively of Professor Grant and Mr. Hartnup, as well as that of Edinburgh, having taken part in bringing it to its present condition of utility. For an instructive and very full description of the methods employed at Greenwich, reference may be made to an article in the Horological Journal for April, 1865, by W. Ellis, Esq., F.R.A.S., to whom, as to all the gentlemen named, the writer has been indebted for much kindly-given information on the results of their long experience.

Although the introduction of the plan in this country has been comparatively recent, the number of American observatories which thus distribute time, is so considerable that the most partial account of their methods, and the extent of their work, would exceed the limits of such an article as the present. In this, the only arrangements described are those in use at the Allegheny Observatory, with which the writer has become familiar from the responsibility of their initiation and superintendence. It is proper to add, that were he writing a history of the progress of electric time signals in the United States, other observatories which have before employed not dissimilar means, would receive earlier mention, and that his own part in introducing these signals at the Allegheny Observatory has been less the contribution of any novel device, than an adaptation of what seemed the best features of plans in use abroad, their arrangement in a form adapted to the needs of American railways, and the supervision of their application to the wants of cities and individuals.

In doing this, a great number of ingenious devices have been examined, and if the system to be described appears to be one of the simplest, it has yet been reached only after much care in setting aside all which would not bear the test of practical trial.

The subject was first specially considered at the Allegheny Observatory some three years since, and a plan was arranged for the managers of the Pennsylvania Central Railroad in 1869. Previously to this, however, at the request of some jewellers of Pittsburg, the time had been transmitted to their stores, at a distance of some miles from the observatory. The system now described has been in use for nearly three years, in furnishing the Pennsylvania Central Railroad with its official standard of time; and by it the time is now sent daily to Philadelphia on the east, as far as Lake Erie on the north, and to Chicago on the west, regulating the clocks on a number of minor roads over whose wires it goes, as well as on those of the principal southern lines connecting the Atlantic with the Mississippi. Thus passing, as it does, over several thousand miles daily, it is believed to be at present one of the most extended systems of time distribution in the world.

The observatory is on the summit of the ascent on the northern side of the valley of the Ohio, about two miles in a direct line from the offices of the Western Union Telegraph Company in Pittsburg. and rather more from those of the Pennsylvania Central, and Pittsburg, Fort Wayne, and Chicago roads. It is connected with these points by three independent lines of telegraph.

One of these runs to the Western Union offices, and thence to the stores of a considerable number of jewelers in Pittsburgh. This is called the "Jewelers' line." The second, connecting the observatory through the offices mentioned with Eastern Pennsylvania and New Jersey Railways, and also with Chicago, is known as the "Railroad line." The third, consisting of a double wire or "loop," communicating with the city, is employed occasionally for the observatory's own messages, and when (as, for instance, in longitude determinations) it is wished to send sidereal time, without interrupting the regular transmission of signals from the mean-time clock. In the transit room, in the western wing of the observatory, are kept the sidereal clock by Frodsham, of London, and the principal mean-time clock by Howard, of Boston.

On the escape-wheel arbor of this, the standard mean-time clock, and turning with it, once a minute, is a wheel cut with sixty sharp radial teeth, of which those corresponding to the 50th, 51st, 52d, 53d, 54th and 59th seconds of the minute have been removed by a file. Near the clock is a "repeater," the circuit through whose coils passes through a local battery, through a second clock in the computing room, and then through the standard clock. Each wire terminates in a delicate spring, close by the wheel just mentioned. While the extremities of these springs, which are shod with gold and platinum, rest in contact, the circuit is unbroken; it is opened by the minutest lifting of one from the other, and this is effected automatically by means of a ruby attached to one of them, and placed within reach of the wheel above mentioned. As each of these teeth passes, the ruby, and with it the spring, is lifted through a minute distance. (Not in practice more than one one-hundredth of an inch, and usually much less.) Once a second, therefore, the circuit is opened during a period of probably less than a twentieth of a second, and as the wheel advances a tooth with each vibration of the pendulum, the armature of the repeater is raised each second of the minute, until the 49th is completed.

Since the teeth corresponding to the next five seconds have been filed away, during those seconds the jewel is not touched, nor the circuit opened. The consequent silence of the "repeater's" beats draws attention to the fact that the end of the minute is approaching, its completion being indicated by the short pause caused by the absence of a tooth at the 59th second.

This action is repeated in every minute of the twenty-four hours without variation. The particular second is thus identified, but one minute is (so far as the action of the standard clock is concerned) not distinguished from another. To do this is the work of the subsidiary clock in the computing room, through which the local wires are led, as has been mentioned. This subsidiary clock (made by Howard of Boston) may be called for distinction the "journeyman," and its principal office is not to give the time, but to interrupt the circuit, which it does on or near the completion of the 58th minute, closing it again about half a minute before the completion of the hour. When the circuit is opened by the journeyman, the repeater is silent for a minute and a half; when it is closed, the standard is again heard ticking on the repeater, and the ensuing short pause evidently precedes the first second of the first minute of the hour. The time is thus wholly derived from the standard clock, and is independent of any other, the journeyman having no power to control or in any way react upon the primary, and being able only to interrupt the message it sends; not to falsify them.

The mechanism for effecting the transmission of the time is essentially that already described, but more is needed to ensure against possible interruption. This may occur from several causes, prominently from oxidation of the platinum or gold contact surfaces; when the current must be interrupted while they are cleaned, if there be no other clock. To meet this contingency, a chronometer of peculiar construction was made for the observatory by Frodsham. It resembles the ordinary marine chronometer in external appearance, but contains in miniature the apparatus for breaking circuit already described, the wheels being cut so as to give the same signal of the approaching end of the minute, as the standand clock. The peculiarity consists less in this, however, than in a device by means of which it can be caused to gain or lose any fractional part of a second, or any number of seconds, without being stopped, and without any disturbance of its normal rate, except while the change is being effected. This chronometer is to replace the prime clock in the circuit, during any temporary stoppage of the latter for repair or adjustment.

The mechanism which has just been described, acts in connection with the local circuits of the observatory; one battery being employed for the sidereal clock and chronograph, and another for the mean-time standard. Any interruption of the main external circuits is shown at once, by the action of a galvanometer in each, which makes an audible and visible signal when the circuit is opened. The accessory apparatus, such as batteries, relays, switch-boards and so forth, which are used in every telegraph office, it will be superfluous to describe here in detail, but before following the operation of the electric current, outside the observatory, it will be well to speak of the method which has been adopted as likely to ensure most accuracy in the time keepers which control it.

The transit instrument in the western wing, is of four inches aperture, and with it and the chronograph, observations for time are made on every fair night of the year, except on Sunday, when if complete determinations have been made on the preceeding night, none are taken. The instrument is of sufficient power to follow the principal Nautical Almanac stars in the day, and these are used (or more rarely the sun), when the weather permits, if the usual night observations have been missed. From three to six stars are customarily taken, the azimuthal error of the instrument being found from the observations of each night, after the other corrections are applied, and the results determined from the chronograph and the sidereal clock. The mean error in the resulting determination of the sidereal clock correction, is from three to four hundredths of a second, but it cannot be assumed that that of the mean time standard is known within these limits, except at the time that the observations are freshly made.

It may be desirable to point out where the system pursued here differs from that in which a few signals are sent at stated hours, as at Greenwich. In the case of the time ball for instance, dropped daily by a clock at Greenwich mean noon, it is customary to compare the mean time clock which drops it, with the sidereal time a few minutes before twelve. If it (the operating clock), be slow, it is caused to gain, and if fast, caused to lose, an amount needed to bring it to coincidence before the automatic action gives the signal.

The time of this signal is nominally exact, but in fact involves the variations in rate of the standard clock or clocks, which are treated in the comparison as having their errors absolutely known. It is by no means meant to criticise this procedure, but to point out that as error must exist where the rates of the clocks are treated as constant in the intervals between observation, no less real accuracy is reached in the method employed here, in which (as the signals are being constantly sent) the signalling clock has no less nominal error at noon (for instance), than at any other hour.

When the sidereal clock has entered its beats upon the chronograph during the time of observation, the record is not interrupted until the mean time standard having been put into the same circuit, both clocks have automatically entered their time on the sheet together, and the break-circuit chronometer has done so also. The sheet being removed, and the breaks of the transit observer measured, the comparison of the various clocks with electric attachments are taken by measurement on the same sheet, and the others compared with the sidereal clock by noting coincidence of beats by ear. The resulting errors of all are then determined, reduced to a common epoch, and entered in a permanent record kept for the purpose in the following form,

($\Delta$T, $\delta$t, being the usual symbols for the respective corrections of error and rate):

Aug. 10, 1872. Time-stars $\left\{\begin{array}{l}\eta \textit{ Herculis,}\\ \alpha \textit{ Camelop.,}\\ \kappa \textit{ Ophiuchi,}\\ \delta \textit{ Herculis,}\end{array}\right.$ A. E. F., observer.

| At mean $9^h$ | | $\Delta$T. | $\delta t$. |
|---|---|---|---|
| Sidereal clock, | $+$ | $7^s{\cdot}32$ | $+1^s{\cdot}18$ |
| Break-circuit chronr. | $+$ | $2^m{\cdot}22^s{\cdot}18$ | $+3^s{\cdot}30$ |
| Chron. 3242, | $+$ | $50^s{\cdot}05$ | $+3^s{\cdot}11$ |
| Mean time standard | $-$ | $00^s{\cdot}27$ | $+0^s{\cdot}46$ |

The mean time clock is here $0^s{\cdot}27$ fast by actual observation, but when the next comparison is made the following morning (at 21 hours), its error can usually be obtained only by comparison with another clock. If it be compared with each of the other clocks in turn, each, owing to the variations of its rate during the night, will probably give a slightly different result, but supposing all the time-keepers equally reliable, the probable error will be less, in taking the mean of the four, than by any single one.

The above corrections for error and rate having been applied to the sidereal clock; a comparison is taken with it in the morning, and the resulting time of the mean-time clock noted, on the assumption that the sidereal clock is an exact standard. The same comparison is made with each, after the respective corrections and rates have been applied, each being successively treated as an independent standard.

The results will then be entered in this form.

1872. August, $10^d$ $21^h$.

| | | |
|---|---|---|
| Error of mean time standard, | $-\ 0^s{\cdot}19$ | (by sidereal clock.) |
| " " " " | $-\ 0^s{\cdot}05$ | " break-circuit chron'r. |
| " " " " | $+\ 0^s{\cdot}11$ | " chron'r 3242. |
| " " " " | $-\ 0^s{\cdot}04$ | " its own rate. |

The mean or "adopted" error of the mean time standard is then

$$\frac{-0^s{\cdot}17}{4} = -\ 0^s{\cdot}04.$$

In the absence of any more absolute criterion, the time of the standard in this instance is assumed to be four one hundredths of a second fast, and this value is adopted and treated as though it represented an error determined by direct comparison with the stars. The clock will be compared again at 9 in the evening, and when this "adopted error" exceeds $0^s{\cdot}25$, such a change is made in the pendulum as will correct the error; not abruptly, but gradually during the ensuing twelve hours.

It is of course impracticable to stop the clock and raise or lower the adjusting screw twice daily for such minute corrections, and many ingenious devices have been proposed for effecting the change without stopping the instrument. One of these as applied to a chronometer has already been referred to; another (employed at Greenwich) involves the use of a small bar magnet permanently attached to the pendulum, and swinging with it; and still another, the changing tension of a long spiral spring which connects the "bob" with the clock-case.

After considering many such plans, that adopted was the old one familiar to most observers, of placing weights on the top of the bob of the pendulum, and then adjusting the bob by the screw till it runs with them approximately, after which a small increment or decrement of the weights will keep the clock under control. This plan has the advantage of employing as an agent, gravity, whose effects can be reckoned on with more certainty than those of electricity, or the tension of a spring. In common with the others it has, however, as commonly employed, the defect that when changes are made daily or oftener, the *rate* of the clock cannot be ascertained, and that reliance must be placed at the times of comparison only on other clocks whose rates are undisturbed. The writer has, therefore, found it advantageous to use these weights quantitatively, by making them of a size such as to cause a gain of 1 second a day; ·$^{s}$01 an hour, etc. Weights representing three or four seconds are kept on the top of the bob, so that their removal will retard the clock if desired to that amount.

A record is kept in which the comparisons in the tabular form above given are entered twice daily, the amount of the weights and the consequent rate which the clock so controlled would have had with an undisturbed pendulum, being noted likewise.

The barometer and clock-case thermometer are also read twice daily for the purpose of laying down curves representing the separate effects of temperature and pressure. Another curve, whose ordinates represent the algebraic sum of the corresponding ordinates of the first two, shows the combined results of both, for comparison with still another representing the clock rates. These are chiefly useful in the occasionally long intervals of cloudy weather which occur in winter. At such times the clock rates are obtained by interpolation from the curves, and "weighted" according to the degree of dependence to be placed on each clock, before making up the final or "adopted error" of the standard. When observations are obtained daily, however, such precaution is needless.

Those who are aware of the number of patented devices for controling distant clocks by electricity, may perhaps feel surprise that so little mention has here been made of their use.

Some of these are of extreme ingenuity and much promise, and the English patents covering such points are alone to be reckoned by scores. Plans have been submitted to the writer by which the clocks along any number of miles of road could be set right, and brought to uniform time in a few seconds, by the operator at the observatory, and these plans appear feasible. The arrangements adopted here, as the reader will observe, do not greatly differ from those employed in telegraphic determinations of longitude, and in fact a prolonged examination of very many ingenious devices for directly controling distant clocks led the writer to set them all aside, and to employ methods not differing in principle from those in use already, for purely scientific ends, in most American observatories.

Of the very numerous plans for controling distant clocks, that of Jones (now well known) appears to be the best, but even this is not quite reliable where the circuit is a long one. The clocks described have subsidiary apparatus enabling them to send controling currents on the Jones' plan, but thus far its use has been confined to the observatory. The whole work, external to the observatory, has therefore been hitherto done by the sending of signals, through which distant clocks may be *regulated*, but without employing means for their *control*, and though this is done over a very extended field, a brief description of it, under the three divisions into which it naturally falls, will suffice.

1st. The supply of time to watchmakers and jewellers. The "jewelers wire" passes through the Western Union Telegraph offices, and the stores of the principal jewelers of Pittsburgh. Beside each "regulator" is a telegraphic sounder, on which the observatory time is heard constantly ticking, and by which almost if not quite all of the clocks and watches of the city are thus at second-hand regulated. There is, in this uniform and recognized standard, everywhere accessible, a convenience to watchmakers of course, but still more to the public, as the discrepancies between clocks, public or private, which cause so many lost minutes in the day to each person in a city, that their aggregate represents a large draft upon the time of the business public, disappear.

Applications have been received from watchmakers in neighboring cities and at a considerable distance from Pittsburgh, for this telegraphic supply of the time, which it has not always been possible to accommodate, but which have been welcome as showing a public appreciation of the utility of the work.

2d. The supply of time to railroads. The watchmakers and jewelers are in permanent telegraphic connection with the observatory by a wire which is devoted to their use, but distant cities, such as Chicago or Philadelphia, can be reached only by the wires of the telegraph or railroad companies, which are too valuable to be exclusively employed for this purpose. The method used on the Pennsylvania Central, and Pittsburgh, Fort Wayne and Chicago roads, will sufficiently illustrate the system as applied to railways.

A special wire connects the observatory with the office in which the wires owned by these roads unite. In this office is a small bell which is struck lightly every second, in the manner described, and except for the pauses to designate the minute and hour, continues to sound unintermittingly; affording to the conductors and other employees specially concerned in the time a means of ready comparison, even without entering the building.

At 9 and at 4, Altoona time (ten minutes fast of Pittsburgh), the Pittsburgh operator in charge connects the main eastern wire to Philadelphia, 354 miles distant, with the observatory, and for the ensuing five minutes the beats of the Howard mean-time standard are automatically repeated on similar bells, or on the customary "sounders" in Philadelphia and in every telegraph office through which the road wire passes; all station clocks and conductor's watches being compared with it as the official standard. After five minutes the clock is "switched" by the Pittsburgh operator out of the main line wire, which is returned to its ordinary use.

A similar set of signals, lasting for five minutes, is sent at 9 and 4 of Columbus time (13 minutes slow of Pittsburgh) to all stations as far west as Chicago inclusive, in the main western line (of 468 miles in length). At Philadelphia the time is repeated to New York, at Harrisburgh to Erie (333 miles), etc. As it is thus sent not only over the main lines from New York to Chicago (nearly a thousand miles), but over a number of subsidiary or branch roads too great for enumeration here, and which form in the aggregate a much larger number of miles than the main trunk, it will be observed that a considerable fraction of the railway system of the whole country is prepared for using a single unit of time, as, though the names of "Philadelphia time," "Altoona" or "Columbus time" are not yet abolished over that part of our railway system referred to, every railroad clock and watch, and the movement of every train, is regulated from a single standard, that of the clock in the observatory.

The advantages of this uniform and wide distribution of exact time in facilitating the transportation of the country, and in enhancing the safety of life and of merchandize in transit between the Western and the Atlantic cities seem to be sufficiently evident. The fact that the system, described in this article, has obtained the extension it has, within three years from its commencement will, it may be hoped, justify the belief that its use has proved not only valuable to railways, but an added security to the safety of the public.

3d. Supply of time to cities. At present, arrangements are in progress for regulating the principal public clock of Pittsburg, (the turret clock of the City Hall, about two miles from the observatory), which it is intended shall strike every third hour on the bells of the fire alarm, and probably also in the various police stations. As the mechanism for doing this is still in course of construction, and may yet be modified in trial, it would be premature to speak of it, especially as its success has not yet been proven in practice here. The city clock will automatically report its own time to the observatory by a special wire, and it is probable that in controling its rate from the observatory, the "Jones" system will be used.

The necessity of a uniform standard of time over the whole country, which was alluded to in the outset as one of growing importance, has not been further directly touched upon in this article, which is yet as a whole devoted to describing the means of meeting it. The evident tendency, in thus sending the time from one standard over so large an extent of territory, is to diminish the number of local times, and so prepare the way for a future system, in which, at least between the Atlantic and the Mississippi, they shall disappear altogether.

A step in this direction has been contemplated by the managers of the roads uniting New York, Philadelphia, Pittsburgh and Chicago, who have intended to use the time of the meridian of Pittsburg between the two extreme points mentioned, running all trains from New York to Chicago by this time alone, in place of using successively the local times of Philadelphia, Altoona and Columbus, as at present. Such a change would have already taken place during the last summer, except for an unexpected cause of delay, on whose removal it will be effected.

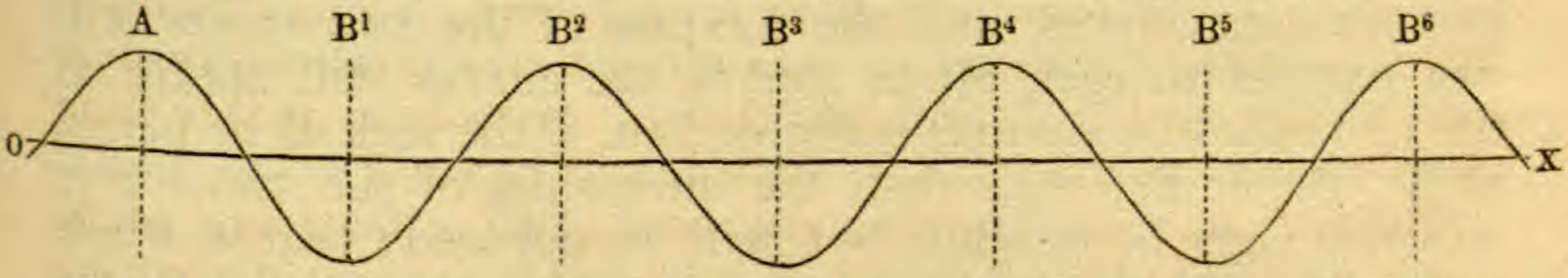
The labors of this and of other American Observatories are tending to the same important end, that of the ultimate adoption of some single time for all the country east of the Mississippi, by which not only the railroads, but cities and the public generally, will regulate themselves. What point shall be chosen is of less importance than that some *one* shall be used and universally.

The subject is one which has hitherto attracted little public attention, but it does not seem unsafe to make the assertion, that the causes which have almost insensibly effected such a revolution in England, will in a few years more bring it about here.

**Allegheny Observatory, Allegheny, Penn., Sept. 22, 1872.**

## ART. XLV.—*On a method of detecting the phases of vibration in the air surrounding a sounding body; and thereby measuring directly in the vibrating air the length of its waves and exploring the form of its wave-surface;* by ALFRED M. MAYER, Ph.D., Member of the National Academy of Sciences. Professor of Physics in the Stevens Institute of Technology.

THE curve A, $B^2$, $B^4$, etc., is the well known symbolic representation of the dynamic condition of the air, at a given instant, when traversed by simple sonorous vibrations. Those portions of the curve above the axis OX represent the length and manner of the aerial condensations, while those flexures below the axis represent the rarefactions; therefore similar points of the flexures above the axis, or similar points in the flexures below the axis, represent like phases of vibratory motion. Imagine these conditions of the air produced by a body vibrating at A; then the distance A to $B^1$, $B^1$ to $B^2$, etc., will equal a half wave-length, while from A to $B^2$, $B^2$ to $B^4$, etc., will represent a whole wave-length, corresponding to the note given by A. If another sonorous body B, giving exactly the same note as A, be placed anywhere on OX, it will have vibrations communicated to it from the vibrating air which touches it, and it will vibrate exactly with the air, almost as though its substance was of the air itself. Now imagine this body B placed at $B^2$, or at $B^4$, or $B^6$, etc., then its phases of vibration will be exactly similar to those of A; but when placed at $B^1$, $B^3$ or $B^5$, etc., its phases of vibration will be opposed to those of A. That is, at distances from A, equal to any number of whole wave-lengths the body B will, at the same moment of time, swing with A, but at distances from A equal to any number of half wave-lengths the direction of its swings, at any given instant, will be opposed to A; while at intermediate positions, on the line OX, the oscillations of B will be lagging somewhat behind or be slightly in advance of the phase of A's vibration.



After this it is evident that if, by any means, we can *see at the same time* the vibrations of A and of B, we will (if the received conception of the nature of a vibration's propagation is correct) see their motions just as has been described above, and will therefore be able to measure, *directly in the air*, a wave-length and to determine any wave surface enclosing a vibrating body. I have devised several processes. I will, however, here describe only two; the first, though impracticable, I speak of to render clear the general method of all; the second I give on account of its simplicity, ease of execution, and the superior accuracy of its numerical results.

Take two tuning forks giving the same note and having mirrors attached to their similar prongs; place one at A, the other anywhere on the line OX. Reflect a pencil of light from each mirror of the forks to a revolving mirror, whose axis of rotation is in a plane parallel to the planes of vibration of the forks. If the fork B, which vibrates sympathetically, be placed at $B^2$, $B^4$, $B^6$, etc, then the two pencils reflected from the forks will, on striking the revolving mirror, be drawn into two sinuous curves, and the flexures of the two curves will run parallel to each other, that is, the curves will appear as the two rails of a sinuous railway; but, if the fork B be placed at $B^1$, $B^3$, or $B^5$, etc., then the sinuosities of the two curves will no longer be parallel but will be opposed; that is, where a flexure of one of the curves is concave on the left, the opposite flexure of the other curve will have its concavity on the right. If the fork B be placed at intermediate positions, in reference to those above stated, we will have neither concordance nor opposition of the flexures, but intermediate relations depending on the fraction of half wave-lengths at which the sympathetic fork is placed on the line OX.

It is now readily seen that if we should place the fork B at two successive points, as $B^2$ and $B^4$, on the line OX, so that exact concordance of flexures of the curves should be seen at each of these points, then evidently we have placed the fork at two positions removed from each other by a wave-length, for at these points the air had at the same instant the same phase of vibration. Thus we have measured a wave-length. Furthermore, if by any means we could move the fork B around A so that during this motion it always preserved, in reference to A, the same relation of vibratory phase, we would have determined the form of the wave-surface produced by the propagation of A's vibrations.

The above is an exposition of the thoughts that have occupied my mind for several months, and they ultimately led to the following method, by which all I have narrated can be accomplished without any difficulty; thanks to the inventive genius of Mr. König, to whose skillful aid so many physicists are continually indebted.

The membranes of Mr. König's manometric capsules furnish us with surfaces which vibrate in perfect accordance with the air which touches them, and we can lead the impulses of these membranes through gum tubes to gas jets placed at any desired point, where the vibrations of their flames can be compared. Thus they are far superior to the tuning forks, which require the relations of delicate adjustments to be maintained during each change of position, and therefore forks could only with difficulty be made to serve in the measure of a wave-length, and could not at all be employed to trace out a wave-surface on account of the impossibility of a continuous comparison of their vibrations, which latter condition the manometric flames admirably fulfill].

### *The Experiments.*

Let us now proceed to experiment. I placed on the acoustic bellows an open $UT_3$ organ pipe, and from its ventral manometric capsule I led a tube to a gas jet placed in front of a cubical revolving mirror. I took an $UT_3$ Helmholtz resonator and adapted to its beak a gum tube, with an interior diameter of 1 centimeter and a length of over 4 meters. This tube led to a firmly supported manometric capsule whose flame was placed quite close to and directly behind, the organ pipe flame, which latter had about twice the height of the resonator flame. On sounding the pipe and holding the resonator quite near it, the two flames, by a slight adjustment, were made to appear as *one series of serrations* in the revolving mirror. Now on gradually moving the resonator away from the pipe, I saw another series of serrations (those of the resonator flame) slowly evolve themselves from the first series, and gradually slide over the latter, until, having removed the resonator from its first position by about 66 centimeters or a half wave-length (German), I had the pleasure of seeing the series of moving serrations standing exactly midway between the first or immovable series. On moving the resonator yet farther from the sounding pipe, I saw the serrations of the resonator flame continue their onward progress until the two series again coincided; and on measuring the distance of the resonator from its first position near the pipe, I found it to be equal to a whole wave-length of the note $UT_3$. When I had removed the resonator one and a half wave-length, I again saw the serrations of the resonator flame bisecting the spaces between the serrations given by the organ pipe flame, and when the resonator had progressed from the pipe to a distance equal to two whole wave-lengths I saw that the serrations of its flame had progressed to another coincidence with those of the organ pipe; and so on, until I had determined on the line of the resonator's motion *all the phases of vibration* corresponding to three whole wave-lengths. I now moved the resonator until I had again caused the serrations of its flame neatly to bisect the spaces between the serrations of the organ pipe flame, and moving around the organ pipe, with the resonator held at such distances from it that the bisections were steadily kept, I described in space the wave surface of the sounding pipe. This surface I found approximately to be an ellipsoid with its foci at the top and bottom of the pipe. Nothing could be more satisfactory, and it was charming to behold how neatly the surface could be determined; for a small change in the distance of the resonator from the pipe produced a sensible shifting of the serrations. I now substituted for the resonator an organ pipe, in every respect similar to the one on the bellows, and with it I repeated the wave-length measures previously made with the resonator; indeed the column of air in the pipe in my hand responded so perfectly to the sounding pipe that I thought it gave more marked results than those produced with the resonator.

### *The manometric flame-micrometer.*

In the experiments described above, we examined the appearances in the mirror with the unaided eye, and with it estimated when coincidences and bisections occurred; but to obtain results of precision, a method was devised which determines neatly these critical points. For that purpose I have invented the following micrometer, founded on a beautiful suggestion of Dr. R. Radau, who thus describes in his excellent "*l'Acoustique*" (Paris, 1867, p. 272), a method of observing the flames of two similar sounding organ pipes. "We attach to the two pipes two of König's flames arranged so that the point of one flame reaches above a small fixed mirror which hides its base, but which shows by reflection the base of the other flame. This produces the illusion of a single flame. If now we observe this hybrid image in the revolving mirror while we sound the two pipes, the point separates from the base, which proves that the two flames shine *alternately*, and the one retracts while the other elongates; if the two tubes act on the same flame, the effect is null, and the flame remains immovable." By placing the above "small fixed mirror" on a divided circle; or by silvering its back and determining its angular displacements around a vertical axis by the method of Poggendorff,—that is, by observing through a telescope, the reflections of a fixed scale from the back of the mirror,—we have devised a simple and precise micrometer for ascertaining the amount of displacement of the resonator's flame. For, having once determined, for a given note, the amount of angular motion of the mirror required to move the bases of the flames over the distance between the centers of two contiguous serrations we have the angular value of a displacement equal to that caused by moving the resonator through a wave-length, and a fraction of the turn required to produce the above movement of the bases of the flames will be equal to that produced by the remove of the resonator over a corresponding fraction of a wave-length. Thus can be measured very small fractions of a wave-length. Indeed, even with the unaided eye and without the use of the micrometric mirror I have distinctly detected a displacement of the flames on moving the resonator, ($UT_3$) over only 3 centimeters or $\frac{1}{44}$th of a wave-length, and with the micrometer I feel assured that I can determine the wave-surface of a body giving the note $UT_3$ to one centimeter of its true position. Of course with higher notes we shall get a proportionally closer determination. But the object of this paper is not to present numerical results; I reserve these for a subsequent communication, in which I will also present diagrams of apparatus and the appearances of the flames in various experiments.

I will here remark that the success of the experiments depends on the resonator with its attached tube being in perfect unison with the organ pipe; also, the relative heights and positions of the flames should be so adjusted that the sharpest definitions are obtained in the rotating mirror, and thus be able to detect and measure the effects of small changes in the position of the resonator; but these and other manipulative details will readily occur to any physicist who repeats the experiments.

### *Applications of the Method.*

When the method I have here blocked out shall have been reduced to the refinement which it is susceptible of, I feel confident that we will have in our hands the power to attack many problems of high theoretic interest which have heretofore been deemed beyond the reach of experiment. Its application to such are so numerous that they are almost co-extensive with the phenomena of sound.

The actual experimental determination of wave-surfaces in free air and in buildings can now certainly be accomplished; and such determinations may serve to extend our knowledge in the direction of giving the proper laws which should govern architects in their construction of rooms for public assemblies.

The differences, if any, in the velocities of sound, corresponding to vibrations differing in intensities and frequencies, may be determined by the use of reflectors, and the direct observation on any changes in wave-lengths different from those which should be given on the assumption that notes of various intensities and heights are propagated with the same velocity.

We can determine a wave-length quite accurately by the following arrangement of apparatus. Take an organ pipe with a resonator in unison with it, and place the resonator in a fixed position opposite the mouth of the pipe, then lead tubes from the capsules of pipe and resonator to contiguous jets, and adjust their flames to coincidence or to bisection of serrations; using for this purpose the manometric micrometer. Now suppose, for simplicity, that the pipe gives 340 complete vibrations in a second; then, as the velocity of sound is 340 meters per second, it is evident that in $\frac{1}{340}$th of a second an aerial pulse will traverse one meter. Therefore, if all things else remain the same, and we lengthen the resonator tube ½ meter, the serrated flames of the resonator will be displaced ½ of the distance between the centers of two contiguous serrations; and if the tube be lengthened 1 meter, or *one wave-length*, the displacement will amount to the entire distance separating the centers of two contiguous serrations; and for *n* number of wave-lengths of elongation of tube, we shall have *n* number of such displacements. Thus can be measured a wave-length; and if the number of vibrations given by the pipe be accurately known, we can reach with the manometric micrometer an accurate determination of the velocity of sound.

Finally, we are bold enough to believe that we have in the highest development of the method, a means of tracking in the air the resultant wave-surface of combined notes; and, in short, of bringing the exploration of acoustic space to approach somewhat to that precision of measurement which, for over half a century, has characterized the study of the æthereal vibrations producing light.

September 21st, 1872.

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## Art. XLVI.—*Growth or Evolution of Structure in Seedlings;* by John C. Draper, M.D.

The continuous absorption of oxygen, and formation of carbonic acid, is an essential condition of evolution of structure, both in plants and in animals.

The above proposition in so far as it relates to animals will probably be admitted by all; the opposite opinion is, however, commonly held as regards plants; yet we propose to show that in these organisms, as in animals, growth as applied to evolution of structure, or organization of material provided is inseparably connected with oxidation.

The discussion of the proposition in question necessarily involves a preliminary review of the character of the gases exhaled from various plants. Commencing with the lower organisms as fungi, the uniform testimony is that these plants at all times expire carbonic acid, while it is chiefly in the higher plants, and especially in those which contain chlorophyl or green coloring matter that carbonic acid is absorbed and oxygen exhaled. The inquiry then in reality narrows itself down to the examination of the growth of chlorophyl-bearing plants.

Regarding these plants the statement is made and received, that they change their action according as they are examined in the light or in the dark, exhaling oxygen under the first condition, and carbonic acid under the second. Various explanations of this change of action have been given, that generally accepted accounting for it on the hypothesis of the absorption of carbonic acid by the roots, and its exhalation by the leaves when light is no longer present.

The change, on the contrary, appears to arise out of the fact that two essentially different operations have been confounded, viz: the actual growth or evolution of structures in the plant, and the decomposition of carbonic acid by the leaves under the influence of the light, to provide the gum or other materials that are to be organized. These two factors are separated by Prof. J. W. Draper in his disscusion of the conditions of growth in plants. We propose to show that by adopting this proposition of two distinct operations in the higher plants, all the apparent discrepancies regarding the growth of these plants are explained.

The growth of seedlings in the dark offering conditions in which the act of growth or evolution of structure is accomplished without the collateral decomposition of carbonic acid, I arranged two series of experiments in which growth under this condition might be studied and compared with a similar growth in the light. That the experiments might continue over a sufficient period of time to furnish reliable comparative results, I selected peas as the subject of trial, since these seeds contain sufficient material to support the growth of seedlings for a couple of weeks.

To secure as far as possible uniformity of conditions between the dark and light series, and also to facilitate the separation, cleansing and weighing of the roots, each pea was planted in a glass cylinder, one inch in diameter and six inches long. These cylinders were loosely closed below by a cork, and filled to within half an inch of the top with fine earth or vegetable mould. They were then placed erect in a covered tin box or tube-stand, in such a manner that the lower end dipped into water contained in the box, while the whole of the cylinder except the top was kept in the dark. Thus the first condition for germination, viz., darkness, was secured; the second, warmth, was supplied by the external temperature, which varied from 70° to 80° F., while regularity and uniformity in the supply of moisture in both series was secured by having a box of cylinders or tubes for each and keeping the level of the water the same in both. The supply of oxygen was also equal and uniform, since the upper part of each tube presented a similar opening to the air.

Thus prepared, one box containing five cylinders was kept in a dark closet, while a second similar in all respects was placed in a window of the adjoining room, where it was exposed to direct sunlight five or six hours every day. To each tube a light wooden rod thirty inches in length was attached, and on this the growth of the seedling was marked every twelve hours.

The hours selected were 7 A. M. and 7 P. M. I thus obtained the night and day, or dark and light growth of every seedling, as long as those in the dark grew. The seeds were planted on June 1st, and appeared above the ground on June 6th, when the measurements were commenced. In each series one seed failed to germinate; the record consequently is for four plants in each, and the history of the evolution of structures is as follows:

*Evolution of structure in the dark.*—In Table I. the seeds are designated as A, B, C, D, and each column shows the date on which leaves and lateral growths appeared. These constitute periods in the development of the plants, which are indicated by the numbers 1, 2, 3, 4, 5, 6. The weight of each seed is given in milligrammes.

Table I.—*Seedlings grown in the dark.*

| Weight of seed. | A.<br>431. | B.<br>436. | C.<br>456. | D.<br>500. |
|---|---|---|---|---|
| Period 1, | 7th day. | 7th day. | 7th day. | 7th day. |
| " 2, | 8th " | 9th " | 9th " | 8th " |
| " 3, | 10th " | 10th " | 11th " | 10th " |
| " 4, | 12th " | 12th " | 13th " | 12th " |
| " 5, | 14th " | 15th " | 15th " | 14th " |
| " 6, | 17th " | 18th " | 18th " | 17th " |

A glance at the above shows the uniformity as regards time with which the structures were evolved in each plant. It also indicates for each plant an equality in the number of periods of evolution, viz., 6, notwithstanding the difference in the weights of the seeds; and suggests that the power of evolution of structure in seedlings resides in the germ alone.

The character of the evolution in the six periods shows a steady improvement or progression.

In the first, the growth consists of the formation close to the stem of two partially developed pale yellow leaves.

The second period is similar to the first, except that the leaves are a little larger.

The third presents a pair of small yellow leaves close to the main stem, from between which a lateral stem or twig about one inch long projects, and bears at its extremity a second pair of imperfectly developed yellow leaves, from between which a small tendril about a sixteenth of an inch long is given off.

The fourth resembles the third, the lateral twig being longer, and the tendril three times as long as in the third.

The fifth is like the fourth, except that the tendril bifurcates.

The sixth is similar to the fifth, except that the tendril trifurcates.

Stem, leaves, twigs, tendrils of various degrees of complexity, all are evolved by the force pre-existing in the germ without the assistance of light.

### *Evolution of structure in the light.*

Table II.—*Seedlings grown in the light.*

| Weight of seed. | E. 288. | F. 426. | G. 462. | H. 544. |
|---|---|---|---|---|
| Period 1, | — | 6th day. | — | 6th day. |
| " 2, | 7th day. | 7th " | 7th day. | 7th " |
| " 3, | 8th " | 8th " | 8th " | 9th " |
| " 4, | 12th " | 9th " | 10th " | 10th " |
| " 5, | 15th " | 11th " | 14th " | 12th " |
| " 6, | — " | 13th " | — " | 14th " |

Table II. was obtained in the same manner as Table I, the columns representing the days on which lateral growths and leaves appeared. Though there is not the same uniformity as in Table I, the periods are identical in both as regards the visible character of the evolution. Nothing appears in the second that did not pre-exist in the first, and in the case of the seeds E and G the evolution is even deficient as regards the first and the sixth periods.

While the general character of the evolution in both series is similar, certain minor differences exist. In II. the leaves and tendrils are many times larger than in I, and they with the whole plant are of a bright green color, instead of the sickly pale yellow of I; but the light has not developed any new structure; it has only perfected those which pre-existed, and converted other substances into chlorophyl which is not an organized body.

Not only did the plants in the two series present similarities in evolution of structure, but the average weight of dry plant in each was very nearly the same, for:

455 of seeds in the dark produced 184 of dry plant, while
455 " light " 215 " "

A comparison of the parts below the ground with those above (both being dried at 212° F.) shows that the proportion of root to total weight of plant was also nearly identical; being,

25 of root for 100 of plant in the dark, and
23 " 100 " light.

The close similarity in the evolution of visible structure in the light and in the dark, the small difference in the total weights of the plants grown in the same time in both series, and the close approximation in the proportional weight of root to plant, all justify the conclusion, that the growth in darkness and in light closely resemble each other, and that it is proper to reason as regards the nature of the action from the first to the second.

Another interesting fact which lends support to the opinion that the process of growth in seedlings developed in the dark is very similar to that occurring in those grown in the light, is the character of the excrements thrown out by the roots. It is well known that many plants so poison the soil that the same plants cannot be made to grow therein until the poisonous excretions from the roots of the first crop have been destroyed by oxidation. In the case of peas this poisoning of the soil takes place in a very marked manner, and I have found that in the pots in which peas have been grown in the dark, the soil is so poisoned by the excrements from the roots that a second crop fails to sprout. Does it not follow, that since in the two series with which I experimented, the excrements from the roots possessed the same poisoning action, the processes in the plants from which these excrements arose must have been similar?

There remains an important argument concerning which nothing has thus far been said. It is to be derived from the consideration of the rate of growth in the light series during various periods of the day of twenty-four hours. If the evolution of structure in a plant in daylight is the result of the action of light, that evolution should occur entirely, or almost entirely during the day. If on the contrary it is independent of the light, it should go on at a uniform rate as in plants in the dark.

For the elucidation of this portion of the subject, I present the following tables; the first of which shows the growth by night, 7 P. M. to 7 A. M. of the seedlings in the dark series, compared with their growth by day, 7 A. M. to 7 P. M. The measurements were taken from the sixth to the twentieth of the month, the day on which growth ceased in the dark series.

Table III.—*Seedlings grown in the dark.*

| | Night growth. | | Day growth. |
|---|---|---|---|
| No. 1 | $12\frac{3}{4}$ inches. | | 14 inches. |
| No. 2 | $13\frac{1}{4}$ " | | 13 " |
| No. 3 | $11\frac{3}{4}$ " | | $11\frac{3}{8}$ " |
| No. 4 | $12\frac{5}{8}$ " | | $11\frac{3}{8}$ " |
| Average, | $12\frac{5}{8}$ " | Average, | $12\frac{3}{8}$ " |

The total day growth and night growth under these circumstances are nearly equal, though there is a slight excess in favor of the night, amounting as the table shows, to $\frac{2}{8}$ of an inch in 12 inches.

In Table IV. the growth of the light series is given in the same manner, by day and by night, for the same time, viz: to June 20th. The thermometric and hygrometric conditions in both series were very similar, as indicated by the dry and wet bulb thermometers suspended in the vicinity of each set of tubes.

Table IV.—*Seedlings grown in the light.*

| | Night growth. | Day growth. |
|---|---|---|
| No. 5 | 3¼ inches. | 4 inches. |
| No. 6 | 8 " | 7 " |
| No. 7 | 5¼ " | 4½ " |
| No. 8 | 9½ " | 8½ " |
| | Average, 6½ " | Average, 6 " |

In the average, and throughout the table, with a single exception, not only is the uniformity in the rate of growth during the day and night shown, but the slight excess of night growth found in the series kept in the dark is likewise copied. We must therefore accept the conclusion, that the act of growth or evolution of structure is independent of light, and that the manner of growth during the day is similar to that at night.

It will be noticed that the total average height attained in the light is only about half that in the dark series. The explanation of this we have already seen in the fact, that in the former the leaves and tendrils were much larger than in the latter, while the dry weights were nearly the same. The material of the seed in the light series was consumed in extending these surfaces, while in the dark series it was spent in lengthening the stem.

Having established the continuous character of growth in seedlings, and the similarity of rate and nature of the process by night and by day, and admitting that at night plants throw off carbonic acid, it is not improbable that this carbonic acid arises, not from mechanical absorption by the roots, and vaporization by the leaves, but as a direct result or concomitant of the act or process of evolution of structure.

To put the matter in the clearest form, let us first understand what growth is. It appears in all cases to consist in the evolution or production of cells from those already existing. According as the circumstances under which the cells are produced vary, so does the tissue ultimately produced vary. Cells formed in woody fibre become wood. Cells formed in muscle in their turn form muscles, but the starting point of the process in every instance is the formation of new cells.

If now we examine the evolution of cells under the simplest conditions, as for example in the fermentation that attends the manufacture of alcohol, we find that with the evolution of the torulæ cells carbonic acid is produced. The two results are intimately connected, and it is proper to suppose that since the carbonic acid has arisen along with the new cells, the latter operation must in some way involve a process of oxidation. Accepting the hypothesis that oxidation is attendant on these processes of cell growth under the simplest conditions, we pass to the examination of what occurs in the lower forms of vegetable organisms found in the air.

The Fungi, and indeed all plants that are not green, with a few exceptions, exhale carbonic acid and never exhale oxygen. In this case, in which cell production often occurs with such marvelous rapidity, the carbonic acid must have arisen as a consequent of the cell growth. It is improbable that it has been absorbed by roots and exhaled from the structures, either in these plants or in those produced during fermentation. In the latter there never are any roots, and in the former, even where roots are present, they bear a small proportion to the whole plant. The quantity of moisture exhaled by such growths is also insignificant, and out of proportion to the carbonic acid evolved. We must, therefore, in this case decline to accept the root absorption hypothesis, and admit that the carbonic acid has arisen as a result of the cell growth in the plant.

Passing to the chlorophyl-bearing plants, we find that in the Phanerogamia it is only the green parts that at any time exhale oxygen, and then only under the influence of sunshine. The other parts of the plant above the ground, that are not green, viz., the stem, twigs, flowers, etc., are at all times, day and night, exhaling carbonic acid. The whole history of the plant, from the time the seed is planted to its death, is a continuous story of oxidation, *except when sunlight is falling on the leaves.* The seed is put into the ground and during germination oxygen is absorbed and carbonic acid exhaled. If the seedling is kept in the dark, oxygen is never exhaled, only carbonic acid, and the plant not only grows, but all visible structures except flowers are formed in a rudimentary condition. In the light the growth during the night time is attended by the evolution of carbonic acid, while during the day time the bark of the stem and branches is throwing off carbonic acid. When flowers and seeds form, the evolution of carbonic acid attending this highest act of which the plant is capable, is often greater than that produced at any time in many animals.

Everything in the history of plants, therefore, tends to show that the evolution of their structures is inseparably attended by the formation of carbonic acid, and it seems impossible, when we consider the evolution alone, to arrive at any other opinion than that already expressed—that, *all living things, whether plant or animal, absorb oxygen and evolve carbonic acid, or some other oxidized substance, as an essential condition of the evolution of their structures.*

**College of the City of New York, Sept. 12th, 1872.**

## ART. XLVII.—*Rejoinder to Prof. Hall's Reply to a "Note on a question of Priority"*; by E. BILLINGS.

To answer the whole of Professor Hall's reply, published in the August number of this Journal, would require a long article. I beg, however, to notice briefly several of the leading points.

With regard to publication, I hold it to be the duty of an author who describes new fossils to make his work accessible to the public. If he fail to do this, he cannot claim priority over one who has published in the regular way. His work may be adopted as a matter of courtesy, but not to take precedence over fair publication. Prof. Hall's pamphlet was not accessible to the public at the time my paper was published, and therefore his genus *Rhynobolus* cannot take priority over my genus *Obolellina*. During the discussion that has taken place it has been argued, with reference to publication, that "no determined rules or laws have been hitherto settled or followed." On this point, I hold that there are laws which result spontaneously from the very nature of the circumstances to which they relate. These laws exist perpetually, although not instituted by legislative enactment, and although they may be habitually transgressed by any number of unscrupulous persons. The law of publication is one of these. Every true naturalist instinctively feels and knows that such a law does exist, and that it is his duty to observe it. My genus was fairly published, in accordance with the requirements of this law, and therefore it must stand, unless, by some miracle, the law should cease to exist.

The genus *Obolellina* belongs to the *Trimerella* group of fossils. Prof. Hall borrowed some of our specimens of that group, and notice was given him that I was at work upon them. The object of giving this notice was to prevent any occurrence which might lead to ill feeling. Instead of doing his best to carry out this object, he almost immediately proposed his genus *Rhynobolus*, on a Canadian specimen from the same locality where those lent him had been collected. I believe he is the only paleontologist in America who would have taken such a course. He attempts to justify himself by saying that I had procured specimens from New York. But the case is wholly different. I require New York fossils for comparison, and for that purpose have bought them, collected them myself, and sent others to collect them for me. But I only use them for comparison. I never described a new species collected in New York. On the other hand, Prof. Hall collects Canadian fossils, and goes further. He describes the new species. He even visited a party who, as he well knew, was collecting for our survey, and procured a collection from him. In his reply, he gives the reader to understand that he has refrained from describing Canadian species "*from a natural sense of propriety.*" Where was this sense of propriety when he described the fossils from Cayuga, Canada West, in vol. iv, Pal. N.Y.; for instance, *Cryptonella iphis*, *Centronella ovata*, and *Meristella lenta?*

He intimates that "in nearly all cases" where he has proposed new genera, during the last "ten or fifteen years," I have raised an objection of some kind. Since reading his paper I have looked over his works, and have made a list of ninety-seven genera proposed by him during the period mentioned. Of these, I have objected to not more than a dozen. His statement is therefore a gross exaggeration. I have not made that sweeping condemnation of his work that he would desire to be imputed to me.

There is, besides the above, nothing in his reply but matter totally irrelative to the subject in dispute.

## ART. XLVIII.—*Elements of Planets* (122) *and* (123); by Prof. C. H. F. PETERS. From a letter to one of the editors, dated Litchfield Observatory of Hamilton College, Clinton, Oneida Co., N. Y., October 15, 1872.

THE planets (122) and (123), of which the former has received the name of *Gerda* (from Scandinavian mythology), the latter that of *Brunhilda* (well known from the Nibelungen), have now gone out of sight, or become so faint that further observations would be of little value. I have, consequently, computed their orbits, for each selecting from the series of observations three positions suitably distributed. The elements, which, on account of the length of the area employed, may be assumed to possess already a great degree of reliability, result as follows:

(122) *Gerda*, from obs. Aug. 1, Aug. 28, and Sept. 24.

Epoch: 1872, August 28·0, Berlin mean time.

$$\begin{array}{lll}
M_0 = 112^\circ\ 59'\ 34''\cdot63 & & \varphi = 2^\circ\ 5'\ 23''\cdot74 \\
\left.\begin{array}{r} \pi = 208\ \ 12\ \ \ 7\cdot76 \\ \Omega = 178\ \ 56\ \ 41\cdot89 \\ i = 1\ \ 36\ \ 16\cdot85 \end{array}\right\} & \text{Mean eq. } 1872\cdot0 & \begin{array}{l} \mu = 613''\cdot5218 \\ \log \alpha = 0\cdot5081178 \\ \ \end{array}
\end{array}$$

(123) *Brunhilda*, from obs. Aug. 1, Aug. 27, and Sept. 25.

Epoch: 1872, August 27·0, Berlin mean time.

$$\begin{array}{lll}
M_0 = 267^\circ\ 54'\ 28''\cdot70 & & \varphi = 6^\circ\ 30'\ 23''\cdot32 \\
\left.\begin{array}{r} \pi = 71\ \ 51\ \ 32\cdot93 \\ \Omega = 308\ \ 42\ \ 13\cdot02 \\ i = 6\ \ 28\ \ 32\cdot48 \end{array}\right\} & \text{Mean eq. } 1872\cdot0 & \begin{array}{l} \mu = 803''\cdot1187 \\ \log \alpha = 0\cdot4301512 \\ \ \end{array}
\end{array}$$

The orbit of *Gerda* is remarkable for having both the inclination and eccentricity very small, a coincidence which is not found among the other known asteroids except in the orbit of *Clytia.*

Upon re-computing, for a check, the observations from the elements, the following insignificant differences remained (calc.—obs.):

|  | (122) |  |  | (123) |  |
|---|---|---|---|---|---|
|  | $\Delta\alpha$ | $\Delta\delta$ |  | $\Delta\alpha$ | $\Delta\delta$ |
| Aug. 1 | $-0^{\mathrm{s}}\cdot01$ | $-0''\cdot1$ | Aug. 1 | $-0^{\mathrm{s}}\cdot01$ | $-0''\cdot1$ |
| Aug. 28 | $-0\cdot02$ | $0\cdot0$ | Aug. 27 | $-0\cdot04$ | $-0\cdot2$ |
| Sept. 24 | $-0\cdot04$ | $-0\cdot1$ | Sept. 22 | $-0\cdot03$ | $0\cdot0$ |

The planet (124) (named *Abeste*) a few days ago reached its stationary point, so that now its right ascension is increasing. Its brightness, now about 11·2 magnitude, will permit observations for some time yet.

## SCIENTIFIC INTELLIGENCE.

### I. CHEMISTRY AND PHYSICS.

1. *On the Chemical Efficiency of Sunlight;* by JAMES DEWAR, Esq. (Phil. Mag., Oct. 1872.)—Of all the processes proposed to measure varying luminous intensity by means of chemical effects, not one has yet been expressed in strictly dynamical measure. This is owing to the very small amount of energy to be measured necessitating very peculiar processes for its recognition. The chemical actions generally induced by light are of the "Trigger" or "Relay" description—that is, bear no necessary relation to the power envolved by the transformation. There is one natural action of light, however, of a very different kind, continuously at work in the decomposition of carbonic acid by plants, necessitating a large absorption of energy, and thus enabling us to ascertain the proportion of the radiant power retained, through the chemical syntheses affected.

So far as I am aware, the following passage, extracted from Helmholtz's Lectures "On the Conservation of Energy," delivered at the Royal Institution in 1864, and published in the Medical Times and Gazette, contains the first estimate of the chemical efficiency of sunlight. "Now, we have seen already, that by the life of plants great stores of energy are collected in the form of combustible matter, and that they are collected under the influence of solar light. I have shown you in the last lecture that some parts of solar light—the so-called chemical rays, the blue and the violet which produce chemical action—are completely absorbed and taken away by the green leaves of plants; and we must suppose that these chemical rays afford that amount of energy which is necessary to decompose again the carbonic acid and water in their elements, to separate the oxygen, to give it back to the atmosphere, and to collect the carbon and hydrogen of the water and carbonic acid in the body of the plant itself. It is not yet possible to show that there exists an accurate equivalent proportion between the power or energy of the solar rays which are absorbed by the green leaves of plants, and the energy which is stored up in the form of chemical force in the interior of the plants. We are not yet able to make so accurate a measurement of both these stores of energy as to be able to show that there is an equivalent proportion. We can only show that the amount of energy which the rays of the sun bring to the work is completely sufficient to produce such an effect as this chemical effect going on in the plant. I will give you some figures in reference to this. It is found in a piece of cultivated land producing corn or trees; one may reckon per year and per square foot of land 0·036 lb. of carbon to be produced by vegetation. This is the amount of carbon which during one year, on the surface of a square foot in our latitude, can be produced under the influence of solar rays. This quantity, when used as fuel and burnt to produce carbonic acid, gives so much heat that 291 lbs. of water could be heated 1° C. Now we know the whole quantity of solar light which comes down to one square foot of terrestrial surface during one second, or one minute, or one year. The whole amount which comes down during a year to one square foot is sufficient to raise the temperature of 430,000 lbs. of water 1° C. The amount of heat which can be produced by fuel growing upon one square foot during one year is, as you see from these figures, a very small fraction of the whole amount of solar heat which can be produced by the solar rays. It is only the 1477th part of the whole energy of solar light. It is impossible to determine the quantity of solar heat so accurately that we could detect the loss of so small a fraction as is absorbed by plants and converted into other forms of energy. Therefore, at present, we can only show that the amount of solar heat is sufficient to produce the effects of vegetable life, but we cannot yet prove that this is a complete equivalent ratio." This estimate is, strictly speaking, the mean agricultural efficiency of a given area of land, cultivated as forest; and considering that active growth only takes place during five months in the year, we may safely adopt $\frac{1}{600}$ of the total energy of sunlight as a fair value of the conserved power, on a given area of the earth's surface in this latitude during the course of the summer. As chlorophyl in one or other of its forms is the substance through which light becomes absorbed and chemical decomposition ensues, it would be interesting to acquire some idea of the storage of power effected by a given erea of leaf-surface during the course of a day, and to compare this with the total available energy. Here we are dealing with strictly measureable quantities, provided we could determine the equation of chemical transformation.

Boussingault's recent observations on the amount of carbonic acid decomposed by a given area of green leaf seem to me to afford interesting data for a new determination of the efficiency of sunlight. By experiments made between the months of January and October under the most favorable circumstances in atmospheres rich in $CO^2$, one square decimetre of leaf was found to decompose in one hour, as a mean, 5·28 cub. centims. of $CO^2$, and in darkness to evolve during the same period of time 0·33 cub. centim. of $CO^2$. In other words, one square metre of green surface will decompose in twelve hours of the day 63·36 cub. centims. of $CO^2$, and produce in twelve hours of the night 3·96 cub. centims. of $CO^2$.*

The quantity of carbonic acid decomposed does not represent the whole work of sunlight for the time, as water is simultaneously attacked in order to supply the hydrogen of the carbohydrates. Boussingault, in summing up the general results of his laborious researches on vegetable physiology, says, "Si l'on envisage la vie végétale dans son ensemble, on est convaincu que la feuille est la première étape des glucoses que plus ou moins modifiés, on trouve répartis dans les diverses parties de l'organisme; que c'est la feuille qui les élabore aux dépens de l'acide carbonique et de l'eau."—*Ann. de Chemie*, tom. xiii, p 415. The fundamental chemical reaction taking place in the leaf may therefore be represented as follows:

$$(1)\quad CO,O \;+\; H^2O \;=\; CO,H^2 \;+\; O,O$$

$$(2)\quad 6(CO.H^2) \;=\; C^6H^{12}O^6$$

In the first equation carbonic acid and water are simultaneously attacked, with the liberation of a volume of oxygen equal to that of the original carbonic acid, together with the formation of a substance having the composition of methylic aldehyde. The second equation represents the condensation of this aldehyde into grape sugar. The transformation induced in (1) necessitates the absorption of a large amount of energy; and if we neglect the heat envolved in the combination of nascent CO and $H^2$, which can be shown to be very little, the calculated result is made a maximum; whereas the condensation of (2) being attended with an evolution of heat, diminishes considerably the amount of power required. Happily Frankland's direct determination of the thermal value of grape-sugar leaves no doubt as to the true equivalent of work done in its formation. Taking the following

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* The rate at which the leaf functions is dependent on the luminous intensity. The relative amounts, therefore, of carbonic acid decomposed through the action of the different colored rays are proportional to their luminous power; and the curve of assimilation is found to follow the curve of Fraunhofer. This proves that the judgment we form of equal luminous impressions is in reality due to equal mechanical effects associated with the different colored rays. Professor Draper, of New York, in his recent paper "On the Distribution of Heat in the Spectrum," by dividing the spectrum into two portions of equal luminous intensity, obtained identical thermal effects by absorption. This does not prove that each ray has the same total energy, but only that in all probability those at equal distances on either side of the mean wave-length in the normal light-spectrum of the sun are identical.

thermal values $CO, O = 68{,}000$, $H^2, O = 68{,}000$, $C^6H^{12}O^6 = 642{,}000$, 1 cub. centim. of $CO^2$ decomposed as in (1) would require 6·06 gramme-units of heat, or its light-equivalent, whereas the complete change into grape sugar of the same amount of carbonic acid requires only 4·78 gramme units. But, we have seen before, 1 square decimetre of green leaf functions at the rate of 5·28 cub. centims. of carbonic acid assimilated per hour; therefore $5{\cdot}28 + 4{\cdot}78 = 25{\cdot}23$ represents the number of gramme-heat-units conserved through the absorption of light in the above period of time. Pouillet estimates the mean total solar radiation per square decimetre exposed normally to the sun's rays in or near Paris, per hour, as 6,000 gramme-units, so that $6{,}000 \div 25{\cdot}23 = \frac{1}{238}$ represents the fraction of the entire energy conserved. The estimate is by no means too great, as Boussingault has shown the leaf may function at twice the above rate for a limited time; and as both sides of the leaf are included in the measurement of the green surface in his memoir, we ought to double the fraction for a leaf exposed perpendicularly to the sun's rays, increasing the above number to the 120th part.

In connection with equation (1), above given, as representing the action of sunlight on the leaf, it is worthy of remark that, supposing the carbonic acid and water equally efficient as absorbing agents of the vibratory energy (although each has a specific absorption for certain qualities of rays), the decomposition of the two compound molecules may take place continuously side by side, owing to the equality of the thermal equivalents of carbonic oxide and hydrogen. We already know, from the laborious researches of Tyndall, how thoroughly aqueous vapor retains thermal radiations; and Janssen has further shown the same substance has a strong absorptive action on the rays of light of low refrangibility (just those rays that are in part selected by chlorophyl), producing the well known atmospheric lines of the solar spectrum. The presence, therefore, of varying quantities of aqueous vapor in the atmosphere in all probability produces a considerable difference of rate in the decomposition effected by the leaf, and may in fact end in carbonic acid and water being attacked in another ratio than that given as the fundamental equation of decomposition. Thus the same plant in different atmospheric conditions may elaborate different substances.

2. *On the Law of Extraordinary Refraction in Iceland Spar;* by G. G. Stokes, M.A., Sec. R.S. (Proc. Roy. Soc.)—It is now some years since I carried out, in the case of Iceland spar, the method of examination of the law of refraction which I described in my report on Double Refraction, published in the Report of the British Association for the year 1862, p. 272. A prism, approximately right-angled isosceles, was cut in such a direction as to admit of scrutiny, across the two acute angles, in directions of the wave-normal within the crystal comprising respectively inclinations of 90° and 45° to the axis. The directions of the cut faces were referred by reflection to the cleavage planes, and thereby to the axis. The light observed was the bright D of a soda-flame.

The result obtained was, that Huygens' construction gives the true law of double refraction within the limits of errors of observation. The error, if any, could hardly exceed a unit in the *fourth* place of decimals of the index or reciprocal of the wave-velocity, the velocity in air being taken as unity. This result is sufficient *absolutely to disprove* the law resulting from the theory which makes double refraction depend on a difference of inertia in different directions.

I intend to present to the Royal Society a detailed account of the observations; but in the mean time the publication of this preliminary notice of the result obtained may possibly be useful to those engaged in the theory of double refraction.—*Phil. Mag.*, IV, xliv, 316.

3. *On a new Galvanic Pile, of economic construction;* by M. GAIFFE.—The high price of galvanic piles and the difficulty of procuring them being often an obstacle to the applications which might be made of them, I essayed the possibility of devising an apparatus that one could make anywhere without the aid of the professional workman, with substances of little value, widely spread in commerce, and possessing the essential quality of constancy in the effects.

The pair which, after some trials, I have adopted, resembles Callaud's in its form, used some years since on telegraphic lines; but its elements are different. It consists of a vessel into which dip two rods—one of lead, the other of zinc. The leaden one descends to the bottom; the zinc is one-half shorter. The bottom of the vessel is coated with red oxide of lead (minium); and the exciting liquid is water containing 10 per cent of chlorhydrate of ammonia.

The electromotive force of this pile is about one-third of that of a Bunsen's pair; its internal resistance is slight, and varies little; the chloride of zinc formed does not sensibly alter the conductivity of the exciting liquid; its constancy is great; finally the expense is almost nothing when the circuit is open.—*Comptes Rendus de l'Acad. des Sciences*, July 15, 1872, p 120.—*Phil. Mag.*, IV, xliv, 320.

## II. GEOLOGY AND NATURAL HISTORY.

1. *Discovery of Fossil Quadrumana in the Eocene of Wyoming;* by O. C. MARSH.—An examination of more complete specimens of some of the extinct Mammals already described by the writer from the Eocene deposits of the Rocky Mountain region, clearly indicate that among them are several representatives of the lower Quadrumana. Although these remains differ widely from all known forms of that group, their more important characters show that they should be placed with them. The genera *Limnotherium*, *Thinolestes*, and *Telmatolestes*, especially, have the principal parts of the skeleton much as in some of the *Lemurs*, the correspondence in many of the larger bones being very close. The anterior part of the lower jaws is similar to that of the *Marmosets*,

but the angle is more produced downward, and much inflected. The teeth are more numerous than in any known Quadrumana. Some of the species have apparently forty teeth, arranged as follows: Incisors $\frac{2?}{2}$ canines $\frac{1}{1}$, premolars and molars $\frac{7}{7}$. A full description of these interesting remains, the first of the order detected in this country, will be given by the writer at an early day.

Yale College, Oct. 7th, 1872.

2. *Note on a new genus of Carnivores from the Tertiary of Wyoming;* by O. C. MARSH.—Additional remains of the large Carnivore described by the writer, on page 203, as *Limnofelis latidens*, show clearly that it represents a genus quite distinct from *L. ferox*. The canines and premolars of the lower jaw somewhat resemble those in the Hyæna, but there were only two incisors in each ramus. One of these is large, and close to the canine. Inside and partially behind this, is a cavity for a second and smaller incisor. The remaining teeth preserved are especially broad and massive. The first lower premolar is separated somewhat from the canine, and is inside the line of the teeth behind it. The remains now known indicate an animal about as large as a lion. The genus they represent may be called *Oreocyon*, and the type species, *Oreocyon latidens.*

Yale College, Oct. 6th, 1872.

3. *Notice of a New Reptile from the Cretaceous;* by O. C. MARSH.—An interesting addition to the Reptilian fauna of the Cretaceous shale of Kansas is a very small Saurian, which differs widely from any hitherto discovered. The only remains at present known are two lower jaws, nearly perfect, and with many of the teeth in good preservation. The jaws resemble in general form those of the Mosasauroid reptiles, but, aside from their very diminutive size, present several features which no species of that group has been observed to possess. The teeth are implanted in distinct sockets, and are directed obliquely backward. There were apparently twenty teeth in each jaw, all compressed, and with very acute summits. The rami were united in front only by cartilage. There is no distinct groove on their inner surface, as in all known Mosasauroids. The dentigerous portion of the jaw is 41·mm· in length, its depth below the last tooth is 5·mm· and below the first tooth in front 3·mm· The specimen clearly indicates a new genus, which may be called *Colonosaurus*, and the species may be named *Colonosaurus Mudgei*, for the discoverer, Professor B. F. Mudge, who found the remains in the upper Cretaceous shale of Western Kansas.

Yale College, Oct. 7th, 1872.

4. *Recent Eruption of Mauna Loa;* by Rev. TITUS COAN. (From a letter to J. D. Dana, dated Hilo, Hawaii, Aug. 27, 1872). —On the night of the 10th inst. a grand and lofty pillar of light rose from the summit of the mountain to the height of some 2,000 feet. This was directly over the great terminal crater, Mokuaweoweo. It was most distinctly seen at first from Kilauea and Kau.

On the evening of the 13th we had the first *perfect* view from Hilo. The illuminated cloud of steam and gases which hung over the crater sometimes rose in a well-defined vertical column to a great height, and then the higher portion would expand, forming an inverted cone; again it seemed lighted up above the mountain and spread out like an umbrella over the crater. The changes of form, the expansion, contraction, and convolutions of the illuminated pile, could be distinctly marked, and also the rapid variations in brilliancy dependent on the greater or less intensity of the fiery lavas in the abyss below.

It is now seventeen days since we first saw the eruption, and still the great furnace is in full blast. The action is, evidently, intense. Of all the demonstrations made in this vast cauldron on the summit of the mountain since our residence in Hilo, none have equalled this in magnitude, in vehemence, and in duration. As yet it is confined to the deep crater; and we know not whether the terrific forces now raging in this abyss will rend the walls of the mountain and let out a flow of lavas to the sea, or spend their fury within the recesses of the mountain. The scene from the border of the crater must now be fearfully grand.

I am ashamed to say, that, so far as we know, no one has yet visited the region of eruption. In spite of my age (nearly 72), were it not for the sickness in my family, I should before this have been on the summit of Mokuaweoweo. From the little ranch of Reed and Richardson in Kapapala, Kau, you can ride up on horseback in a day. I hope soon to hear that some one has been to the summit.

Ten thousand feet below the summit fires is Kilauea. This crater has also been very active of late. The south lake, which was so deep when I last wrote you, has long been filled, and it has overflowed many times, sending off broad streams of incandescent lava, filling up the great basin of 1868, elevating the southern portion of Kilauea, raising cones that puff and screech, and throw out vapor, hot gases, and sulphur. The present activity looks like some kind of sympathy with the summit furnace.

Along the shore, 4,000 feet below Kilauea, there was, on the 23d inst., a *tidal wave.* It occurred at 1 P. M. during a calm. The sea in our bay rose silently and rapidly, like an incoming tide, to the height of four feet two inches. In about six minutes it had subsided to a low point and returned again to the height of three feet. Quickly and quietly it retired again; and thus in the space of 1½ hours it made fourteen oscillations, each succeeding one growing fainter, until the sea returned to its normal condition. We had no earthquake at the time.

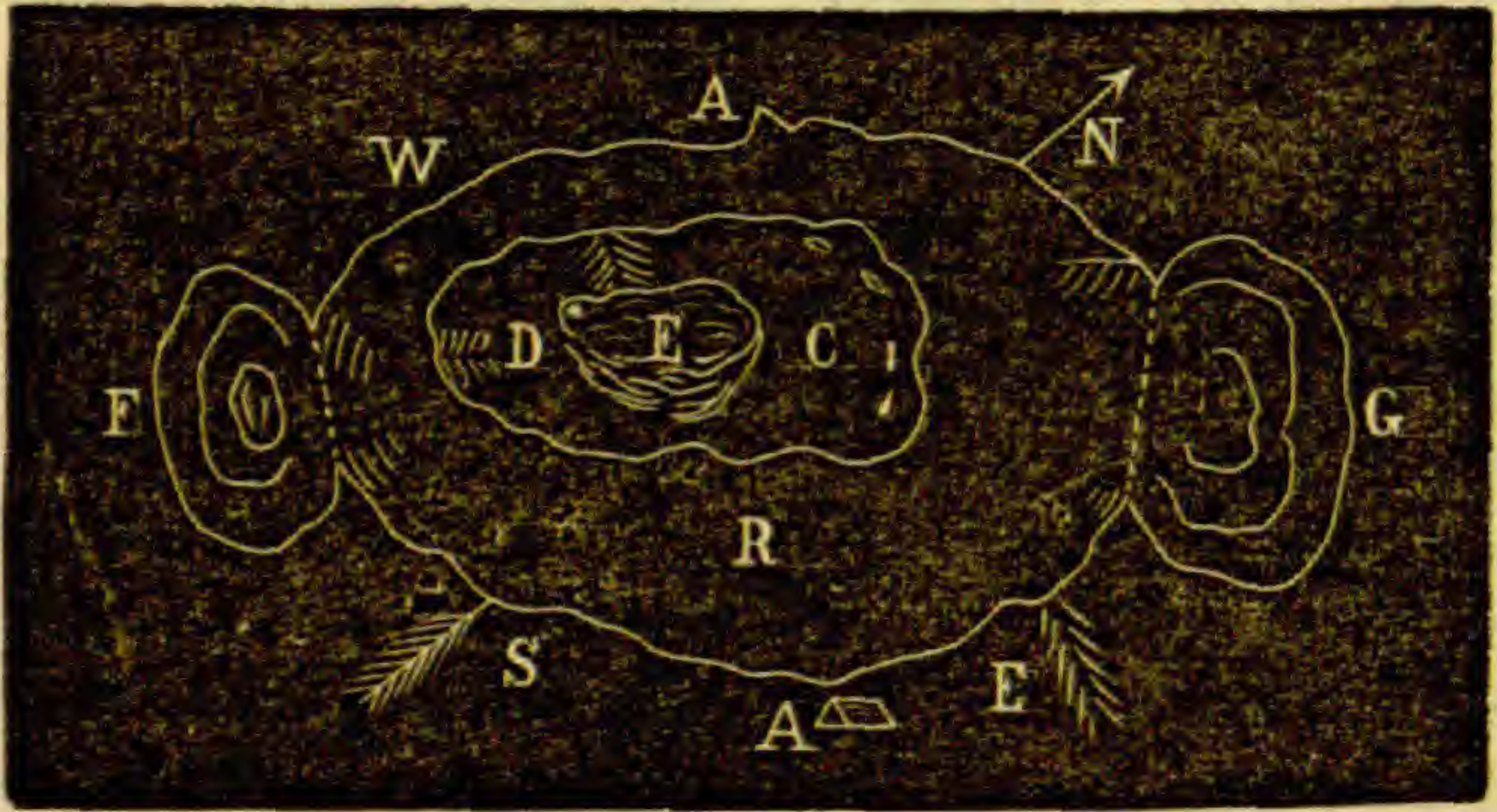
We have had occasional slight earthquakes of late, but no severe ones.

5. *Ascent of Mauna Loa to the scene of Eruption.*—The following is an extract from an account of the ascent of Mauna Loa to the place of eruption, published in the Pacific Commercial Advertiser of Sept. 21.

*The summit.*—Before us lay a rugged plain, about two miles in diameter, of black lava, overlaid in many places with fields of brown *a-a*, and everywhere torn into unheard of shapes by the fierce power that had upheaved the whole. To our right rose a remarkable monument or pillar showing black against the sky. On every hand yawned deep crevices, and spent waves of lava had dashed together in myriad shapes, and so congealed. Hurrying on as well as we were able, we finally reached a *cul-de sac*, formed by a branching *a-a* flow, and here we dismounted, and tethered our animals for the night. This done, we took our way five hundred yards over a narrow strip of rugged lava, and all at once stood upon the edge of the

*Crater of Mokuaweoweo.*—There before us, at our feet as it were, yawned a terrific chasm, with black perpendicular walls carrying the eye down some 800 feet, to where, in the inky blackness of the lower basin, sprung up in glorious sparkling light, self-born, a mighty fountain of clear molten lava.

Referring to the diagram published herewith, the reader will find that we reached the crater's edge on the eastern side at the point marked by the outline of a tent. The ancient walls that encircle the pit, marked A, on our side fell perpendicularly about five hundred feet, while on the opposite or western side they descended nearer eight hundred, to where the plateau marked B formed a floor to the crater, broken down again to form the pit marked C. The general shape of the central crater, Mokuaweoweo, was an irregular ellipse, rather more than three-quarters of a mile through its shorter axis, by about a mile and a quarter from the dividing wall marked by a dotted line on the left, that separated it from F, the crater known as Pohaku Hanalei, to a similar though not so well defined partition wall on the right hand that joined to it the crater G. Looking straight across and below us at a distance in an air line of possibly three-quarters of a mile, there rose from a cone located near the southwest corner of the lower basin a magnificent fountain of liquid lava, about seventy-five feet in diameter, that sent its volume of brilliant sparkling molten matter to a height estimated at five hundred feet in a compact and powerful jet. The axis of this gigantic fountain was somewhat inclined toward us, so that the descending cascade fell clear and distinct from the upward shooting jet, and formed one continuous fall of liquid lava, surpassingly beautiful to gaze upon. Behind this fiery fountain, a dark incline of *débris*, partly thrown up by this



outbreak, partly formed by masses falling from the wall against which it rested, reared itself against the steep side of the crater, and stood out boldly in the intense light from the incandescent fountain. On the left, with its base nearly reached by the liquid streams flowing out from the lake into which the cascade fell, was another long pile of debris that reached to the level of the plateau B. I should say here that the sides of this plateau looked to be about one hundred feet deep to where they met the edge of the lower basin. The basin itself occupied about one-third of the space bounded by the ancient walls of the crater.

Flowing down the sides of the symmetrical cone that the falling stream of lava was rapidly forming were many bright rivers of liquid light that, spreading as they flowed away, and crossing and recrossing in a tangle of bright lines, formed a lake of rivulets that, ever widening, mingling, spreading and interlacing, presented a unique and beautiful appearance. On the extreme right hand verge of this lower basin, detached pools of fire showed that while a dark crust was forming on the surface beneath, the entire area of the basin was overflowed by the melted lava.

We watched steadily the grand fountain playing before us, and called frequently to each other to note when some tall jet, rising far above the head of the main stream, would carry with it immense masses of white-hot glowing rock which, as they fell and struck upon the black surface of the cooling lava, burst like meteors in a summer sky.

As soon as we had reached the summit level of the mountain, we heard the muffled roar of the long pent up gases as they rushed out of the opening which their force had rent in the basin's solid bed. And now that we were in full view of the grand display, our ears were filled with the mighty sound, as of a heavy surf booming in upon a level shore, while ever and anon a mingled crash and break of sound would call to mind the heavy rush of ponderous waves against the rocky cliffs that girt Hawaii.

At night the jet looked loftier, and gazing intently into the fiery column with a good glass that we had, we could see the limpid sparkling upward jet rising with tremendous force from out an incandescent lake. Following up the glowing stream, we saw it arch itself and pour over as it were in one broad beautiful cascade. While the ascending stream was almost silvery in its intense brightness, the falling sheet was slightly dulled by cooling, and thus the two were ever rising, falling, shooting up in brilliant jets, and showering down with mingled dashes of bright light and shooting spray, while in the lake out of which rose the fountain, and into which fell the fiery masses, danced and played a thousand mimic waves, and fiery foam swirled round and round. Upon its surface danced myriad jets and bubbles, and from its edge flowed out the rivulets of lava, that in a tangled maze of lines covered all the lake.

6. *Volcanic Energy: An Attempt to develop its true Origin and Cosmical Relations;* by ROBERT MALLET, F.R.S. (Proc.

Roy. Soc., No. 136, 1872). (Abstract).—The author passes in brief review the principal theories which in modern times have been proposed to account for volcanic activity.

The chemical theory, which owed its partial acceptance chiefly to the fame of Davy, may be dismissed, as all known facts tend to show that the chemical energies of the materials of our globe were almost wholly exhausted prior to the consolidation of its surface.

The mechancial theory, which finds in a nucleus still in a state of liquid fusion a store of heat and of lava, etc., is only tenable on the admission of a *very thin* solid crust; and even through a crust of about 30 miles thick it is difficult to see how surface-water is to gain access to the fused nucleus, yet without water there can be no volcano. More recent investigation on the part of mathematicians has been supposed to prove that the earth's crust is not *thin*. Attaching little value to the calculations as to this, based on precession, the author yet concludes, on other grounds, that the solid crust is probably of great thickness, and that, although there is evidence of a nucleus much hotter than the crust, there is no certainty that any part of it remains liquid; but if so, it is in any case too deep to render it conceivable that surface-water should make its way down to it. The results of geological speculation and of physico-mathematical reasoning thus oppose each other, so that some source of volcanic heat closer to the surface remains to be sought. The hypothesis to supply this, proposed by Hopkins and adopted by some, viz: of isolated subterranean lakes of liquid matter in fusion at no great depth from the surface remaining fused for ages, surrounded by colder and solid rock, and with (by hypothesis) access of surface-water, the author views as feeble and unsustainable.

A source, then, for volcanic heat remains still to be found; and if found under conditions admitting to it water, especially of the sea, all known phenomena of volcanic action on our earth's surface are explicable.

The author points out various relations and points of connection between volcanic phenomena, seismic phenomena, and the lines of mountain elevation, which sufficiently indicate that they are all due to the play of one set of cosmical forces, though different in degree of energy, which has been constantly decaying with time.

He traces the ways in which the contraction of our globe has been met, from the period of its original fluidity to the present state: first, by deformation of the spheroid, forming generally the ocean basins and the land; afterward by the foldings over and elevations of the thickened crust into mountain ranges, etc.; and, lastly, by the mechanism, which he points out as giving rise to volcanic action. The theory of mountain elevation proposed by C. Prevost was the only true one—that which ascribes this to tangential pressures propagated through a solid crust of sufficient thickness to transmit them, those pressures being produced by the relative rate of contraction of the nucleus and of the crust; the former being at the higher temperature, and having a higher co-efficient of contraction for equal loss of heat, tends to shrink away from beneath the crust, leaving the latter partially unsupported. This, which during a much more rapid rate of cooling from higher temperature of the whole globe and from a thinner crust gave rise in former epochs to mountain elevation, in the present state of things gives rise to volcanic heat. By the application of a theorem of Lagrange, the author proves that the earth's solid crust, however great may be its thickness, and even if of materials far more cohesive and rigid than those of which we must suppose it to consist, must, if even to a very small extent left unsupported by the shrinking away of the nucleus, crush up in places by its own gravity and by the attraction of the nucleus.

This is actually going on, and in this partial crushing, at places or depths dependent on the material, and on conditions pointed out, the author discovers the true cause of volcanic heat. As the solid crust sinks together to follow down after the shrinking nucleus, the *work* expended in mutual crushing and dislocation of its parts is *transformed into heat*, by which, at the places where the crushing sufficiently takes place, the material of the rock so crushed and of that adjacent to it are heated, even to fusion. The access of water to such points determines volcanic eruption. Volcanic heat, therefore, is one result of the secular cooling of a teraqueous globe subject to gravitation, and needs no strange or gratuitous hypothesis as to its origin.

In order to test the validity of this view by contact with known facts, the author gives in detail two important series of experiments completed by him:—the one on the actual amount of heat capable of being developed by the crushing of sixteen different species of rocks, chosen so as to be representative of the whole series of known rock formations from Oolites down to the hardest crystalline rocks; the other, on the co-efficients of total contraction between fusion and solidification at existing mean temperature of the atmosphere of basic and acid slags, analogous to melted rocks.

The latter experiments were conducted on a very large scale, and the author points out the great errors of preceding experimenters, Bischoff and others, as to these co-efficients.

By the aid of these experimental data, he is enabled to test the theory produced when compared with such facts as we possess as to the rate of present cooling of our globe, and the total annual amount of volcanic action taking place upon its surface and within its crust.

He shows, by estimates which allow an ample margin to the best data we possess as to the total annual vulcanicity of all sorts of our globe at present, that less than one fourth of the total heat at present annually lost by our globe is upon his theory sufficient to account for it; so that the secular cooling, small as it is, now going on is a sufficient *primum mobile*, leaving the greater portion still to be dissipated by radiation. The author then brings his views into contact with various known facts of vulcanology and seismology, showing their accordance.

He also shows that to the heat developed by partial tangential thrusts within the solid crust are due those perturbations of hypogeal increment of temperature which Hopkins has shown cannot be referred to a cooling nucleus and to differences of conductivity alone. He further shows that this view of the origin of volcanic heat is independent of any particular thickness being assigned to the earth's solid crust, or to whether there be at present a liquid fused nucleus, all that is necessary being a *hotter* nucleus than crust, so that the rate of contraction is greater for the former than the latter. The author then points out that, as the same play of tangential pressures has elevated the mountain chains in past epochs, the nature of the forces employed sets a limit to the height of mountain possible of the materials of our globe.

That volcanic action due to the same class of forces was more energetic in past time, and is not a uniform but a decaying energy now. Lastly, he brings his views into relation with vulcanicity produced in like manner in other planets, or in our satellite, and shows that it supplies an adequate solution of the singular and so far unexplained fact that the elevations upon our moon's surface, and the evidences of former volcanic activity, are upon a scale so vast when compared with those upon our globe.

Finally, he submits that if his view will account for all the known facts, leaving none inexplicable, and presenting no irreconcilable conditions or necessary deductions, then it should be accepted as a true picture of nature.

7. *Solvent action of water.* From the Anniversary Address of J. Prestwich, President of the Geological Society, February, 1872.—Let us now look at the geological bearing of the question connected with the solvent action of the water on the strata it traverses. The analyses, made for the Commission by Drs. Frankland and Odling, of the waters of the Thames and its tributaries in the Oolitic and Chalk area, show that the rain-water has taken up of solid matter in every 100,000 parts or grains a quantity varying from 25·58 to 32·95 parts or grains, or an average of 29·26, which is equal to 20·48 grains per gallon; another analysis of the Thames water at Ditton gives 20·78 grains per gallon of solid residue. It was also shown by Drs. Letheby and Odling and Professor Abel that the unfiltered waters of the Thames Companies, which take their supplies above Kingston, contained 20·82 of solid residue. If from the average of 20·68 we deduct 1·68 grain for organic and suspended matter, we have 19 grains of dissolved inorganic matter for every gallon of water flowing past Kingston. This is of course apart from the sediment carried down in floods. The ordinary monthly analysis, conducted by the same eminent chemists during the course of several past years, shows that this quantity is liable to very little variation, the only difference being that it is somewhat larger in winter and less in summer.

Some general estimates have already been made by Professors Ramsay and Geikie of the quantity of mineral matter carried down in solution by the Thames; but the more exact data supplied to the Commission enables us to make some additions to previous results. Taking the mean daily discharge of the Thames at Kingston 1250 million gallons, and the salts in solution at 19 grains per gallon, the mean quantity of dissolved mineral matter there carried down by the Thames every twenty-four hours is equal to 3,364,286 lbs. or 1502 tons, which is equal to 548,230 tons in the year. Of this daily quantity about two-thirds, or say 1000 tons, consist of carbonate of lime, and 238 tons of sulphate of lime; while limited proportions of carbonate of magnesia, chlorides of sodium and potassium, sulphates of soda and potash, silica and traces of iron, alumina and phosphates constitute the rest. If we refer a small portion of the carbonates, and the sulphates and chlorides chiefly, to the impermeable argillaceous formations washed by the rain-water, we shall still have at least 10 grains per gallon of carbonate of lime, due to the Chalk, Upper Greensand, Oolitic strata, and Marlstone, the superficial area of which, in the Thames basin above Kingston, is estimated by Mr. Harrison at 2072 square miles. Therefore the quantity of carbonate of lime carried away from this area by the Thames is equal to 797 tons daily, or 290,905 tons annually, which gives 140 tons removed yearly from each square mile; or, extending the calculation to a century, we have a total removal of 29,090,500 tons, or of 14,000 tons from each square mile of surface. Taking a ton of chalk, as a mean, as equal to 15 cubic feet, this is equal to the removal of 210,000 cubic feet per century for each square mile, or of $\frac{9}{100}$ of an inch from the whole surface in the course of a century, so that in the course of 13,200 years a quantity equal to a thickness of about 1 foot would be removed from our Chalk and Oolitic districts.

8. *Correlations of the Coal Measures of Britain, France and Belgium.* From the Anniversary Address of the President of the Geological Society, Joseph Prestwich, F.R.S., February, 1872.—It may be asked if any correlation can be established between the coal measures of Bristol and South Wales, and those of France and Belgium. So far as the identity of any particular bed of coal or of rock, it is impossible, and we should not expect it; for the variation in all the beds of any coal basin is well known to be so great and rapid that in the different parts of the same basin it is often difficult, and sometimes impossible, to establish any correlation, while in adjacent basins, such as those of Wales and Bristol, or of Hainaut and Liége, such attempts have, with few exceptions, hitherto utterly failed. There are, however, general features which serve to show some relationship. The great dividing mass of from 2000 to 3000 feet of rock called Pennant exists in both the Welsh and Bristol coal field; and the total mass of Coal-measures is not very different, it being, say, 10,500 feet in the one, and 8500 in the other, and there being in Wales 76, and in Somerset 55 workable seams of coal. In the Hainaut (or Mons

and Charleroi) basin, the measures are 9400 feet thick, with 110 seams of coal; in the Liége basin 7600 feet, with 85 seams; and in Westphalia 7200 feet, with 117 seams. On the other hand, none of our central or northern coal basins, with the exception of the Lancashire field, exceed half this thickness, and more generally are nearer one fourth. Further, the difference which exists between the northern coals and those of Wales and Somerset, the preponderance of caking coals in the north, and of anthracite, steam, and smiths' coal in the south, equally exists between our northern coals and those of Belgium, which latter show, on the one hand, close affinities with those of Wales and Bristol. I am informed by two experienced Belgian coal-mining engineers and good geologists, who have twice visited our coal districts, that the only coals they found like those of Belgium were the coals of South Wales and Radstock; there was the same form of cleavage, the same character of measures, and the same fitness for like economical purposes. Organic remains afford us a little help; but not sufficient is yet known of their relative distribution. The plants are, as usual, the same; so also are shells of the genus *Anthracosia*, and a number of small Entomostraca; while there is a scarcity of the marine forms which are more common in some of our central and northern fields. That, therefore, which best indicates the relation between the coal fields of the southwest of England and those of the north of France and Belgium, is the similarity of mass and structure, uniformity of subjection to like physical causes, and identity of relation to the underlying older and to the overlying newer formations.

It was in the north that the conditions fitted for the formation of coal first set in. The common *Stigmaria ficoides* and various Coal-measure plants appear at the base of the Carboniferous or in the Tuedian series of Northumberland, which there overlies conformably the Upper Old Red Sandstone; and productive beds of coal exists low down in the Mountain-Limestone series. These disappear in proceeding southward, and the great productive coal series becomes confined to beds overlying the Millstone Grit. If the coal growth set in earlier in the north, it seeme to have been prolonged further south, under more favorable conditions, to a later period. What those conditions were—whether the proximity of a greater land-surface, of a longer and greater subsidence, with more numerous rests—we cannot yet pretend to say.

9. *Recent Observations in the Bermudas;* by MATTHEW JONES.—As my late visit to these islands has placed me in possession of facts relating to their original aspect of a somewhat conclusive nature, I deem it advisable to communicate such in a brief form, instead of awaiting the time requisite for the preparation of a more elaborate paper on the subject.

On previous occasions I have always regretted my inability, from lack of time, to look more closely into their geological character in the hope of discovering some satisfactory clue to their primitive condition. I was aware that in different parts of the islands road cuttings and well borings had revealed layers of red earth at certain depths below the surface, the consistence of which was similar to that now forming the present surface soil, and it did not require much force of imagination, after personal inspection, to conceive that such layers of red earth were first formed by the decomposition of vegetable matter which grew upon former surfaces, and became covered to their respective depths by accumulated masses of drift sand, which from natural causes hardened into more or less compact sandstone. But these different layers were but a few feet beneath the surface, and so, although interesting as throwing light upon the gradual elevation of the land by drift material forming over them, yet they afforded no evidence of a contrary nature—viz: the *submergence* of the Bermuda group. Indeed, I have always been led to suppose from appearances that the whole group was the result of an upheaval of the ocean bed slightly above the water level, and a gradual elevation afterward by means of drift matter aided by the consolidating agency of reef-building zoophytes encircling the whole with a barrier reef, and by isolated patches gradually filling up the space within. The investigations, however, which I have recently been able to make, tend I think to prove that the barrier reef encircling the islands, which has hitherto been considered an atoll, is merely the remnant of the more compact calcareous rock which formed the shore of a much more extensive island group than that now existing.

My views in this respect are borne out by the following facts: —The barrier reef, as far as I have inspected it, is merely ordinary calcareous rock coated with Serpulæ, Nullipores, &c., the reef builders working only in the sheltered waters between the reef and the shore in three to eight fathoms. About two years ago submarine blastings were carried on at the entrance of Hamilton Harbor, and at a depth of over six fathoms a cavern was broken into which contained stalactites and red earth. Again within the last few months, I have, through the kindness of his Excellency Major-General Lefroy, C.B., F.R.S., the present Governor, been placed in possession of still more satisfactory information. During the past two years extensive submarine blastings have taken place inside an artificial harbor, situated at the western extremity of the islands, for the purpose of forming a bed of sufficient depth for the reception of the "Great Bermuda Dock," which attracted so much attention off Woolwich when launched some three or four years ago. The excavations extended to a depth of fifty-two feet below low water mark. At forty-six feet occurred a layer of red earth two feet in thickness, containing remains of cedar trees, which layer rested upon a bed of compact calcareous sandstone. Here we have the first satisfactory evidence of the submergence of an extensive deposit of soil once upon the surface, and that to the depth of forty-eight feet below the present low water level, which consequently grants an equal elevation above it in former times. Now, on carefully surveying the Bermuda chart, we find that an elevation of forty-eight feet will bring the whole space which intervenes between the present land and the barrier reef, now covered with water, above the water level. This attained, what more is required to prove the former extent of the island group before the present submergence to the present barrier reef? But having clearly ascertained beyond doubt that the Bermudas were once forty-eight feet higher than at present, will any one be bold enough to deny them a greater elevation? I have reason to believe that they once extended in a south-westerly direction—not only out to the reef, but to a greater distance. There are some rocky ledges about twenty to twenty-five miles from land in that direction, known as "The Flatts," lying in about thirty five to forty fathoms water; and, singularly enough, in the very oldest maps of the Atlantic, copies of which I have consulted in the British Museum, "The False Bermudas" are put down about this position. Is it unreasonable to suppose that a low lying group of islets did actually exist here in former times? Again, in Smith's "History of Virginia," which gives an excellent account of the islands in the early part of the seventeenth century, it is stated, among other notes upon their natural history, that flocks of crows, no doubt the same species (*Corvus Americanus*) which now inhabits them, were in the habit every evening of winging their flight from the main island toward the north. This observation, which from its simplicity I should the more readily believe to be a true statement, would clearly prove the existence of land in that direction at no great distance; for the habit of this bird to leave its roosting place for distant feeding grounds during the day, to return at random, is one of its well-known characteristics.

Taking these matters into consideration, I see everything to support the supposition that the Bermudas once presented a much more extensive aspect than they do at present, and certain additional evidences which I hope to bring forward shortly in a collected form, will, I conceive, tend to confirm my impression that the restricted terraqueous area lying within the limits of the outer barrier reef is merely the summit of one of a range of islands which extended in somewhat semicircular form for a distance of seventy or eighty miles, and which have suffered submergence to a depth only to be correctly ascertained by borings, which might be successfully accomplished under the auspices of the Government at a trifling expence.—*Nature*, *Aug.* 1.

10. *History of the names Cambrian and Silurian in Geology*, by T. Sterry Hunt. 64 pp. 8vo. From the Canadian Naturalist for April and July, 1872.—Prof. Hunt has here made a valuable contribution to historical geology. But the conclusion of the whole matter that the name Cambrian should be now used in this and other lands for the Primordial or part of it, because this would be in accordance with "historic truth," does not seem necessarily to follow.

In England, the so-called Cambrian has turned out, as Hunt recognizes, Primordial in its upper half at least—a part now called the Menevian group,* with the underlying Harlech grits; that is, it contains Paradoxides and the same range of generic forms that Barrande and others had previously found in the European Primordial; and a transfer, therefore, of all to the Primordial was natural. But Prof. Hunt says, after alluding to the views of some others, that Barrande's course "is a still greater violation of historic truth," as if error which history had made venerable might never be eradicated from science.

The term Silurian, as used in Great Britain, has included a wide range of formations, from the Lingula Flags to the top of the Ludlow group, and all this in spite of a wide range in the tribes of fossil species, and notwithstanding the unconformability between the Upper and Lower Silurian. Now the Lingula Flags pass into the Cambrian without break or unconformability, and with but a small change in the life. What good scientific reason is there for cutting off this comprehensive division of geological time, the Silurian, at a point both stratigraphically and paleontologically unimportant? Why should Murchison's or Sedgwick's determination of the limits of the Silurian, made in an early stage of the science, have anything more than a historical interest? To throw the Lingula flags down into the Cambrian, as Lyell has done, is violating "historic truth" as much as to throw the Primordial Cambrian beds up. "Historic truth," in fact, has little weight in the question, though important as regards the labors of two eminent English geologists. Whichever course best exhibits the system of geological truth should be the one adopted by the science. If the term Cambrian has advantages over Primordial sufficient to make its substitution for the latter desirable, that will take place, whatever the past may say; but otherwise, not. In a similar manner, if the distinction between a *Paradoxides* and an *Olenus*, and other differences less important between the living species of these groups, is not enough to demand that the Primordial (or Cambrian) should be separated from the Silurian and be made a separate and equivalent grand division in the system, it should not be done whatever the authority for it.

11. *Report of the Geological Survey of the State of New Hampshire*, showing its progress during the year 1871; by C. H. HITCHCOCK, Ph.D. 56 pp. 8vo. Nashua, N. H., 1872.—Prof. Hitchcock has made great progress during the year in determining the distribution of the Labradorite and other rocks of the White Mountain region, and is throwing much light on this dark and most difficult part of American geology. His report is accompanied by a colored map, showing the areas occupied by the different kinds of rocks. There are also descriptions of the

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* Some of the species described from it by Salter are *Paradoxides Davidis, P. Aurora, P. Hicksii, Anopolenus* (near Paradoxides) *Henrici, A. Salteri* Hicks, *Conocoryphe* (Conocephalites) *variolaris, C. Bufo* Hicks, *C. applanata, C.* (?) *humerosa, Holocephalina* (near Conocoryphe) *primordialis, Agnostus princeps, Microdiscus punctatus, Leperditia Solvensis* Jones, *Theca corrugata, Protospongia fenestrata.* See further, Quart. Jour. Geol. Soc., xx, 233, xxi, 476, xxv, 51, xxviii, 173 and Rep. Brit. Assoc. for 1865, 1866 and 1868.

rocks, and a number of analyses of the contained feldspar. The Report also contains very valuable tables of heights along different railroads, and in the mountains. There are brief accounts also of iron-ore in Bartlett, and the alluvial gold of Indian Stream. The preparation of the final quarto Report on the geology of the State is stated to have been begun.

12. *Memorie per servire alla Descrizione della Carta Geologica d' Italia*; publicate a cura del R. Comitato Geologico del Regno. Vol. I. 364 pp. 8vo.—A beautiful volume of Italian geological memoirs, illustrated by a geological map of the Island of Elba, and many plates of sections and fossils. The figures of Tertiary fossils fill seven quarto plates, and are admirably engraved.

13. *On the Occurrence of Native Sulphuric Acid in Eastern Texas;* by J. W. Mallet, Ph.D. (Proc. Brit. Assoc., 1872.)—Not far from the Gulf of Mexico, and within twenty-five or thirty miles to the westward of the Neches river, there occur at several localities—in some instances in the woods, in others in the midst of open prairie—small drainage-wells and shallow pools of water strongly sour to the taste. This sourness is due to the presence of free sulphuric acid, which is accompanied by various salts, especially aluminium and iron sulphates. At most of these points gases are continually escaping (hydrogen sulphide, marsh gas, and carbonic anhydride), the bubbles burning readily on the application of a light.

At the bottom of the water in some instances, as at one point where, by means of an artificial bank, a pond has been formed of some 250 feet in diameter, known locally as the "sour lake," an earthy crust with intermingled free sulphur is observable. A thick, tarry variety of petroleum is found oozing from the surrounding soil, occasionally to such an extent that sods taken up with a spade are set on fire and used to give light in the open air at night.

At a point in Louisiana some fifty or sixty miles further east, where, however, the acid water does not occur, though combustible gas and petroleum are met with on the surface, a most remarkable bed of native sulphur, 100 *feet in thickness*, has been reached at the depth of 450 feet by boring, and a shaft is being at present sunk for its exploitation. This large mass of native sulphur is more or less mingled with calcium carbonate, and underlaid by gypsum. The circumstances connected with the occurrence together in this region of combustible gases, petroleum, sulphur, sulphuric acid, and gypsum are of great interest in relation to the mineral history of native sulphur.

The sulphuric acid water, which seems to be probably altogether of superficial origin, is worthy of notice from the unusual strength occasionally attained. The water varies very much at the different localities and at different times. In one instance, a specimen examined by Dr. Mallet contained no less than 5·290 grms. of free sulphuric acid ($H_2SO_4$) to the litre, or 370 grs. to the imperial gallon, this exceeding any amount hitherto reported from other localities, unless the acid spring of the Paramor de Ruiz, in New Granada, be an exception, examined by Lewy, who does not state precisely how much of the very large quantity of sulphuric acid found is uncombined with bases. The water of the Rio Vinagre, flowing from the volcano of Purace in the Andes of Popayan, as described by Humboldt and Boussingault, contains only 1·11 of free sulphuric acid ($SO_3$?) in 1000 parts of the water, with 0·91 of hydrochloric acid.

14. *Analysis of a Compact Talc from North Carolina;* by Mr. J. B. ADGER, of the Laboratory of the University of Virginia. (Chemical News, No. 654.)—Among the minerals referred to there was a very beautiful "soapstone, from the Nantahela Mountains, 8 miles from the mouth of Nantahela River, Swayne Co. (formerly Cherokee Co.), N. C." It had been sawn into slabs of about 1¼ inches thick, was very uniform in character, compact, with indistinct traces of foliated structure, white with a faint greenish shade, lustre pearly, streak white, moderately translucent, greasy to the touch, hardness = about 1·25, sp. gr. = 2·82. It resembled somewhat the finer and light-colored specimens of Chinese jade or nephrite. Analysis afforded: Silica 57·72, magnesia 33·76, alumina 2·52, iron monoxide 0.64, water 6·01=100·65.

If the silica, magnesia, and water, alone be considered, the above numbers correspond pretty closely to the formula $(\frac{5}{6}MgO+\frac{1}{6}H_2O),SiO_2+\frac{1}{6}H_2O$.

The mineral is obviously distinct from the foliated talc from Webster, Jackson Co., N. C., analyzed by Genth (this Jour., II, xxxiii, 200, as quoted in Dana's "Mineralogy," 5th ed., p. 453). In the latter 0·23 per cent of nickel oxide was found, and but 0·34 per cent of water.

In the mineral now described there is no nickel.

University of Virginia, March 12, 1872.

15. *Leucite.*—Prof. vom Rath, the excellent crystallographer of Bonn, has found, through the examination of a twin crystal, as well as by measurements, that the crystals of leucite, instead of being isometric trapezohedrons, are really tetragonal.

16. *On the occurrence in recent Pine timber of Fichtelite, a hydrocarbon hitherto known only in a fossil state;* by J. W. MALLET, Ph.D. (Proc. Brit. Assoc., 1872.)—Some nearly colorless crystalline crusts, found in clefts between the annual rings of growth of a log of long-leafed pine (*Pinus Australis*) in Alabama, were found to dissolve in boiling alcohol (more easily in ether), and, on cooling, to crystallize in monoclinic forms with greater distinctness. A specimen was exhibited of this material purified by two or three re-crystallizations; it had been found to agree perfectly in physical and chemical properties with the fichtelite of Bromeis and Clark, and on analysis yielded—

| | |
|---|---|
| Carbon .. .. .. .. .. .. .. | 87·82 |
| Hydrogen .. .. .. .. .. .. | 11·91=99·73 |

agreeing with the formular $x(C_5H_8)$. The fusing-point was found =45°C.

17. *Botanical Publications and Intelligence.*—Among the publications that have come to hand the most important to American botanists is the

*Genera Lichenum, an Arrangement of the North American Lichens*, by Edward Tuckerman, M.A. Amherst: Edwin Nelson. 1872. pp. xv, 281, 8vo.—This small volume contains the mature and long-considered results of many years of earnest study, and has been retarded and (as respects its introductory chapter) curtailed by serious illness brought on by overwork upon it. Having carried it through the press, the author is seeking repose and complete restoration in the Old World, whither our best thanks and wishes, and those of all his botanical associates, follow him. We hope for his return in due time, in full vigor, and that the "Synopsis of the North American Lichens, which is in preparation, but for the present necessarily laid aside," may be resumed and completed. Then our students of lichens, and those who would fain be such, will for the first time be supplied with text books for the study, and those of the very highest order of merit. The present work, addressed to lichenologists and necessarily critical, not to say recondite, needs to be supplemented by the Synopsis, which will be a practical guide to the beginner. We are incompetent to criticize this volume; but we may say that the print and paper are truly excellent, and if, as the imprint indicates, the composition and press-work were done in a country office, it is wonderful.

*The Flora of British India*, by J. D. Hooker, C.B., &c., assisted by various Botanists. Part I. pp. 208, 8vo. London: L. Reeve & Co. 1872.—The Indian Flora is here begun again, in a form and scope and with a vigor which render its completion hopeful. "The originally contemplated and more extended plan," exemplified in the first and only volume of Dr. Hooker and Dr. Thomson's *Indian Flora*, being abandoned, on account of the long-continued ill-health of the latter and the manifold other duties of the former, the task is now taken up more advantageously in a form like that of the other British Colonial floras, but still more condensed, indeed, nearly on the model of Dr. Hooker's Flora of the British Islands. This gives on the average between three and four species to the page. The references are sufficient; and the terse characters are more serviceable than long descriptive phrases. The work will be voluminous enough when the 14,000 Indian species are described in it. The present half volume begins with *Ranunculaceæ* and ends with *Polygalaceæ*. Dr Thomson's name is associated with the editors in the earlier orders; Dr. Anderson's in *Cruciferæ;* and the *Polygalaceæ* are elaborated by Mr. A. W. Bennett.

*Grevillea*, a Monthly Record of Cryptogamic Botany and its Literature; edited by M. A. Cooke, M.A. Williams & Norgate, 8vo. In parts of 16 pages; sixpence a number; five shillings a year.—Three numbers, July—September, are before us; each has a plate, colored or plain; and, although Fungi naturally predominate in the letter-press, the other lower Cryptogams have their share of attention. The characters of the *Fungi* described by Mr. Peck in the 23d Report of the Regents of the University of the State of New York, are here reproduced. A series of articles by the veteran mycologist Berkeley, describing North American Fungi, begins in the third number. In fact, American Cryptogamic Botany is likely to receive no small share of attention, and we would warmly commend this little journal to all who in this country are interested in these subjects. We think the work will prove worthy to bear the honored name of one of the most worthy of British Cryptogamists of the last generation.

*Linnæa.*—The third volume of the new series, edited by Garcke (of which three fascicles are at hand), continues Böckler's long account of the *Cyperaceæ* of the Berlin herbarium, and also the late Dr. Rohrbach's papers on *Caryophylleæ.* The portion of the latter now published relates mainly to the *Alsineæ* of the New World, from Mexico southward, and is edited by Dr. Garcke. It is seldom wise to print posthumous manuscript which is not fairly completed by the author. For instance, *Lastarriæa* of Remy is here included in the *Illecebreæ* or *Paronychieæ*, with the remark that, although Remy, with all the characters before him, had referred the genus to the *Polygonaceæ*, Mr. Bentham had correctly transferred it to the former order, and had even been anticipated by Kunze and (in MSS.) by Kunth. If Rohrbach, and still more, Bentham, had really noticed the orthotropous ovule and seed, they could hardly have failed to see the Polygonaceous character. The genus, as we have elsewhere indicated, is as it were a *Chorizanthe* without a distinct gamophyllous involucre, but with perianth imitating it.

Casimir DeCandolle, in the third fascicle, publishes descriptions of new *Piperaceæ* which have come to his notice since the publication of that order in the Prodromus. It begins with "*Anemiopsis Bolanderi*," from California, founded on specimens from Dr. Bolander which are yet unknown to us.

The *Flora Brasiliensis*, continued by Professor Eichler, has Dr. Masters' revision of the *Passifloraceæ* for the 57th fascicle; and this completes vol. 13, part 1, with index, &c. The synopsis of all the known American species is a great help. Fascicle 58, which completes the second part of vol. 14, contains the *Phytolaccaceæ*, by J. A. Schmidt of Hamburg, and the *Nyctagineæ* by the same; also the *Crassulaceæ* (only two indigenous species) and *Droseraceæ* (a dozen of *Drosera*) by the editor himself. He remarks (from Caspary) that in all the pentamerous species of *Drosera* the second sepal faces the axis, while in *Aldrovranda* the sinus between the second and fourth is in this position, the third being superposed to the bract. *D. intermedia* (i. e., *D. longifolia* L., var. *Americana*) extends to Brazil, where *D. graminifolia* represents our northern *D. filiformis.* Fascicle 59 is a very thin one; it contains only the *Equisetaceæ*, which were elaborated by the late Prof. Milde, and the two sheets were printed more than two years ago. They have been awaiting Prof. Braun's account of the *Rhizocarpeæ* and *Isoetes*, which, when issued, will complete the volume of the higher Cryptogamia.

*Prof. Hofmeister* has accepted the botanical chair at Tübingen, vacated by the death of Von Mohl; *Dr. Sachs* succeeds Hofmeister at Heidelberg; and *Dr. Hegelmaier* is the new professor at Kiel. Prof. Kerner has been transferred from Innspruck to the University of Prague. By a misprint in the July number of this Journal, the name of the late *Dr. Wight* was given as Dr. Wright.

*Herbarium of the late Dr. Curtis.*—A biographical notice of the late Dr. M. A. Curtis, which has been already too long defered, must now be postponed to the January number. His herbarium is to be sold by his family, under directions from Dr. Curtis that it shall be kept together if practicable. Clearly this collection ought to be retained in North Carolina, being authentic for the flora of that State and the exponent of his full catalogues and descriptions of the plants of North Carolina, published by the State Government. A moderate sum would probably secure it, and would be most worthily invested to that end. If the State should not acquire it, or if it should be found necessary to divide it, the collection of *Fungi* ought to find a ready sale, as it contains the types of the hundreds or even thousands of species which Dr. Curtis has described, or at least determined and catalogued. Its loss to science, or even to this country, would be irreparable.

Bentham and Hooker's *Genera Plantarum*, vol. 2.—We have seen more of the earlier sheets of this volume, the first part of which may be expected at the close of the year. A. G.

17. *A Handbook of Chemical Technology;* by RUDOLPH WAGNER, Ph.D., Professor of Technology at the University of Wurtzburg. Translated and edited from the eighth German edition, with extensive additions, by William Crookes, F.R.S. Large 8vo, pp. xvi, 745, xvi. New York, 1872. (D. Appleton & Co.)—Professor Wagner's name has become universally known by his excellent Jahresbericht der chemischen Technologie, which first appeared in 1856. The first edition of the work before us was issued in 1850, and a new edition has since appeared regularly every three years, constantly enriched and improved by the progress of discovery. The eminence of the author in his chosen field ensures a valuable volume. And, although, from the necessities of the case, it must be somewhat cursory in its plan, since it is obviously impossible to get the whole of applied chemistry into such a volume, yet we are agreeably disappointed at the number of the subjects considered and the thoroughness with which they are treated. Mr. Crookes deserves praise, also, not only for the excellence of his translation, but for the original matter he has added. Now that we have in English so good a text-book upon Chemical Technology, we trust that more attention will be paid to teaching it systematically in our schools of science.

18. *On Beavers and Beaver Dams in Mississippi;* by Mr. JOHN SHELTON. (From a letter to one of the editors, dated Raymond, Hinds Co., Mississippi, Sept. 12.)—I have resided in this

county since 1837, now for nearly thirty-five years. When I came here I was young and somewhat given to hunting. At the outset, to my inquiries of other hunters, whether there were beavers here, it was replied, that there were a few, but no one could then tell me where there was one of their dams in this neighborhood.

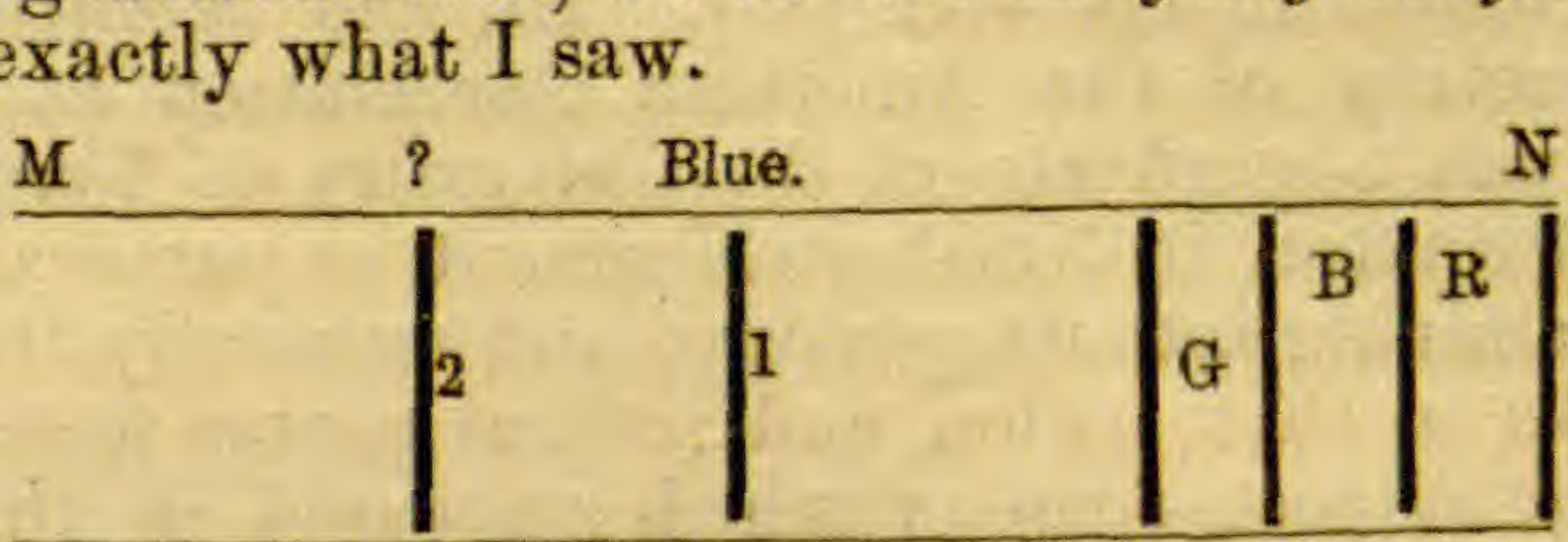
And yet by the year 1850, their dams were to be found in nearly all the streams in the county, that were not so small as to become dry during our long summers, or two large for the operations of the beaver. They continued to increase, greatly to the injury of most of our low land, and to the annoyance of its cultivators. In 1858 or 1859, a professional trapper from Wisconsin, if I am not mistaken, caught seventy-five or eighty beavers in this county in less than a month's time.

They are yet increasing in this county, as I have no doubt they are in all the counties of central Mississippi and Alabama, and perhaps entirely throughout both States. I have no doubt that, in Hinds county, they are more than half as numerous as the population. I now write in the Court House of the county, and they can be found in sight of it, and at a less distance than one mile.

## III. Astronomy.

1. *Spectrum of the Aurora;* by Edward I. Holden, 2d Lieut. of Engineers. (From a letter to the editors, dated West Point, N. Y., Oct. 14, 1872.)—I have this evening succeeded in observing the spectrum of a very fine aurora, which appeared about 7 P. M., and lasted perhaps 20 minutes. It first appeared as a rosy cloud about 15° wide and perhaps 30° high, bearing N. 30° W. by compass. Afterward it spread to the zenith, and was principally in the shape of a band, of (say) 15° wide extending from the N.W. to the E. No pulsations of any magnitude were evident, but a radiated structure was manifest.

The spectroscope (pocket, by Hawkins & Wales) was first turned on the full moon, and an idea of the length of the spectrum obtained; then with a wide slit it was turned on the aurora, and the following sketch made, which was *carefully verified*, so that it represents exactly what I saw.



The length MN is what I conceived to be the length of the spectrum given by my instrument under usual conditions. The violet (extreme) rays seemed cut off, and I saw 1° a broad and bright *red* band (R), 2d a black space equal in width to it (B), 3d a green and bright band (G) nearly as wide, then a faint spectrum of diffused light, and a bright line in the blue (1), then a bright line more refrangible but whose color could not be definitely *seen* (2). The relative distances for my instrument are kept in the drawing.

I then opened Angström's "Spectre Normal," and saw that he gave the auroral line as in the yellow. I observed this green line again, and cannot persuade myself that it was yellow. The black space I am *sure* of; and it was also seen plainly by an inexperienced person, into whose hands I put the instrument. The slit was then narrowed and turned on the moon, and adjusted to give the Fraunhofer lines most clearly. The aurora by this time was fainter, and I can only be sure of a bright line (green) with a suspicion of my former blue line. Opening the slit again, the red band of the diffused light spectrum was *close* against the green bright line. The aurora then faded. I mention this black space as it is not what I expected to see from my reading of Angström and Winlock.

## IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Institute of Technology, Boston.*—Prof. T. Sterry Hunt, for twenty-five years connected with the Geological Survey of Canada, has entered upon his duties as Professor of Geology in the Institute of Technology.

2. *Annual Rep. of the Director of the Meteorological Observatory, Central Park, New York.* 42 pp. 8vo.—At the close of this excellent Report there is a series of synoptic charts, one for each month of the year, giving the mean height of the barometer, that of the thermometer, and the strength of the wind for the month.

3. *Hayden's Geological Exploration in the Rocky Mountains.*—A letter from Dr. Hayden, dated Gallatin City, Montana, Oct. 10, states that his two parties have been successful in their work at every point; and that no accident or sickness has occurred. Dr. Hayden's branch of the exploration will have for the Report a map of a territory 10,000 square miles in area, with contour lines of 100 feet.

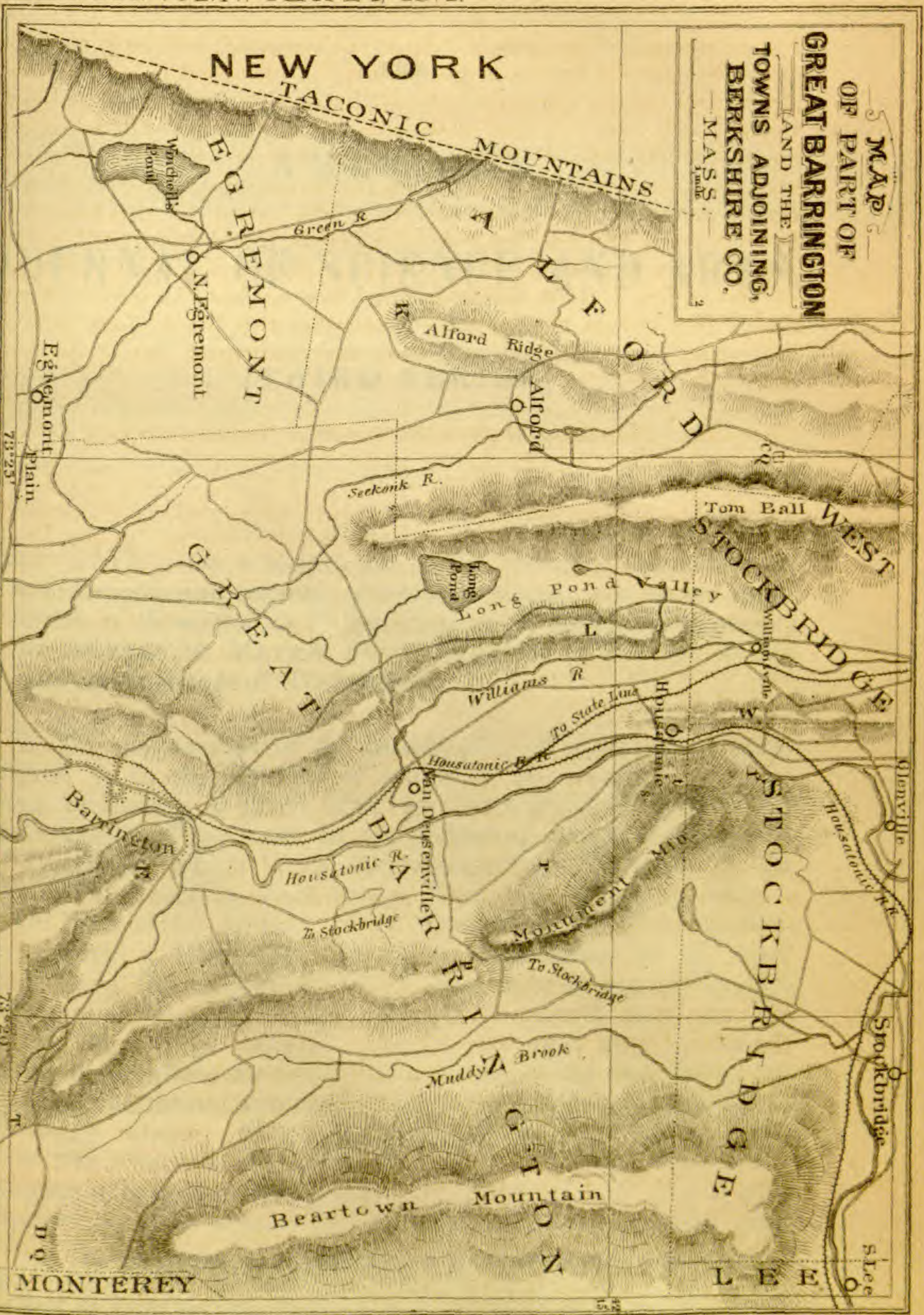
### OBITUARY.

Rev. JOHN B. PERRY.—Professor Perry, in charge of the Department of Primordial Geology in Harvard College, died on the 30th October, in his 47th year. He had recently returned from the meeting of the American Association at Dubuque. Prof. Perry was a graduate of the University of Vermont. His residence at Swanton, in Northern Vermont, as pastor of a church, led to his examining into its geology, and especially the so-called Taconic rocks of that region, and several papers were published by him on the subject, some of which appeared in this Journal. He was greatly interested in his favorite science, and active as an observer.

JOHN F. FRAZER, LL.D., Professor of Natural Philosophy and Chemistry in the University of Pennsylvania, died on Saturday afternoon, at a quarter before three o'clock, of heart disease. A further notice will be given in our next issue.

On the Geology of Lower Louisiana and the Salt Deposit on Petite Anse Island; by Eugene W. Hilgard, Ph.D., Prof. Chem., Univ. Mississippi. 34 pp. 4to. No. 248 of the Smithsonian Contributions to Knowledge.

AM. J. SCI. VOL. IV. PLATE 4, 1872.



THE

# AMERICAN

# JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

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Art. XLIX.—*On a simple and precise method of measuring the wave-lengths and velocities of sound in Gases; and on an application of the method in the invention of an Acoustic Pyrometer;* by Alfred M. Mayer, Ph.D., Professor of Physics in the Stevens Institute of Technology.

1. *The measurement of the wave-length.*—Without any consideration as to the velocity of sound or the number of vibrations producing a given note, we can accurately measure the wave-length of the note by the following simple arrangement of apparatus, which is an instrumental simplification of the method first used by Zoch (Pogg. Ann., vol. cxxviii, p. 497.)

On the acoustic bellows fix an organ pipe, and place opposite its mouth a Helmholtz resonator responding to its note. Lead from the resonator a thick gum tube to one of König's manometric capsules, whose gas jet is placed near the jet of the organ pipe capsule. Sound the pipe, and by means of the manometric flame-micrometer* adjust the two flames so that their serrations appear to coincide when viewed in a cubical revolving mirror. Now suppose, for simplicity, that the pipe gives 342 complete vibrations in one second; then, taking the velocity of sound at 342 meters per second (at 15° C.), it will require $\frac{1}{342}$ of a second for an aerial pulse to traverse one

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* See "On the method of detecting the phases of vibration in the air surrounding a sounding body; and thereby measuring directly in the vibrating air the length of its waves and exploring the form of its wave-surface," Nov. No. of this Journal. In this paper I gave the credit of the suggestion on which I founded my micrometer to M. Radau; but I find that it is due to Zoch (Pogg. Ann., vol. cxxviii). M. Radau mentions it in his *L'Acoustique* without giving credit to the real inventor.

meter. Therefore, if the resonator tube be lengthened ½ meter the serrations of its flame will no longer coincide with those of the pipe, but will bisect the spaces between the latter; for an impulse from the resonator has now to traverse such an increased length that it arrives at its manometric flame $\frac{1}{684}$ of a second later than before the tube was lengthened. If the tube be lengthened one meter, or a whole wave-length (German), the displacement of the resonator serrations will amount to the entire distance separating the centers of two contiguous serrations; and on elongating the tube $n$ number of wave-lengths, $n$ number of such displacements will occur. Thus can be measured, not only *one*, but *many* wave-lengths, for I have not seen sensibly diminished the intensity of the pulses after they have traversed many meters of firm thick tubing. Therefore the error made in the determination of the distance occupied by many wave-lengths will not be greater than that occurring in the measure of the length of only one; and, consequently, this error being divided over so great a number will proportionally increase the accuracy of the deduced length of a single wave.

2. *The determination of the velocity.*—If the number of vibrations given by the pipe can be determined with an accuracy comporting with the above measure of its wave-length, we will succeed in arriving at very precise measures of the velocity ($\nu = n\,\lambda$) of sound in air and in various gases. To make the measure of a wave-length in a gas other than air, we close the mouth of the resonator with a delicate membrane, and fill the resonator, its capsule and connecting tube with the gas; or, we can substitute for the resonator a cavity of the proper volume and form, closed by a large membrance which vibrates in unison with the fundamental note of the pipe, and proceed as above.

It requires but little consideration to see that the determinations of the acoustic wave-length and velocity of sound in a gas, by the process I have described, greatly exceed in accuracy the results heretofore obtained by Dulong, Wertheim and others who deduced the length of the wave and velocity from measures of the internodal distances in organ pipes; and I have reason to be of the opinion that my method will give results exceeding in accuracy even those obtained by Quincke (Pogg. Ann., vol. cxxviii, p. 177), who used an instrument which embodies the principle invented by Herschel, and which has its highest development in the exquisite interference apparatus which König has recently described in Poggendorff's Annalen, Bd. cxlvi, p. 165.*

In my lecture-room I have hung up before the students a series of gum tubes having lengths of ½, 1, 1½, 2, 2½, 3, etc., wave-

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*** See the translation of König's paper in this No. of the Journal.**

lengths of different notes. The tubes, forming any one of these series, are used with the organ-pipe and resonator corresponding to their note; and as they are successively adapted to the resonator, they cause the serrations of its flame successively to coincide with and to bisect those of the organ pipe flame. Students after such exhibitions do not depart from the room with their usual skepticism as to the existence of an acoustic wave-length, but look upon the tubes as measurés of actual entities.

3. *The Acoustic Pyrometer.*—Having devised this simple arrangement of apparatus for measuring the number of wave-lengths contained in a given tube, the idea occurred to me that I could use the method in determining the variation in the number of wave-lengths contained in this tube, caused by a change in the temperature of the air which it contained; and thus succeed in readily determining any temperature to which the tube might be exposed.

The accuracy of this (as far as I know) entirely new method of pyrometry, and the facility of its application can be judged of by the following discussion.

The formula $V=\sqrt{\frac{gh\Delta}{d}(1+at)\frac{c'}{c}}$ gives the velocity of sound in air of a known temperature. This formula, as is well known, is reduced numerically to $V=333^{m}\sqrt{1+{\cdot}00367\,t}$. In which, V=the velocity of sound at the temperature $t$ centigrade; 333= the velocity of sound in meters, at 0° C.; and ·00367 the coefficient of expansion of air under a constant pressure. We will suppose that we have outside of the furnace, whose temperature we would measure, an $UT_4$ organ pipe; that we have placed opposite its mouth an $UT_4$ resonator; and that tubes from pipe and resonator lead to contiguous gas jets placed before the revolving mirror. We will also assume that the air in and around the organ pipe is at 0° C., and that the serrations of the flames of pipe and resonator are brought to coincide when 13 meters of metal tube, connecting the resonator with its manometric capsule, are placed in a furnace which also has the temperature of 0° C. Therefore the length of a wave in the furnace tube is $\frac{333}{512}=0^{m}{\cdot}65$, and it will contain 20 wave-lengths. Now gradually raise the temperature of the furnace to 820° C. As the temperature rises we will see the serrations of the resonator flame gradually slide over those of the organ pipe flame, and when the temperature has reached 820° C. we will have observed that the serrations of the resonator flame have glided over 10 times the distance separating the centers of two contiguous serrations of the flame of the organ pipe; for at

820° C. the air in the furnace tube will have expanded to 4 times its volume at 0° C., and therefore $\left(\lambda=\frac{333\sqrt{1+\cdot00367\times820}}{512}\right).$ it will contain $\frac{1}{2}$ the number of wave-lengths it did when at 0° C., and the length of one of these waves in the tube will be 1·3 meter.

We will now determine the limit of accuracy of the method by elevating the temperature of the furnace 100°, or to 920° C. At this temperature the velocity of the pulses in the furnace tube will equal 696·63 meters, and the length of the wave at this velocity will be $1^{m}\cdot36$. But $1^{m}\cdot36-1^{m}\cdot3=0^{m}\cdot06$, the difference in wave-length produced by the increase in temperature from 820° to 920°, and sufficient to cause the serrations to be displaced ·46 of the distance separating the centers of two contiguous serrations of the organ pipe flame. But by means of the manometric flame micrometer $\frac{1}{10}$th of this displacement can be measured, therefore, we can measure an increase of 10° C. in temperature above 820°.

From an examination of the well established formula for the determination of the velocity of sound, it will be seen that the accuracy of our determinations of furnace temperatures will alone depend on the precision of the coefficient ·00367, which is the number arrived at by Regnault and Magnus for the expansion of air under a constant pressure; and this is one of most reliable constants we have in Physics. Hence the accuracy of our measures, to 10° C., will be equal to those of the air-thermometer, whose indications are, at present, necessarily received as our standards of thermometric determinations.

We will now examine the relation existing between temperatures and wave-lengths. I have computed two tables; the first gives the velocities of sound and the wave-lengths of the note $UT_4$, corresponding to temperatures between 0° C. and 2,000° C.; the second those corresponding to temperatures between 0° C. and −272·48° C.

| Temperature. | Velocity. | Wave-length. |
|---|---|---|
| 0° C. | $333^{m}$ | $0^{m}\cdot650$ |
| 100 | 389·34 | ·760 |
| 200 | 438·53 | ·856 |
| 300 | 482·72 | ·942 |
| 400 | 523·14 | 1·021 |
| 500 | 560·74 | 1·095 |
| 600 | 596·03 | 1·164 |
| 700 | 629·04 | 1·228 |
| 800 | 660·67 | 1·290 |
| 900 | 690·77 | 1·349 |
| 1000 | 719·64 | 1·405 |
| 1100 | 747·38 | 1·458 |
| 1200 | 774·19 | 1·512 |
| 1300 | 799·96 | 1·562 |
| 1400 | 824·94 | 1·611 |
| 1500 | 849·35 | 1·658 |
| 1600 | 872·96 | 1·705 |
| 1700 | 895·97 | 1·748 |
| 1800 | 918·41 | 1·793 |
| 1900 | 940·26 | 1·836 |
| 2000 | 961·70 | 1·878 |

| Temperature. | Velocity. | Wave-length. |
|---|---|---|
| 0° C. | 333· | ·650 |
| − 50 | 300·86 | ·587 |
| −100 | 265·00 | ·517 |
| −150 | 223·14 | ·435 |
| −200 | 171·79 | ·335 |
| −250 | 95·60 | ·186 |
| −272·48 | 00·00 | ·000 |

These related numbers I have projected into the accompanying curve, whose abscissas are the temperatures, and whose ordinates are the wave-lengths. This curve, which is the graphical expression of $y=\dfrac{333\sqrt{1+{\cdot}00367x}}{512}$ is evidently a parabola, since it has the form $y^2=ax$; and $y$ will equal 0 when $x$ has receded to the point on the axis of abscissas equal to $-272{\cdot}48°$ C., which is "the absolute zero" of temperature.

It is evident that this same curve will give the numerical relations between temperatures and the wave-lengths of any note, or the velocities of sound in any gas, by merely giving different numerical values to the divisions on the axis of ordinates.

It only remains to give the simplest formula for determining the temperature of the furnace in terms of the observed displacements of the resonator serrations, and of the known number of wave-lengths in the furnace-tube at the temperature $t$.

Let $t=$ temp. C. of the air in and around the organ-pipe.
$t'=$ " " " the furnace-tube.
$v=$ velocity of sound at temp. $t$.
$v'=$ " " " $t'$.
$l=$ number of wave-lengths in furnace-tube at temp. $t$.
$d=$ observed displacement of resonator serrations by an elevation of temperature $t'-t$.

Then $l-d$ will equal the number of wave-lengths in the furnace-tube (allowance made for elongation of tube by heat) at the temp. $t'$. As the velocity of sound in the furnace-tube will be inversely as the number of wave-lengths it contains, it follows that

$$v':v::l:l-d;\ \text{hence}\ v'=\frac{v\,l}{l-d};\quad \text{but}$$

$$(1)\qquad v=333\sqrt{1+{\cdot}00367t},\quad \text{and}$$

$$(2)\qquad v'=333\sqrt{1+{\cdot}00367t'},\quad \text{hence}$$

$$(3)\qquad \frac{v\,l}{l-d}=333\sqrt{1+{\cdot}00367t'}$$

Reducing equation (3) we obtain

$$(4)\qquad t'=\left(\frac{v\,l}{20{\cdot}16\,(l-d)}\right)^2-272{\cdot}48,$$

which gives $t'$ in terms of $v$, $l$ and $d$. Combining equations (1) and (3) we obtain

$$(5)\qquad t'=\frac{272{\cdot}48\,(2l-d)\,d+tl^2}{(l-d)^2},$$

which gives $t'$ in terms of $l$, $d$ and $t$. But as $v$ has to be calculated in order to obtain $l$ in equation (5), it follows that equation (4) is the simpler and the more readily worked numerically.

If we call T *the absolute temperature* centigrade, then $T=t'+272{\cdot}48$, and equation (4) becomes (6) $T=\left(\frac{v\,l}{20{\cdot}16\,(l-d)}\right)^2$, in which equation the origin of coördinates is at the vertex of the parabola.

This paper is intended only to give a general account of the new method of pyrometry, and therefore I have not touched on the details of apparatus and experiment. These I will present in a subsequent communication, in which accounts of actual applications of the method will be given. I may, however, here remark that, if the tube in the furnace is 13 meters long and of $0^{m}{\cdot}015$ diameter, it can be coiled into a spiral of $0^{m}{\cdot}5$ in diameter, or into two spirals, each of $0^{m}{\cdot}25$ diameter. Or, the tube can be bent on itself several times and thus form a compact fascicle of tubing. Also, the tubing should *ascend* from the resonator to the furnace-tube, and *descend* from the latter to the manometric capsule, so that the rarefied air in the hot tube cannot enter the tubing outside of the furnace. If the serrations of the manometric flames are too dim to be readily observed, they can be rendered distinctly visible, even in broad day-light, by the use of "carbonized" gas, or by sifting into them the fine scrapings of lead pencil. In ascertaining the number of displacements produced by any temperature, the furnace-tube is slowly moved into the furnace, so that the displacements of the resonator serrations can be counted as the tube gradually attains the highest temperature, when the serrations become stationary.

**October 12, 1872.**

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## ART. L.—*On the stability of the Collodion Film;* by LEWIS M. RUTHERFURD.

THE very numerous and concordant micrometric measures of star photographs, made by myself and under my direction during the past seven years, have inspired me with the utmost confidence in the stability of the collodion film, particularly when applied to a plate of glass properly albumenized.

The measures above referred to were made upon nearly thirty plates of the Pleiades group taken at many different times, and embracing within a space of about 80′ square from thirty to seventy-five star images, according to the time of exposure, the state of the atmosphere and the sensibility of the chemicals; also upon many plates of the Præsepe group, averaging about thirty-four stars on each plate; also upon many plates of the group about $\theta$ Orionis; also upon many plates of the group surrounding 41 Boötis; also of the group surrounding $\mu$ Cassiopæa.

In addition to the measures of the position and distance of the members of the several groups mentioned, very many measures have been made upon long lines of star images, at intervals of one second of time from each other, for the purpose of determining the angular value of any given portion of the plates. Very many measures have been made for the same purpose upon selected star pairs, and the results of these measures compared with the results of transits of stars over diamond lines drawn upon a plate of glass placed in the photographic plate-holder at the photographic focus,—and finally many measures have been made upon star groups taken on the preceding, central and following regions of the plates for the purpose of detecting the character and amount of the distortion, if any, of the images.

The results of all these measures were so concordant as to forbid the idea of the existence of any great change in the collodion film, and the conviction of this stability is very greatly strengthened by a very few attempts to remove the collodion film from an albumenized plate by any ordinary mode of rubbing or washing.

This confidence in the reliability of the photographic method for measures of precision was rudely shocked by the statements of Mr. Paschen, contained in an elaborate article upon the application of photography to the observation of the coming transit of Venus, printed in the Astronomische Nachrichten in April last. He found in three measures upon albumenized plates and upon one not albumenized respectively, a shrinkage to the following fractions of the whole space measured: in No. 1, $\frac{1}{523}$, No. 2, $\frac{1}{612}$, No. 3, $\frac{1}{1856}$, No. 4, $\frac{1}{2123}$. Nos. 1 and 2 were measures of the same plate, but in directions perpendicular to each other. No. 4 was on a plate not albumenized: these quantities are so large and irregular, that if they really exist, we must be compelled to relinquish the hopes based upon the use of photography for precise astronomy. My own experience and a distrust of the methods used by Mr. Paschen for obtaining these results, prevented me from losing confidence in the value of a photographic image. Still the question was so grave and the general merits of Mr. Paschen's article were so great, that I determined to subject the point to a thorough examination, and for that purpose the following measures were made upon plates, albumenized before the application of the collodion, first when leaving the camera and quite wet, and second when they had become dry; some of the plates were negatives of dark lines upon a white ground, and some of white lines upon a dark ground; in some while yet wet most of the collodion between the lines was removed to encourage a shrinkage outward, and in others most of the collodion outside of the lines was removed to encourage a shrinkage inward; most of the collodion used was such as is in ordinary use in my observatory. The measures were made by the aid of my micrometer screw, one revolution of which is equal to $\frac{1}{48}$ of an inch. The plates were made and the measures conducted by Mr. Chapman, my assistant, who is an accomplished photographer and familiar with the use of the micrometer. The plates were left clamped upon the stage of the micrometer during the interval between the measures wet and dry, and both these measures were consequently made on the same parts of the screw. In every case five bisections of each of the two lines were made, and the difference between the means of these readings shows the distance between the lines in terms of screw revolutions. For the purpose of exhibiting the degree of precision attainable by the measures, I will transcribe in full the measures of the first two plates, giving for the rest only the results.

The values of the measured spaces have been reduced to a uniform temperature of the screw of 68° Fahrenheit by the application of a correction deduced from the recognized coefficients of expansion of steel and glass. It will be seen that in all cases save two the distance was greater between the lines when the plate was dry than when wet, the mean excess of the nine measures is Rev. 0·0017, which is $\frac{1}{28,236}$ of an inch; it reaches in no case $\frac{1}{4800}$ of an inch. This result is no doubt due to the cooling of the glass plate, by the evaporation which takes place the moment the wet plate is taken from the plate-holder and exposed to the air under the micrometer. This excess of distance (Rev. 0·0017), would be caused by an increase of temperature for the dry glass of about 4° F.

This consideration reveals a source of error in the use of wet plates which I have not hitherto considered, since the same evaporation takes place no doubt during the long exposures given to star-plates; the amount will vary according to the hygrometric state of the atmosphere, and may be met by reading wet and dry bulb thermometers.

My objection to the method used by Mr. Paschen, as I understand it, is that instead of being confined to an investigation of what happens to the collodion film between the moment of exposure wet and the moment of measurement when dry, it is a comparison of the actual state of the plate when dry, what it ought to have been had all the adjustments, manipulations and instruments been perfect.

PLATE NO. 1.

June 27, 1870. Therm. 74°.

*Wet.*

| Line No. 1. | Line No. 2. |
|---|---|
| 165·5500 | 72·1175 |
| ·5470 | ·1125 |
| ·5525 | ·1225 |
| ·5575 | ·1275 |
| ·5540 | ·1240 |
| 165·5522 | 72·1208 |
| 72·1208 | |
| 93·4314 | |
| +10 Therm. Cor. | |
| 93·4324 | |
| *Dry.* | Therm. 72°. |
| 165·1125 | 71·6850 |
| ·1150 | ·6880 |
| ·1175 | ·6890 |
| ·1175 | ·6850 |
| ·1125 | ·6860 |
| 165·1150 | 71·6866 |
| 71·6866 | |
| 93·4284 | |
| +7 Therm. Cor. | |
| 93·4291 | Dry−R.0·0033 |

PLATE NO. 2.

June 27, 1870. Therm. 74°.

*Wet.*

| Line No. 1. | Line No. 2. |
|---|---|
| 164·3275 | 71·9350 |
| ·3325 | ·9450 |
| ·3250 | ·9300 |
| ·3350 | ·9370 |
| ·3275 | ·9475 |
| 164·3295 | 71·9388 |
| 71·9388 | |
| 92·3907 | |
| +10 Therm. Cor. | |
| 92·3917 | |
| *Dry.* | Therm. 72°. |
| 163·9075 | 71·5100 |
| ·9000 | 50 |
| ·9100 | 00 |
| ·9025 | 50 |
| ·9025 | 50 |
| 163·9045 | 71·5130 |
| 71·5130 | |
| 92·3915 | |
| +7 Therm. Cor. | |
| 92·3922 | Dry+R.0·0005 |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Plate | No. 1. | June | 27. | Therm. | 74° and 72° | Dry−R. | 0·0033 |
| " | No. 2. | " | " | " | 74° and 72° | " +R. | 0·0005 |
| " | No. 3. | July | 3. | " | 82° and 83° | " +R. | 0·0004 |
| " | No. 4. | " | " | " | 84° and 85° | " +R. | 0·0007 |
| " | No. 5. | " | 8. | " | 77° and 78° | " +R. | 0·0009 |
| " | No. 6. | " | " | " | 82° and 80° | " +R. | 0·0092 |
| " | No. 7. | " | 10. | " | 78° and 80° | " −R. | 0·0059 |
| " | No. 8. | " | " | " | 81° and 83° | " +R. | 0·0040 |
| " | No. 9. | " | " | " | 84° and 82° | " +R. | 0·0089 |

Mean excess of Dry, R. 0·0017 $=\frac{1}{28,236}$ of an inch.

## ART. LI.—*Note upon Aventurine Orthoclase, found at the Ogden Mine, Sparta Township, Sussex Co., N. J.*; by Prof. LEEDS.

AMONG the masses of gneiss rock thrown out in sinking one of the shafts of the Ogden mine, I found, during the course of the past summer, large quantities of very beautiful sunstone, which appears hitherto to have escaped notice. The three cleavages *O*, *i-ī* and *i-ĭ*, are easily obtained, and afford the cleavage angles of orthoclase. The very thin plates, which may be procured by slicing the stone in the direction of the principal cleavage, are of considerable size, and furnish excellent specimens for microscopic examination.

The color of the orthoclase is a delicate flesh-red, which color is due entirely to the imbedded crystalline scales of what has been supposed to be göthite. The stone itself is translucent and quite colorless. The results obtained in two analyses were:

| | 1. | 2. | Mean. |
|---|---|---|---|
| Silica, | 64·80 | 64·82 | 64·81 |
| Alumina, | 19·02 | 19·25 | 19·02 |
| Ferric oxide, | 0·23 | | 0·23 |
| Lime, | 1·29 | 1·23 | 1·26 |
| Magnesia, | 0·61 | 0·58 | 0·59 |
| Potash, | 15·22 | 13·38 | 14.30 |
| Ign., | 0·26 | 0·26 | 0·26 |
| | | | 100·47 |

In an analysis of an aventurine oligoclase from Tvedestrand in Norway, Scheerer obtained $SiO_2$ 61·30, $Al_2O_3$ 23·77, $Fe_2O_3$ 0·36, CaO 4·78, $Na_2O$ 8·50, $K_2O$ 1·29. In this the per cent of göthite is somewhat greater than in the New Jersey orthoclase, but in both cases the extremely small amount of foreign matter which suffices to impart the brilliant aventurine character to the feldspar is remarkable. It is worth noting in this connection that all the specimens of sunstone from Kennett, Chester Co., Pa., in the cabinet of the Stevens Institute, are oligoclase, not orthoclase.

## ART. LII.—*On Soil Analyses and their Utility*; by EUG. W. HILGARD, State Geologist of Mississippi.

(Read at the Dubuque Meeting of the Am. Assoc. Adv. Sci., August, 1872.)

IN the American Journal of Science for September, 1861, Prof. S. W. Johnson published a criticism on the "Soil-analyses of the Geological Surveys of Kentucky and Arkansas," whose strictures, to a great extent eminently just, appear to have so impressed the scientific public in this country, that few if any soil-analyses have since then been made in connection with any state or national survey, excepting that of the State of Mississippi, where the work already begun was continued, either by myself, or under my charge, or recommendation, by others. Holding myself responsible for this departure from the generally adopted views, I propose in the present paper to discuss specially Prof. Johnson's objections, and to give my reasons for persisting in a course of research that has, more than once, secured for myself and my co-laborers the compassionate sympathy of true believers. While I consider the work far from being as complete as it should be, and for that as well as other reasons its publication in detail may be delayed for some time; yet I think what can now be said of sufficient importance to be brought before this meeting.

I propose, in this discussion, to maintain the mainly practical standpoint assumed by Prof. Johnson himself. I shall therefore leave out of consideration the performance of such exhaustive investigations of *all* the physical and chemical properties of the soil, as have been made in some cases, for special purposes, e. g., by Prof. Mallet, on some of the cotton soils of Alabama. If the investigation of each soil, to possess practical importance, requires from three to six months labor, we may as well, for practical purposes, consider such researches out of the question for the present. We want something analogous to the metallurgical assay of minerals, as distinguished from their complete ultimate analysis. So far, therefore, as the agricultural qualities of a soil may be inferred and approximately estimated by an experienced eye, I would relieve the chemist from the exact numerical determination, e. g., of the power of absorbing heat from the sun, the specific heat, the "water-holding" power, the capillary coefficients, etc. However necessary for theoretical investigations, I hold that for practical purposes, these laborious determinations may in most cases be dispensed with; since from what has already been done, or what can be done with a few typical soils, we may infer the comparative magnitude of these coefficients with a sufficient degree of approximation.

The amount of labor bestowed on each soil by Dr. Peter, as reported in the Kentucky and Arkansas surveys, approaches very closely the limit beyond which the *immediate* advantages to be derived from such knowledge of soils as analysis may impart, would seem, to many, disproportioned to the expenditure involved. How very modest we are, truly, when a purely scientific object is involved, whose immediate practical application is not obvious at a glance! In what other branch of technical science would it be thought admissible to proceed without obtaining such knowledge of the prime materials as chemistry may afford, even if *no* immediate application of this knowledge be foreseen? Our public treasuries are constantly drawn upon for hundreds of thousands of dollars, in behalf of objects of at least questionable usefulness. Yet Prof. Johnson seems to have thoroughly satisfied our state geologists that they are not justified in giving the virgin soils of their respective States the benefit of such light as chemistry may even now confessedly afford; apart from the important general inferences which may fairly be expected to be drawn hereafter from the history of their cultivation. How are we to advance in our knowledge of soils, if we abandon as hopeless the determination of their chemical character? Are the proofs that have been brought against the utility of soil analyses really of such a character as to justify so grave an omission? an omission, too, which in many cases cannot hereafter be supplied. Even in the comparatively youthful State of Mississippi, I have found difficulty in obtaining reliable specimens of some soils, whose great productiveness had led to their cultivation by the earliest settlers, over the entire area of their occurrence.

I question the propriety of this omission, and the justice of the *testimonium paupertatis* thus inflicted upon agricultural and analytical chemistry.

To define my position, I premise that—

1. I fully agree with Prof. Johnson as to the comparative uselessness of a single analysis giving the percentages of soil ingredients found, *in ordinary cases.* It is only when such analysis demonstrates the *great abundance*, or *very great deficiency*, of one or several primarily important ingredients, that, by itself, it conveys information of considerable practical importance. Note, that such cases are not altogether infrequent, even in virgin soils.

2. I agree that an "average soil" is a *non ens*, except as referred, comparatively, to a *particular set of soils closely related in their origin.*

3. Also, that the claim of being able to detect the minute differences caused by cropping without return to the soil, is precarious, and perhaps beyond the power of our present analytical resources.

4. I further admit that, ordinarily, the analysis of soils long cultivated, and treated with manures, can give but little and very partial information as to the condition and composition of the soil; from the great difficulty, if not impossibility, of obtaining fair representative specimens.

5. Furthermore, that to designate soils by the names of the Cretaceous, Carboniferous or Silurian strata they may happen to overlie, is very loose practice; since in most cases they are derived from Quaternary deposits, which may or may not have been influenced in their composition by the subjacent rocks.

On the contrary I demur, in the first place, to the broad assertion that "it is practically impossible to obtain average specimens of the soil," as inapplicable to a very large class, especially of virgin soils, covering large areas with a uniformity of character corresponding to that of subjacent formations, from which they have been directly derived, by substantially identical and uniform, or uniformly variable, processes.

The importance of this exception is not, it is true, very obvious in the stony fields of New England (such as discouraged

Prof. Johnson in his vacation trip to Northern New York), or in fact, in any district where a great variety of formations has directly contributed toward forming the soil, and "chunks" of undecomposed minerals are diffused through it. In such cases, the analysis of the rock which has predominantly contributed to the mass of the soil proper, would be a more correct index of the prevalent characteristics of the latter, than if itself were taken in hand. And from such analyses we could at least deduce what ingredients, and in what form, it would certainly be *useless* to add to the soil.

But when we come to the great plains of the West and Southwest, whose soils are consistently derived from widespread Quaternary deposits, composed of materials almost impalpable save as regards siliceous sand; or even the rolling uplands of the Gulf States, whose subsoil stratum of "yellow loam" can only be diluted, but not otherwise changed, by the admixture of the underlying drift, leached long ago of everything soluble in carbonated water, or available to plants: the objection based upon the supposed impossibility of securing representative specimens, becomes obviously untenable; as I shall hereafter show from the close correspondence in the composition of soils, and especially *sub*soils, from widely distant portions of the State, derived from the same geological (Quaternary) stratum.

A word in regard to the "freaks and accidents" mentioned by Prof. Johnson as liable to make sport of the devoted analyst. Undoubtedly such errors must be ultimately provided against by multiplication of analyses (not necessarily of the same acre, but of other corresponding specimens, in the sense mentioned above); and while questioning the efficiency of a bird or squirrel in vitiating a properly taken sample of soil, I must admit the disastrous consequences which might result if a dog, cow, or horse were similarly concerned. No specimen of "virgin soil" can, of course, be obtained where such animals usually do congregate. But as a rule, it is not at all difficult to avoid such places; while the chance of accidentally hitting upon a sporadic animal deposit in the broad woods or prairies is singularly small, and is notably diminished by the circumstance, that an attentive observer (and none other should take soil specimens) will be able to distinguish such localities for years, by the peculiarity of their vegetation.

I will remark, however, that I consider the sampling of a soil with a view to securing a representative specimen, as a matter second in difficulty and delicacy only to the analysis itself; that I rarely have thought it worth while to analyze specimens sent by other than intelligent persons specially instructed by me; and even then have frequently had to reject them, from their having obviously been taken at an improper locality, e. g., near a foot-path, by the side of a fence, on a partially denuded hillside or ravine, in the bed of a run, at the foot of a tree, etc.

The question of *depth* must, in my view, be left to be determined by the circumstances of each case, except in so far as the extreme depth to which tillage may cause the roots of crops to reach, must be within the limits of the samples taken. Of these, *one* should ordinarily represent what, under the usual practice of tillage, becomes the arable soil; another, the subsoil not usually broken into; a third will in most cases be useful to show what materials would be reached were the land to be underdrained. As a rule, I have taken no specimens of soil to a less depth than six inches, and as much deeper as uniformity of color reached—for obvious reasons. But in special cases, when important differences were suggested by the aspect of the soil and subsoil, they have been separately examined, at whatever depth the change of color might occur.

With soils of the character referred to, samples selected and taken with due care, and strict attention to thorough intermixture, both in the field and subsequently in the laboratory, I am unable to see why even two grammes may not correctly represent the characteristics of a 1000 acre tract. Not that every point of that tract would be likely to give the same percentage result, perhaps; especially as regards the surface soil, which might in places be more clayey or more sandy than the sample analyzed. Still, the *relative proportions of the soil ingredients, and their degree of availability*, would remain substantially the same; the wider range and readier penetration of roots in sandier soils, making up, within certain limits, for the smaller percentage of available ingredients in a given bulk, as compared with more clayey ones.

From the fact that the atmospheric surface water must, in its course, inevitably have a tendency to bring about such inequalities, by carrying forward the finer particles of the soil in larger proportion than the coarser ones; as well as from the greater influence of vegetation: we shall, in the series of analyses made a postulate by Prof. Johnson, expect to find a closer agreement between those of subsoils than those of surface soils. Such I find to be very decidedly the case; so much so, that I habitually look to the former as the most reliable index of a soil's distinctive character. To this there can be no legitimate objection, when, as in all the upland soils now under consideration, the surface soil is directly derived from the subsoil, and its depth is less than thorough culture would give to the arable soil.

As regards the analysis itself, I premise that I have always found even the most "chemically pure" reagents sold by dealers quite inapplicable to the purpose of soil analysis. From first to last, I have prepared or purified these myself; and, as regards the acids, especially hydrochloric, I have found it necessary to reject, as a rule, even the purest, after keeping it for a few weeks in a glass bottle. The same is true, and perhaps in an aggravated degree, of aqua ammoniæ. The severe ordeal of slow evaporation on a bright platinum foil will rarely be passed by ammonia a fortnight old; and still less frequently by hydro-sulphide of ammonium.

Armed with these, and a multitude of other precautions, usual and unusual, to secure the utmost possible accuracy; always treating the soil with the same large excess of acid of uniform strength, and precipitating all corresponding precipitates as much as possible from the same volume of liquid; using none but the best Bohemian glass, and platinum vessels, and filters specially extracted—operating, in short, as uniformly as the nature of the materials would permit: I confess I felt considerable confidence in the correctness of my results, until the experiments made in Bunsen's laboratory, on the solubility of glass vessels, gave rise to unpleasant doubts, On consideration, however, I found that the (sensibly constant) error so introduced would not, when allowed for, amount to more than the differences between two analyses of one and the same material, or vitiate in any serious degree the conclusions arrived at. Nevertheless, I shall hereafter, to the utmost possible extent, carry on all operations liable to introduce errors on this score, in platinum and porcelain vessels, as advised by Bunsen.

As regards Dr. Peter's failure to determine the amounts of soluble silex, nitric acid, ammonia, chlorine, and the degree of oxidation of the iron, I agree that the former is desirable, not only because, whether "essential" or not, some plants do habitually absorb it in very large quantities, and it might be best to let them have it; but also because it is a desirable index of the degree of decomposition which the soil silicates have undergone. I have therefore made this determination regularly, by boiling with solution of sodium carbonate. In a series of these determinations, an unmistakable relation between the soluble silex and the amount of lime in the soil becomes manifest; as might, indeed, have been foreseen.

As regards nitric acid, the consideration suggested by Prof. Johnson himself, viz., that its quantity must be exceedingly variable, within short periods, in one and the same soil, seems to me a sufficient dispensation from the laborious determination.

The same holds good, in a measure, for ammonia. Its quantity varies continually in the soil, as it does in the atmosphere; its chief absorbers in the soil are "humus" and clay. Where these prevail largely, ammonia can scarcely be deficient as a nutritive ingredient to an injurious extent; albeit, more might doubtless be beneficially added. Moreover, the characteristic effects of ammonia on vegetation are sufficiently obvious (in "running to weed") to render its determination in virgin soils, laborious and even uncertain as it is, a matter of comparatively little *practical* consequence, however great might be its theoretical interest.

As for the determination of the degree of oxidation of iron, I confess I fail to see its practical bearing. When ferric oxide is present, plants surely can have no difficulty in reducing the modicum they need to a soluble condition. When ferrous oxide exists to any great extent, it indicates a want of drainage, and manifests itself both in the color of the soil and in the poisonous effect on vegetation. But farmers surely do not need the aid of chemical analysis to tell them that their soil needs drainage and aëration! A determination made to-day would be of no value to-morrow, if the soil had been plowed in the interval.

Finally, Dr. Peter *does* determine *chlorine*, in the treatment of soils with carbonated water; though it is not put down in the general analysis. However, the soluble chlorides, like the nitrates, are so constantly liable to variation and, as experience shows, so little likely to be deficient in the soil, that its omission would not be a serious practical objection.

A much graver defect is the failure to determine separately the organic matter ("humus") and the chemically combined water; and to this is owing, in a measure, the unsatisfactoriness of the analyses as regards information on the physical character of the soils. A large amount of water of hydration indicates, in ordinary cases, a correspondingly clayey soil, where heaviness in working may, or may not, be relieved by a large amount of "humus." The "volatile matter" item, however, gives us no information whatsoever on these vitally important points; and there is, unfortunately, no *simple* method by which the determinations in question can be effected even approximately. That they *should* form part of every soil analysis, is obvious, if only on account of the importance of "humus."

I have attempted to obtain a reliable scale of the different degrees of "heaviness" of soils, from the determination of their maximum absorption of hygroscopic moisture at ordinary temperatures. I find that at temperatures from about +7° to +21°, the amount of aqueous vapor absorbed by a thin layer of soil exposed to a *saturated* atmosphere remains very nearly constant, being for

| | | |
|---|---|---|
| Very sandy soils, | 1·5 to 2·0 | per cent. |
| Loam soils, | 5·0 to 8·5 | " |
| Clay soils, very heavy, | 12·0 to 15·0 | " |

there being, of course, all intermediate grades of hygroscopic power, as well as of "heaviness." It appears that for this interval of temperature, the decrease of absolute absorbing power in the soil, resulting from the rise of temperature, is just balanced by the increased amount of vapor diffused in the air—not an unimportant circumstance, with regard to vegetable life.

There are, however, two soil ingredients which interfere seriously with the correctness of the estimate as to "heaviness," derived from the coefficient of absorption, viz., "*humus*" and *ferric oxide*. Both of these are highly hygroscopic, yet both *counteract* the "heaviness" caused by excess of clay. Moreover, there is a class of soils (viz., fine siliceous silts) whose exceeding "heaviness" in cultivation is much complained of, yet whose absorbent power is very small.

When, as in the majority of cases, the surface soil has been directly derived from the subsoil, the disturbing effect of the "humus" may be sensibly eliminated by comparing, not the soils, but the subsoils, in this respect.* As to the ferric oxide, there are among about 200 Mississippi soils analysed but three or four whose agricultural qualities would have been seriously under-estimated by a reliance upon the coefficient of absorption alone.

But I do not for a moment admit, that in a material so complex both in its composition and mode of action any one or few data, whether chemical, physical, or agricultural, may be relied upon to characterize the soil: or, as Prof. Johnson expresses it, "to do violence to agriculture." So far from this, I consider that a proper interpretation of the analytical results must take into consideration, not only all the chemical and physical facts observed on the specimen, but all that has been or can be observed *in loco*—the location, depth, derivation, relations to drainage, etc.; as well as all that is known concerning the qualities or peculiarities of the soil, both in its natural state and in cultivation. As Prof. Johnson says, it should "form part of a system of observations and trials; must be a step in some research; must stand, not as an index to a barren fact, but as the revelator of fruitful ideas."

Such, precisely, has been my object from the beginning of my researches on the soils of Mississippi, for sixteen years past. Clearly, the difference between Prof. Johnson's position and mine is one of degree only; yet this difference is not a slight

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* In such cases, the surface soil is always more sandy than the subsoil.

one, since while, as before remarked, I have made, or caused to be made, some 200 analyses of soils and subsoils, his classic works on the growth and nutrition of plants do not contain so much as a tabular exemplification of the composition of various soils, as resulting from chemical analysis. If, then, "the probabilities of its uselessness in direct application to practice are so great," as Prof. Johnson seems to hold, I have committed a grievous error, and squandered the substance of the State.

I think that the considerations already adduced should plead measurably in extenuation of my course. But I will now state succinctly what services, in my view, soil analyses may fairly claim to be capable of performing, when conducted substantially in the manner, to the extent, and under the conditions defined above.

I take it for granted that, if in the determination of the mineral ingredients we were able to distinguish clearly from one another the portion immediately available to plants, from that which is in an unavailable form, we would go far toward accomplishing what was originally claimed for soil analysis; and this Dr. Peter attempted to do by treatment of the soils with carbonated water. It cannot be doubted, however, that plants, as well as agriculturists, have at their disposal much more powerful, or at least more *energetic*, solvents; and that, therefore, a determination of those ingredients which may fairly be considered practically within the reach of agriculture, must go deeper than does that with carbonated water.

Opinions may differ widely as to the proper strength and nature of the solvent ("*Aufschliessungsmittel*") to be selected. Hydrofluoric acid, or ignition with the alkaline earths, would evidently go too far; as no soil, probably, will ever yield up the whole of its nutritive ingredients to plants, and fertility is far from being proportional to the *whole* amount of potash, phosphoric acid, etc., contained therein.

When, however, a *partial* solvent of uniform strength is used in all cases alike, and its action continued for the same length of time, it may fairly be presumed that, *as between soils of similar origin*, the amounts so rendered soluble are, in a measure, proportional to the amounts of available nutriment present.

In using hydrochloric acid of the strength 1·11 to 1·12 sp. g., obtained by slow steam distillation of stronger or weaker acid, rejecting the first and last portions, I have in most cases found quite a satisfactory agreement between the results so obtained and the experience of cultivators as to the productiveness and duration of the respective soils; always *provided*, that the difference in the amounts of inert sand present, of specific gravity, of depth of soil, etc., were taken into account.

The proviso is important; but that with a proper local knowledge these allowances *can* be made, and that in most cases the information thus gained regarding the nature and treatment of the soil will be vastly more complete and reliable than the judgment of any number of "old intelligent farmers," my experience has fully convinced me; witness the egregious mistakes daily made by such in the selection of new lands. Moreover, a small minority only of farmers is likely to possess the requisite "age and intelligence"; and it is quite important that the multitude of those less fortunate should have the benefit of all the help science can give them.

I will adduce but one "odious example" of a widely prevalent error in reference to the character of a class of soils, that I have as yet been unable to eradicate, even from among the "old and intelligent;" who are unfortunately very much given to theorizing on inadequate premises. Our prairie soils are notoriously limy; they are also very "sticky"; and the mud takes the hair off the feet of cattle. *Ergo*, every "sticky" clay soil in the State is called, considered, and treated as a "prairie" soil, especially if the hardened clods adhering above the hoofs of cattle should carry the hair with them. If such soil is unthrifty, and rusts cotton, it is because "there is too much lime in it," which "scalds" the seedlings. In matter of fact, most of these soils are notably deficient in lime, so as to be most directly and immediately benefited by its application wherever it has been tried, in accordance with my suggestion. The lime here acts, probably, as much chemically as physically; the clay being rich in potash, as per analysis.* While the physical defects of these soils are doubtless the main cause of the crop failures, yet analysis has suggested a remedy which relieves, for the time being, from the necessity of the more costly improvements; lime being comparatively easy of access.

Analogous cases are far from infrequent, both in this and in the adjoining States; and I have been led to attach special importance to the determination of *lime* in soils, from the (not unexpected) rule which seems to hold good very generally, viz., that, *cœteris paribus*, the *thriftiness* of a soil is sensibly dependent upon the amount of lime it contains; while, at the same time, in the usual mode of culture without return to the soil, the *duration* of fertility is correspondingly diminished, and its cessation is very abrupt wherever much lime is present.

It may be said that, after all, this is but what, from data already known, might have been expected. Granted; then, *a fortiori*, soil analysis, involving the determination of lime,

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* See, for example, the article "Heavy Flatwoods Soil," in my Miss. Rep., 1860, pp. 276, 279.

is of considerable use in determining the present and future value of soils.

In speaking of the "amount" of lime, I must be understood to refer, not so much to its absolute percentage, as to its quantity in comparison with that of potash, which, with phosphoric acid, is what all our fertilizers chiefly aim to supply. Their determination must, of course, be considered of prime importance, since their absence or extreme scarcity is fatal to profitable fertility; while, when they are present, even though *immediately* available for absorption to a slight extent only, we possess in lime, ammonia, etc., and the fallow, ready and powerful means for correcting their chemical condition.

Here again, the practical value of soil analysis is direct and indisputable. It is of no small interest to know whether the soil we intend to cultivate contains 0·75 per cent of potash and 0·25 of phosphoric acid, soluble in H Cl, or only the fifth or tenth part of these amounts. *One* will bear improvement of all kinds—will pay for underdraining, terracing, etc.; while the other, quite similar in aspect perhaps, would not, according to Liebig's testimony, ordinarily be capable of profitable culture.

Again, it is well known that the same species of plants may occupy soils of widely different quality and value. True, an attentive observer will in such cases see differences in the mode of development;* yet these are often such as to escape ordinary remark, and grievous disappointments frequently arise from this source, with new settlers especially. It is of no small importance to be able to *identify*, as well as to distinguish, soils resembling each other; and this, soil analysis can undoubtedly do, if there is any virtue in the law of probabilities even—admitting all that may otherwise be said against their reliability.

Even if no other direct benefits than those already mentioned could be attained by the chemical and mechanical analysis of soils (which I do not admit, and expect to prove otherwise hereafter); even if we leave out of consideration the addition to our general knowledge which may fairly be expected to result from extensive series of such investigations, carried out upon a uniform plan, whereby accidental errors (whether caused by "birds or squirrels," or analytical and other mistakes) will be eliminated; even thus, I contend that the practical and theoretical value of soil analyses is sufficiently great to justify whatever labor and expenditure may be bestowed upon them by state and national surveys; and that the neglect with which this branch of research has

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* Miss. Rep., 1860, p. 203.

of late been customarily treated, is the more to be regretted as no probable amount of private effort can accomplish what must, of necessity, be done on an extended scale and with the *prestige*, voluntary assistance, and interest, not usually accorded to any but public enterprises. And with due deference to the author of the two volumes whose extraordinary merits no one appreciates more than myself, I call upon my colleagues in State surveys, especially in the West and South, to reconsider this subject before it is too late, and a legislative fiat declares their work to be "finished." It is true that the agricultural colleges must and will take up and continue, as far as possible, the investigation of the agricultural peculiarities of each State; but the special and local experience acquired by those conducting a field survey, as well as their opportunities for extensive and comparative observation, are unfortunately "not transferable," even to the finest quarto report. In order to attain their highest degree of usefulness, our agricultural colleges should teach, not merely general principles, together with a sufficiency of the handicraft of agriculture; but they should be enabled to point out to each student, with reference to his particular neighborhood, How Crops Grow, and How Crops Feed.

Univ. of Miss., July, 1872.

## ART. LIII.—*The Heat produced in the Body, and the effects of Exposure to Cold;* by JOHN C. DRAPER, M.D.

THE following results were obtained in an attempt to determine the quantity of heat passing off from the surface of the body, by finding how much it would elevate the temperature of a known mass of cool water during a given period of time.

The manner of experimenting was as follows:—Seven and a half cubic feet of cool water were drawn into a bath, and the temperature taken after careful mixing. The bath was then covered over for about four-fifths of its extent to prevent the action of currents of air, and at the close of an hour the temperature was again tested. The rise of ½ a degree represented the amount of heat absorbed from the air during one hour, and was deducted as a normal error from the results afterward obtained.

During the time occupied in determining the normal error of the bath (viz., one hour), I lay on a sofa to bring the circulatory and respiratory functions into a condition similar, as regards position of the body, to that to which they would be submitted while in the bath. My dress during this phase of the experiment consisted of a thin flannel summer undershirt, linen drawers, and cotton socks. At the completion of the hour of rest these were removed with as little exertion as possible and I stepped into the bath, and lay down, allowing only the head to project above the surface of the fluid. At the close of an hour the temperature of the bath was again taken. I then left it, and drying the surface of the body, reassumed the same dress and lay down on the sofa. Throughout the whole of each experiment, the temperature of the air, the dew point, the temperature of the bath, of the armpit, mouth and temple were taken, together with the rate of respiration and of the pulse.

Since in these experiments two series of phenomena are investigated, I have for the sake of clearness of description separated the results in accordance with the phenomena in question, and direct attention first to the

*Quantity of heat evolved from the body.*

| | During Rest. | | During Motion. |
|---|---|---|---|
| | 1st Exper. | 2d Exper. | 3d Exper. |
| | July 4. | July 5. | July 11. |
| Temp. of air, | 90° F. | 84° F. | 83° F. |
| Wet bulb thermometer, | 78° F. | 76° F. | 74° F. |
| Experiment commenced at | 11.45 A.M. | 12.10 P.M. | 11.50 A.M. |
| Temp. of water when drawn, | 73½° F. | 73½° F. | 75° F. |
| Temp. of water at the end of an hour on entering the bath, | 74° F. | 74° F. | 75½° F. |
| Temp. of water at the close of an hour on leaving the bath, | 76½° F. | 76½° F. | 78° F. |
| Heat imparted to the water, deducting normal error, | 2° F. | 2° F. | 2° F. |
| Volume of water in the bath, | 7½ cubic feet. | | |
| Volume of the body, | 3 " " | | |
| Weight of the body, | 180 lbs. | | |
| Height of the body, | 5 feet 5½ inches. | | |

In the first and second experiments I laid perfectly still; the results therefore show the quantity of heat passing off from the surface of the body in a state of rest. This, as the table indicates, could warm seven and a half cubic feet of water two degrees in one hour. The volume of the body being three cubic feet, it follows that if we consider the specific heat of the body as about the same as that of water (which it probably is), enough heat is evolved in the course of one hour to warm the body itself about five degrees of Fahrenheit's scale. The converse of this may also be considered as true, viz., that after death, the air being at 73°, enough heat is lost in the course of an hour to cool the body five degrees, at least during the first hour. It is therefore a fact of considerable importance from a medico-legal point of view, especially in estimating the time a body has been immersed in water after recent drowning when the temperature of the water is about 73°, as is the case with the Croton and other streams in summer.

In the third experiment one or other of the lower extremities was alternately kept in motion during thirty minutes of the hour in which the body was immersed in the bath. The movement consisted in extending and flexing the leg on the thigh at the rate of fifty flexions per minute, and being performed under the surface of water involved considerable muscular exertion. Notwithstanding this violent exertion, as the table shows, there was no increase in the amount of heat imparted to the water. The consequences flowing from this result are of great physiological importance, but we reserve their consideration until we have completed the history of our experiments. We therefore pass to the examination of

### *The physiological effects of the cold bath on the body.*

*Experiment of July 4,—Rest—Temp. of Bath* 74° *F.*

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| | Temp. before entering the bath. | Immed. after entering the bath. | After 1 hour in the bath & just before leaving it. | Immed. after leaving the bath. | One hour after leaving the bath. | Two hours after leaving the bath. |
| Temp. of the mouth, | 99° F. | 99° F. | 98° F. | 97° F. | 97° F. | — |
| " " armpit, | 96° F. | 97° F. | 95° F. | 92° F. | 96° F. | — |
| " " temple, | 96° F. | — | — | — | 94° F. | — |
| Rate of respiration, | 20 | 22 | 16 | 13 | 16 | 19 |
| Rate of pulse, | 74 | 73 | 65 | 54 | 60 | 72 |

*Note.*—A chill or shock was experienced on entering the bath, and the sensation of coolness remained while in the water. Skin was dry and hot for an hour and a half after coming out. Perspiration set in and skin became cool two hours and a half after coming out. Shortly after leaving the bath slept for 30 minutes.

*Experiment of July 5,—Rest—Temp. of Bath* 74° *F.*

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| | Before entering the bath. | Immed. after entering the bath. | After 1 hour in the bath & just before leaving it. | Immed. after leaving it. | One hour after leaving bath. | Two hours after leaving bath. |
| Temp. of mouth, | 99° F. | 99° F. | 98° F. | 97° F. | 97° F. | 98° F. |
| " " armpit, | 98° F. | 97° F. | 94° F. | 94° F. | 97° F. | 97° F. |
| " " temple, | 97° F. | — | 95° F. | 95° F. | 96° F. | 96° F. |
| Rate of respiration, | 17 | 21 | 18 | 15 | 16 | 16 |
| Rate of pulse, | 78 | 66 | 64 | 55 | 56 | 60 |

*Note.*—Symptoms same as in experiment 1, but not as well marked; slept 30 minutes as in preceding.

If in the tables we compare column 1, representing the condition before entering the bath, with column 4, representing the condition immediately after leaving it, we find that in both experiments the exposure for one hour to water at a temperature of about 74° F. lowered the temperature of the mouth two degrees, of the armpit four degrees, and of the temple two degrees. The rate of respiration is also diminished in one case two and in the other four movements, and that of the pulse twenty beats in one and twenty-three in the other. It is therefore evident that the effects of the long continued application of a degree of cold such as that employed, is to reduce the temperature of the body and the rate of respiration slightly, while it affects the rate of pulsation in a very profound manner.

One of the consequences of this effect of cold on the action of the heart was a great reduction in the quantity of oxygen introduced into the system. The rate of pulsation being reduced nearly one-third, the quantity of oxygen conveyed into the interior of the body was diminished in a somewhat similar ratio. In a short time this began to exert its influence on the nervous centers, and there was a overwhelming disposition to fall asleep, which was unconsciously indulged in in both experiments shortly after leaving the bath, notwithstanding the strong desire to keep awake for the purpose of recording the rates of pulse and respiration at given periods.

Another evident consequence of such a sluggish movement of the blood is the disposition to congestion of various internal organs, and herein we may see a partial explanation of the action of cold in causing inflammations, especially of those organs engaged in the processes of secretion and excretion.

The discussion of the results obtained has thus far been confined to the consideration of columns 1 and 4. I have followed this course because, while in the bath, a slight access of water to the armpit or to the temple causes irregularities in the thermometric indications. In the case of the respiratory movements it is also very difficult to avoid influencing them in the act of counting. The mouth temperatures are, it is true, free from the influence of external agents, but the differences are too small to be perfectly reliable. In the case of the pulse determinations none of these objections can be urged; they are considerable, and by counting for half a minute for every record made, the error is reduced to a maximum of one beat. The movements of the heart are, in addition, free from the liability to error that exists in the case of the respiratory movements.

Accepting the pulse determinations as being accurate and reliable indications of the effects produced, both while in the bath and out of it, we may return to the consideration of the tables, and compare together columns 1, 3 and 4. Recollecting that 1 represents the condition on entering the bath, and 3 that just before leaving it, after an immersion of one hour we find that the pulse was reduced nine beats in the first experiment, and fourteen in the second. If now we compare 3, the condition just before leaving the bath, with 4, the condition just after leaving it, we find that the rate of the pulse has diminished eleven beats in the first and nine in the second experiment. The explanation of this extraordinary reduction of the pulse rate on leaving the bath is by no means clear. One thing is, however, very evident, and that is, the profound effect of the application of cold, as is shown not only by the singular phenomenon of which we have just spoken, but also by the slowness with which the original rates of pulsation are regained, as is demonstrated by columns 5 and 6 of the tables.

The motion experiment of July 11th gave the same general physiological results as the rest experiments of July 4th and 5th. The difference being, that during the motion the respiratory movements became 30 per minute and the pulse 90, both regaining the rate represented in the rest experiments very soon after the cessation of the exercise. Placing this great increase of the respiratory movement in juxtaposition with the failure of the exercise to cause any perceptible increase in the temperature of the bath, it is evident that the contact of the cold water must put an almost absolute stop to the functions of the skin, and the whole duty of exhalation of vapor of water and consequent removal of heat is thrown on the lungs; hence the increased respiratory action, and hence also the special tendency of application of cold to the surface to produce inflammations of those organs by increasing the work they are obliged to perform, and raising the pulse-respiration ratio to that actually existing in pneumonia.

In conclusion, it may be observed that the primary and most important effect of the application of cold to the whole surface of the body is to reduce the action of the heart. This reduction is still further increased on removing the cold, if the application has continued for a sufficient length of time; and, as a consequence of the reduction of the heart's action, the phenomenon of stupor or sleep appears, produced either by deficient oxidation or by imperfect removal of carbonic acid. There is also a tendency to congestion of various internal organs, especially of the lungs, and the establishment of a pulse-respiration ratio similar to that of pneumonia.

**College of the City of New York, Oct., 1872.**

## ART. LIV.—*On the Quartzite, Limestone and associated rocks of the vicinity of Great Barrington, Berkshire Co., Mass.;* by JAMES D. DANA.

(Continued from page 370.)

### 3. *Stratification.*

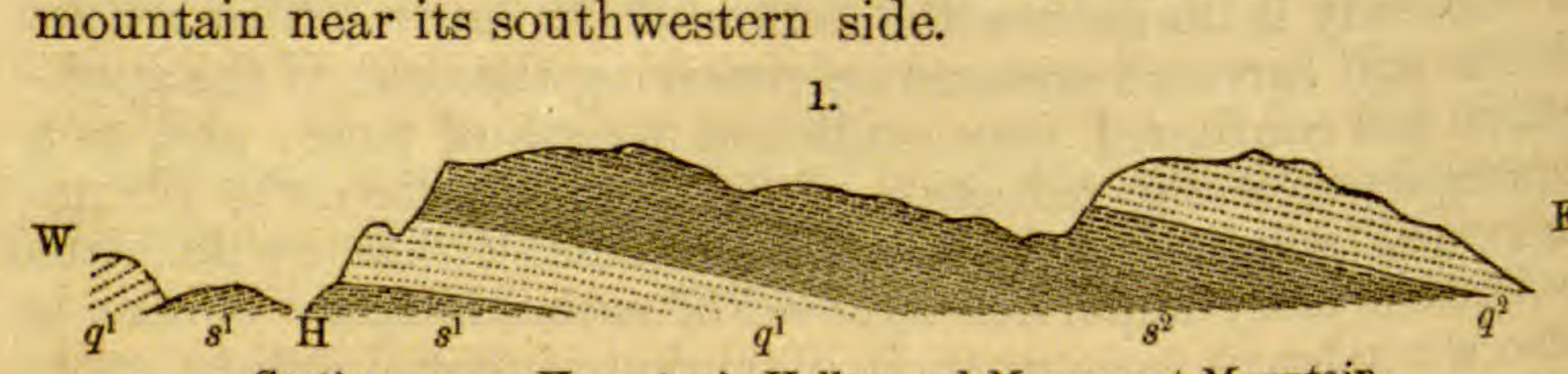
(A.) *Monument Mountain, and the Housatonic Valley adjoining it on the west.*

Monument Mountain has a precipice of hard quartzite on its eastern front,* and a wall of the same rock along the sides facing southwest and west, while through the interior the rock is mica schist and gneiss.

The hard quartzite of the walls is without bedding, but is jointed in various directions. The most regular and profound of these joints have approximately a north-and-south direction (N. 10° E. to N. 10° W.),† and are nearly vertical. Another system of divisional planes in the eastern mass of quartzite is strikingly apparent in a view from the Stockbridge road along the eastern foot (see map); the planes through the northern half dip northward 15° to 35°, increasing to the northward; and in the southern, southward 10° to 20°, the northern bending over into the southern. The former are also well seen along the mountain path near the *western* foot of this crest of quartzite, but the dip here in view is 45° to 50°. The strike on this western side is nearly east-and-west; at one place it was N. 70° E. The divisional planes of this system look as if due to bedding, and an anticlinal fold; and yet they are simply joints,—perhaps a result of contraction on cooling after consolidation.

The accompanying figures, 1 and 2, give a general idea of the stratification. The first is a section drawn through the mountain near its southwestern side.

1.



Section across Housatonic Valley and Monument Mountain.

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* This eastern quartzite crest or ridge of Monument Mountain is the part usually ascended by tourists. A path leaves the Stockbridge road near *p* (see map) and passes up the interior of the mountain, near the western foot of this ridge, to its north end, where is the best place of ascent; a branch path leads off also to its south end. The view takes in the Catskills on the west, Graylock to the north, Mount Everett and the Taconic range to the southwest, and, nearer by, the beautiful valley of the Housatonic, with its lakes and villages.

† The courses given in this paper are compass courses. The variation in the Barrington region is about 8½° W., which would make N. 10° E. compass course correspond to N. 1½° E. true course.

H is the position of the Housatonic river, and a little south of the line of the section is the village of Housatonic. Near $q^2$ (*p* on the map) passes the road, first northeastward, then northward, to Stockbridge. As shown, there are in Monument Mountain two strata of quartzite, $q^1$, $q^2$, with two of schist, (mica schist and gneiss) $s^1$, $s^2$, all dipping southeasterly, the average slope 25°.

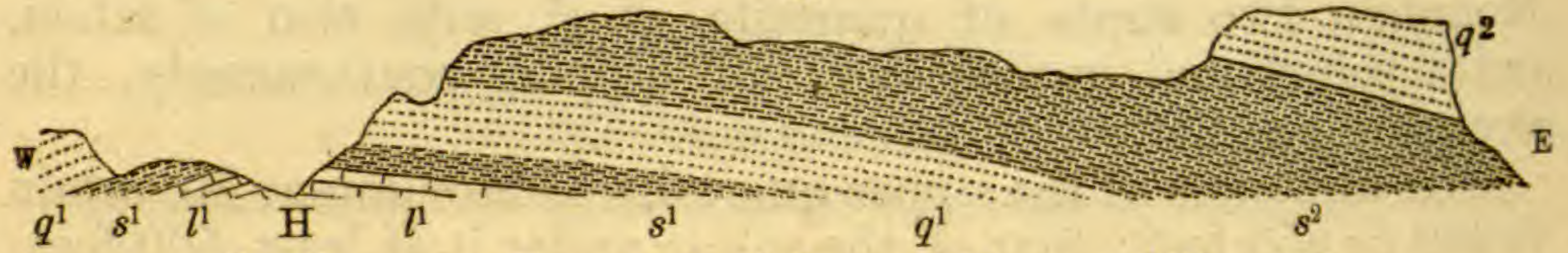
The thickness of the upper quartzite of Monument Mountain is 200 or 250 feet; that of the schist under it at least 500 feet; and that of the lower quartzite about 250 feet.

The schist ($s^2$) over the interior of Monument Mountain varies much in strike and dip, as is common in beds that are little inclined. The dip is for the most part to the east of south-east 20° to 25°, the strike being about N. 25° E. Just west of the eastern quartzite the amount of dip varied from 15° to 25°, and the direction was generally that just stated: yet in some places the strike was N. 50° W. and N. 70° W. At the quarry (*s* on the map), where there is a large quartz vein containing some black tourmalines (and a trace of copper pyrites) the bedding was much obscured by joints, but where best exhibited the strike was N. 25° E., and the dip 45° to 60°—an exception in its large amount to the general dip in the mountain.

There is no good outcrop of the lower schist ($s^1$) at the west end of Monument Mountain *east* of the Housatonic river; but the smooth surface of the foot hills or slopes in that part, and the sudden transition from numberless quartzite fragments to occasional masses of gneiss which is found on the descent of this side of the mountain, are evidence of its existence. Besides this, the stratum of schist is well exhibited on the opposite or *west* side of the river, where it first dips eastward at an angle of 20°, and then, farther from the river, *westward* 10° to 25°, the strike nearly north; a change of direction which indicates the existence along the valley of a low anticlinal, as represented in the section (fig. 1). A little farther west there is a bold ridge of quartzite, which is evidently the western side of the fold of the quartzite ($q^1$). A narrow depression or valley intervenes between the schist and this quartzite, so that the actual superposition of the former was not visible; but the dip of the schist was not only toward the quartzite on the east (as shown in the section), but also on the west of it in the Williamsville valley; and hence the quartzite is the course of a shallow synclinal. The quartzite was of the hard jointed kind, indistinct in its bedding. At one place I observed a westward dip of 20°—the strike N. 20° W.—in divisional planes which appeared to be those of the bedding.

The following section of Monument Mountain (fig. 2) represents the stratification along a line half a mile north of that of the first (fig. 1).

2.



Section across Housatonic Valley and Monument Mountain.

On the *west*, it passes the Housatonic river, near an old dismantled iron furnace, (*f*, on map), about three-fourths of a mile north of Housatonic village. On the *east*, it comes out in front of a limestone quarry, just west of the road to Stockbridge.

The upper quartzite, $q^2$, forms a bold precipice of 200 feet along the eastern front of the mountain. Below this and its talus, the slope is made up of great blocks of gneiss, many like houses in size, but all displaced, owing evidently to the wedging action of growing trees. This line of junction between the schist and quartzite, along the front, rises going *northward;* and at the northern limit of the quartzite crest, where this crest loses rather abruptly 150 feet or more of its height, it meets the stratified schist which is there in place as the top rock of the mountain ; while *southward*, the upper line of the schist falls gradually in height, corresponding with the southeasterly dip of the strata. The schist of the interior of the mountain here passes through to the east face, beneath the bed of quartzite.

In addition to this evidence as to the true stratification and position of the quartzite, there is *distinctly stratified quartzite conformable beneath the gneiss*, at the west end of the mountain, toward the summit. The locality where I observed this bedded quartzite is just above the level of the hard bedless quartzite, and about a hundred yards to the east of it, at a point marked *t* in the map ;* a weathered bluff of it much resembles in its regular lines of nearly horizontal stratification the bluff of gneiss that forms the western brow of the mountain a hundred feet above it. This bedded quartzite is a somewhat crumbling rock, and the decomposition of the lower layer is undermining the bluff. It lies conformably beneath the schist ; the dip of the quartzite is 15° to 20°, the strike N. 25° E., and the dip of the schist above and a little farther east 25°, with the same strike. The depression on the east side of the hard western quartzite (indicated in both sections), varying in depth from 40

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* The point *t* may be reached by taking a quarry road at *r* (see map) and following it beyond the quarries (*s*) until the road becomes a mere path (at *t*). The bluff of bedded quartzite is to the right of the path, a little distance from it.

to 80 feet, is probably due to the abrupt change in the rock from the hard jointed to the softer bedded kind.

Traces of similar bedding, and of like softness in the rock where bedded, occur at the northern margin of the *eastern* crest of quartzite, along the path descending northward; also in some places near the hard quartzite of the southwestern wall, where, at places, I passed isolated masses of great size undergoing deep disintegration, that bore evidences of the great amount of degradation which had taken place around them.

Descending the western slope, toward the Housatonic river and village, the quartzite is passed; and then a region of schist, indicated (as stated above) by a sudden substitution over the slopes of loose masses of schist in place of quartzite; and, finally, within less than a hundred feet of the base, on the path leading northwestward toward the old furnace, there is an outcrop of *limestone* with a slight easterly dip. West of the river the same limestone is exposed in the slopes beyond the road, and it there has a *westerly* dip (10° to 25°, the strike north) as shown in the section; and this limestone, at a point just west of a quarry, is visibly and conformably overlaid by mica schist, dipping 25° to the westward. Neither the limestone of section 2 nor the lower schist ($s^1$) (mica schist and gneiss, one passing into the other) of section 1 is exposed to view in the river banks, on account of the deep covering of stratified drift, so that the thickness of the schist was not ascertained; it probably does not exceed 50 feet. The limestone is the true Stockbridge limestone.

The same low anticlinal is here apparent that is represented in figure 1; and it is further evident that the anticlinal has an inclined axis dipping southward, inasmuch as the limestone, an inferior stratum in the fold, is exposed on going north, while covered to the south.

The mica schist of the interior of Monument Mountain, and part of the gneiss layers, decompose rapidly and deeply, and by this means much of the region is deeply covered with earth, which is partly clayey.

The main propositions stated at the commencement of this paper on the Great Barrington region—that the quartzite occurs interstratified conformably with the Stockbridge limestone and the accompanying mica schist and gneiss, and that the quartzite is one of the newer rocks in this part of the Green Mountains—appears to be well established by the facts already adduced. But others no less decisive are afforded by the country adjoining on the west, south, and east.

[To be continued.]

## ART. LV.—*On the relation between Color and Geographical Distribution in Birds, as exhibited in Melanism and Hyperchromism;* by ROBERT RIDGWAY.

THE two chief modifications of color experienced in the several geographical, or climatic, regions of the North American continent, by certain species of birds which are resident over a very extended area, are the following:—I. *A melanistic tendency*, which may be either an increase in the intensity of color or in the extent, of the black parts of the plumage; and II. *A greater brightness, or an increased prevalence, of the three primary colors, red, blue, and yellow.*

These features are mainly noticeable as the result of a tropical influence, for they are most highly developed in middle America, and become exaggerated in proportion to the decrease of latitude. But in the Pacific province of North America they are, in many cases, either entirely similar, or represented by somewhat modified analogous laws: thus, the law of melanism seems to be equally, if not more specially, characteristic of the Pacific province; the degree of modification of yellow tints also appears to be about the same in the two regions; but in regard to red, the rule seems to be that in middle America it is increased in *intensity*, and in the Pacific province of North America it is increased in extent, or *amount;* while blue seems to be affected latitudinally only, its prevalence, in certain cases, increasing as we trace a species southward.

These generalizations may be best illustrated by presenting the following especially noteworthy cases:

I. *Melanism.* A striking example in illustration of this law is found in *Chrysomitris psaltria*, under which we range as races, *C. Arizonæ* Coues, *C. Mexicana* Swains., and *C. Columbiana* Lafr. Specimens of this bird from the southern portion of the Western province of the United States (Rocky Mountains to California, its northern limit being about the parallel of 40°), have the black of the upper parts confined to the head, wings and tail, the entire dorsal region being olive-green; this form constitutes the true, or restricted, *C. psaltria.** Examples from Arizona, New Mexico, and the northern provinces of Mexico (var. *Arizonæ*),† have this olive-green clouded, or mixed, with

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* CHRYSOMITRIS PSALTRIA var. PSALTRIA.
*Fringilla psaltria* Say, Long's Exp., ii, 1823, 40.—*Chrysomitris psaltria* Bonap. List, 1838.—Baird, B. N. Am., 1858, 422.
*Hab.* Rocky Mts. and Middle province of U. S., north to about 40°.

† CHRYSOMITRIS PSALTRIA var. ARIZONÆ.
*Chrysomitris Mexicana* var. *Arizonæ* Coues, P. A. N. S., Philad., 1866.—Cooper, Orn. Cal., i, 1870, 170.
*Hab.* Southern border of U. S., in New Mexico and Arizona.

black, there being more of the latter color in the Mexican than in the Arizonan specimens. By the time we reach the latitude of Mirador and Mazatlan, the black entirely replaces the olive-green; the bird now is the var. *Mexicana*,[*] and continues with nearly the same characteristics south to Costa Rica and Panama, from which latter countries we find specimens in which the black is often appreciably more intense and lustrous than in those from Mexico. These three forms all have white on the tail; but in specimens from New Granada, and occasionally in those from Panama, there is usually a total absence of the white marks on the tail, or else they are greatly reduced in size. These equatoreal specimens (var. *Columbiana*)[†] exhibit the extreme of melanism in this species; and when compared with specimens of var. *psaltria*, only or without any of the intermediate races, appear so different as to convey the impression of entire distinctness; but when we bring together a large series, such as that contained in the Smithsonian collection, and notice how the increasing proportion of black strictly coincides with the decreasing latitude of the localities of the specimens, and observe, too, that it is utterly impossible to draw a latitudinal boundary to separate any two of these several races, the conclusion that they all represent climatic modifications of one species seems unavoidable. The females, it is well known, can scarcely be distinguished by experts.

*Myiarchus Lawrencii*, which, starting with a grayish brown crown in the most northern examples (var. *Lawrencii*),[‡] gradually assumes a blacker and blacker crown, as we trace it southward, first through var. *nigricapillus*[§] (Costa Rica and Panama), and finally ending in var. *nigriceps*[‖] (Ecuador), which has the

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[*] CHRYSOMITRIS PSALTRIA var. MEXICANA.
*Carduelis Mexicana* Swains., Syn. Birds Mex., Phil. Mag., 1827, 435.—*Chrysomitris M.* Bonap., Consp., 1850, 516.—Baird, B. N. Am., 1858, 423, pl. liv, f. 1.
*Hab.* Middle America (coast to coast), from Northern Mexico to Costa Rica.

[†] CHRYSOMITRIS PSALTRIA var. COLUMBIANA.
*Chrysomitris Columbiana* Lafr., Rev. Zool., 1843, 292.—Baird, B. N. Am., 1858, 423.—Sclater, Catal., 1862, 124.
*Hab.* Bogota to Panama.

[‡] MYIARCHUS LAWRENCII var. LAWRENCII.
*Tyrannula Lawrencii* Giraud, 16 sp. Texas Birds, 1841, pl. ii.—*Myiarchus L.* Baird, B. N. A., 1858, 18.—Sclater, Cat. Am. B., 1862, 233.—Coues, P. A. N. S., July, 1872.
*Hab.* Mexico, from near U. S. boundary southward to Mirador, Mazatlan, etc., and including both coasts.

[§] MYIARCHUS LAWRENCII var. NIGRICAPILLUS.
*Myiarchus nigricapillus* "Cabanis, MS.," Sclater, Cat. Am. B., 1862, 233.—*M. Lawrencii* Coues, P. A. N. S., July, 1872.
*Hab.* Middle America, from Southern Mexico to Panama.

[‖] MYIARCHUS LAWRENCII var. NIGRICEPS.
*Myiarchus nigriceps* Sclater, P. Z. S., 1860, pp. 68, 295 (Ecuador).—Ib., Catal. Am. B., 1862, 234.—Coues, P. A. N. S., July, 1872.
*Hab.* Panama to Ecuador.

crown deep black. *Sayornis nigricans*,[*] from California and Northern Mexico, has the crissum pure white; Mirador specimens have it clouded with dusky, while in Costa Rican specimens (var. *aquaticus*),[†] it is entirely blackish, only the middle of the abdomen being white. In all these cases the change is gradual, the two extremes being connected by a perfectly unbroken series of intermediate specimens. *Vireosylvia gilva* of the United States has the crown of an ashy gray color, and tropical American examples (var. *Josephæ*, Costa Rica and Ecuador), have it dark snuff-brown, while examples from Mirador, Mexico, are as exactly intermediate in colors as they are in habitat.

The same law as regards the Pacific province of North America is made evident by the well known cases of *Picus villosus* var. *Harrisii*,[‡] *P. pubescens* var. *Gairdneri*,[‖] *Sphyropicus varius* var. *ruber*,[¶] the Northwest coast forms of *Falco peregrinus*, *F. Columbarius*, *Bubo Virginianus*, *Scops asio*, and numerous other similarly affected species.

II. *The law affecting the primary colors.*—Of this class we may begin with yellow, as being the color whose changes are most nearly parallel with those of black, i. e., affected nearly similarly in middle America and the Pacific province of North America. The following cases afford illustrations:—*Xanthoura luxuosa*[**] from the Rio Grande of Texas and Northern Mexico, has the lower parts deep green, while the same species from Guatemala (var. *Guatemalensis*),[††] is pure gamboge yellow

[*] SAYORNIS NIGRICANS.
*Tyrannula nigricans* Swains., Phil. Mag., 1827, i, 367.—*Sayornis n.* Bonap., Compt. Rend., xxxviii, 657.—Baird, B. N. A., 1858, 183.; Scl., Cat., 1862, 200.
*Hab.* California and Northern Mexico.

[†] SAYORNIS NIGRICANS var. AQUATICUS.
*Sayornis aquaticus* Scl. and Salv., Ibis, 1859, 119 (Guatemala).
*Hab.* Central America, from Southern Mexico to Panama.

[‡] PICUS VILLOSUS var. HARRISII.
*Picus Harrissii* Aud., Orn. Biog., v, 1839, 191, pl. 417.—Baird, B. N. Am., 1858, 87.
*Hab.* Western Province of N. Am., and south into Mexico, where it grades into the smaller and still darker var. *Jardini*, which reaches its extreme condition in Costa Rica.

[‖] PICUS PUBESCENS var. GAIRDNERI.
*Picus Gairdneri* Aud., Orn. Biog., v, 1839, 317.—Baird., B. N. Am., 1858, 91.
*Hab.* Western Province of N. Am.

[¶] SPHYROPICUS VARIUS var. RUBER.
*Picus ruber* Gmel., S. N., i, 1788, 429.—*Sphyropicus ruber* Baird, B. N. Am., 1858, 104.
*Hab.* Pacific Province of N. Am.

[**] XANTHOURA LUXUOSA var. LUXUOSA.
*Garrulus luxuosus* Lees., R. Z., April, 1839, 100. *Xanthoura l.* Bonap., Consp., 1850, 380.—Baird, B. N. Am., 1858, 589.
*Hab.* Mexico, from the Rio Grande to Isth. Tehuantepec.

[††] XANTHOURA LUXUOSA var. GUATEMALENSIS.
*Xanthoura guatemalensis* Bonap., Consp., p. 380.
*Hab.* Yucatan, Guatemala and adjacent portions.

beneath, intervening localities producing specimens having a mixture of green and yellow below—either color predominating, according to the proximity to the region of the extreme to which it tends. Specimens of *Myiodioctes pusillus** from the Pacific coast of North America (var. *pileolata*,† Alaska to Costa Rica), are decidedly richer yellow than eastern ones; and the olive-green above inclines more to yellow. Specimens of *Helminthophaga celata* from the same region (var. *lutescens*),‡ are altogether more yellowish than eastern ones. The case of the genus *Geothlypis*§ furnishes another striking example of the tropical influence upon this color, and also affords a series of facts which lead to generalizations of other kinds.

The following synopsis—the result of the joint investigations by Professor Baird and myself, and modified from that already in print in our forthcoming work upon North American Birds, chiefly by the introduction of *G. speciosa* and *G. semiflàva*, the types of which, being in London, we have not seen, and which, until Mr. Salvin's notice of them, had not been sufficiently well characterized—will, perhaps, render the remarks which follow it more lucid:

### *Synopsis of the species and their subordinate races of the genus Geothlypis.*

Throat yellow, Series I; Throat ashy, Series II.

SERIES I.

A. Black "mask" extending backward beneath the eye, on to the auriculars. Bill slender, the culmen nearly straight (as in *Oporornis*).

  1. G. TRICHAS. Crown whitish; maxillæ black.
    Abdomen almost always whitish; occiput russet-olive. Bill, from nostril, ·30; tarsus, ·70; wing, 2·25; tail, 2·15. *Hab.* Whole of the United States in summer; in

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* MYIODIOCTES PUSILLUS var. PUSILLUS.
*Muscicapa pusilla* Wils., Am. Orn., iii, 103, pl. 26, fig. 4.—*Myiodioctes pusillus* Bp., Consp., 815. Baird, B. N. Am., 1858, 293 (in part).
*Hab.* Eastern Province and Rocky Mts. of N. Am.; eastern middle America in winter.

† MYIODIOCTES PUSILLUS var. PILEOLATA.
*Motacilla pileolata* Pallas, Z. R. As., i, 1831, 497.
*Myiodioctes pusillus* var. *pileolata* Ridgway.

‡ HELMINTHOPHAGA CELATA var. LUTESCENS.
*Helminthophaga celata* var. *lutescens* Ridgway.
*Hab.* Pacific Coast, from Radiak to Cape St. Lucas.

§ A very interesting paper upon the genus *Geothlypis*, by Mr. Salvin, appears in the London *Ibis*, for April, 1871. In this valuable article several generalizations are made, and theories suggested, which we may be able to corroborate by a few additional facts bearing upon the relationship of the several species of this genus and their subordinate forms.

winter south through middle America to Chiriqui, and throughout most of the West India Islands. *α. trichas.*[*]

Abdomen yellow; occiput russet-olive. Bill, ·45; tarsus, ·90; wing, 2·50; tail, 2·50. *Hab.* Nassau, Island of New Providence, Bahamas; resident. *β. rostrata.*[†]

Abdomen bright yellow; occiput whitish ash, tinged with yellow. Bill, ·32; tarsus, ·75; wing, 2·45; tail, 2·50. *Hab.* Eastern Mexico (Xalapa?). *γ. melanops.*[‡]

2. G. SPECIOSA. Crown black; abdomen ochraceous; bill wholly black. Wing, 2·40; tail, 2·30. *Hab.* Eastern Mexico. *α. speciosa.*[§]

Abdomen bright yellow; bill with the lower mandible yellowish. *Hab.* Ecuador. *β. semiflava.*[‖]

3. G. ÆQUINOCTIALIS. Crown deep ash; maxillæ yellow.

Black of the auriculars bordered posteriorly by the olive-green of the nape. Bill, ·17 deep; wing, 2·50; tail, 2·35. *Hab.* Northeastern South America (Trinidad, Guiana, Venezuela and New Granada). *α. æquinoctialis.*[¶]

Black of the auriculars bordered posteriorly by the ash of the crown.

Forehead narrowly black. Bill, ·14 deep; wing, 2·40; tail, 2·50. *Hab.* Southern South America (Brazil, Paraguay, Buenos Ayres, etc.). *β. velata.*[**]

Forehead broadly black. *Hab.* Chiriqui. *γ. Chiriquensis.*[††]

B. Black mask not extending beneath the eye, but confined to the lores and a narrow frontlet. Bill thick, the culmen curved (much as in *Granatellus*).

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[*] GEOTHLYPIS TRICHAS var. TRICHAS.
*Turdus trichas* Linn., S. N., 1766, 293.—*Geothlypis t.* Caban., Mus. Hein., 1860, 16.—Baird, B. N. Am., 1858, 241; Rev. Am. B., 1864, 220.

[†] GEOTHLYPIS TRICHAS var. ROSTRATA.
*Geothlypis rostratus* Bryant, Pr. Bost. Soc., Oct., 1866, 67.

[‡] GEOTHLYPIS TRICHAS var. MELANOPS.
*Geothlypis melanops* Baird, Rev. Am. B., 1865, 222.

[§] GEOTHLYPIS SPECIOSA var. SPECIOSA.
*Geothlypis speciosa* Scl., P. Z. S., 1858, 447. (We are unable to describe these two races more exactly, since we have not been able to see specimens; while the published descriptions are lacking in sufficient details.)

[‖] GEOTHLYPIS SPECIOSA var. SEMIFLAVA.
*Geothlypis semiflavus* Scl., P. Z. S., 1860, 273, 291.

[¶] GEOTHLYPIS ÆQUINOCTIALIS var. ÆQUINOCTIALIS.
*Slotacilla æquinoctialis* Gm., S. N., 1788, 972.—*Geothlypis æq.* Caban., Mus. Hein., i, 1860, 16.—Baird, Rev. Am. B., i, 1865, 224.

[**] GEOTHLYPIS ÆQUINOCTIALIS var. VELATA.
*Sylvia velata* Vieill., Ois. Am. Sept., ii, 1807, 22, pl. lxxiv.—*Geothlypis velatus* Caban., Mus. Hein., i, 1850, 16.—Baird, Rev. Am. B., i, 1865, 223.

[††] GEOTHLYPIS ÆQUINOCTIALIS var. CHIRIQUIENSIS.
*Geothlypis chiriquiensis* Salvin, Ibis, April, 1872. (No measurements are given in Mr. Salvin's description.)

4. G. POLIOCEPHALA. Crown wholly ash; maxillæ yellow.
   Eyelids white; nape and auriculars olive-green; abdomen whitish. Bill, ·30–·15 deep; wing, 2·20; tail, 2·50. *Hab.* Western Mexico (Mazatlan). *α. poliocephala.*[*]
   Eyelids black; nape and auriculars ashy; abdomen wholly yellow. Bill, ·35, ·18 deep; wing, 2·40; tail, 2·50. *Hab.* Guatemala, and Costa Rica. *β. caninucha.*[†]

SERIES II.

5. G. PHILADELPHIA. Head and neck wholly ashy.
   Eyelids dusky; lores dusky, not in strong contrast with the ash; black centers of the feathers of the gular region larger, or expanded, posteriorly, suffused with a patch—sometimes uniform—on the jugulum. Tail, 2·05 to 2·15. *Hab.* Eastern province of North America, south in winter (migrating across the Gulf of Mexico and Caribbean Sea, without stopping by the way!) to Costa Rica, Panama and Bogota. *α. Philadelphia.*[‡]
   Eyelids pure white; lores deep black in strong contrast with the ash; black centers of the feathers of the gular and jugular region, not larger posteriorly, and showing no disposition to form a patch on the jugulum. Tail 2·25 to 2·50. (Female distinguishable only by longer tail, more distinctly white eyelids, and more dusky lores.) *Hab.* Western province of North America, from British Columbia to Costa Rica. *β. Macgillivrayi.*[§]

In studying closely the affinities of the different forms given in the above synopsis, one of the most striking facts noticeable is that all of the peculiarly southern species, except *G. poliocephala*, have the belly wholly yellow; while the most northern species (*G. trichas*), with the belly whitish in the northern form, has it yellow in the two southern ones; two of the tropical American species have also the belly whitish in their northern (*G. poliocephala* and *G. speciosa*) and yellow in their southern races (*G. poliocephala*, var. *coninucha*, and *G. speciosa* var. *semiflava*). These facts we consider as evidences, if not proofs, of a tropical

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[*] GEOTHLYPIS POLIOCEPHALA var. POLIOCEPHALA.
*Geothlypis poliocephala* Baird, Rev. Am. B., i, 1865, 225.

[†] GEOTHLYPIS POLIOCEPHALA var. CANINUCHA.
*Geothlypis poliocephala* var. *caninucha* Ridgway.

[‡] GEOTHLYPIS PHILADELPHIA var. PHILADELPHIA.
*Sylvia Philadelphia* Wilson, Am. Orn., ii, 1810, 101, pl. xiv.—*Geothlypis Philad.* Baird, B. N. Am., 1858, 226; Rev. Am. B., 1865, 226.

[§] GEOTHLYPIS PHILADELPHIA var. MACGILLIVRAYI.
*Sylvia Macgillivrayi* Aud., Orn. Biog., v, 1839, 75, pl. 399.—*Geothlypis Mac.* Baird, B. N. Am., 1858, 224, pl. lxxix; Rev. Am. B., i, 1865, 227.

influence, whereby yellow is extended or intensified in intertropical regions. In the North American series of *G. trichas* (of which there are over one hundred and fifty specimens in the Smithsonian collection), it is also noticed that while specimens from the Atlantic States have, almost universally, whitish bellies, those from the valley of the Mississippi (as far north as Wisconsin), where the climate is much more warm and humid than that of the Atlantic coast in corresponding latitudes, as well as those from the Gulf States, frequently have the belly wholly yellow, and closely approach in characters also the Mexican race, var. *melanops.* Middle American examples of var. *trichas* are precisely like those from the United States, they being merely winter visitants to the south from the latter country. The var. *rostrata* we can only consider as a gigantic insular race of the common species, its yellow belly being due to its permanently southern habitat, and its larger size to be accounted for by local causes, as yet unknown. Specimens of var. *trichas* from the Pacific coast differ from eastern ones only in longer tails,* and in having a more yellowish tinge in the olive-green of the upper parts; so also has the western race of *G. poliocephala* a longer tail, and more yellowish cast to the upper plumage, than its eastern form (var. *coninucha*). While the western form of this species has a longer tail than the eastern one, and white, instead of dusky, eyelids, so also has that of *G. Philadelphia* (var. *Macgillivrayi*). In the case of *Myiarchus Lawrencii*, before alluded to, it is noticed that the yellow of the abdomen increases in richness, just at the same rate that the blackish of the pileum does in intensity, as it approaches its most southern extreme.

[To be continued.]

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## ART. LVI.—*A Theory of the Formation of the great Features of the Earth's Surface;* by JOSEPH LECONTE, Prof. Geol. and Nat. Hist. University of California.

[Concluded.]

As already stated, every other theory fails to account for the immense crushing together shown by *plication* and *slaty cleavage.* Many theories take cognizance of this crushing, but in all it is a *subordinate accompaniment* instead of the *cause* of the elevation. Let us examine very briefly some of the more recent theories, and show their inadequacy.

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* For a discussion of this law see Baird, in this Journal, vol. xli, March, 1866, pp. 21, 22.

Prof. Hall undoubtedly deserves the thanks of geologists for first strongly drawing attention to the fact that mountain chains consist essentially of immense masses of sediments, much thicker, indeed, than the height of the mountains themselves. His views on this subject form, I believe, an era in the history of geological science. Nevertheless, I think his theory entirely fails to explain the actual process by which mountain chains have been formed, and especially to account for the immense horizontal crushing and plication of the strata. According to Hall's view, as explained by himself and by Sterry Hunt,* the Appalachian chain has been formed as follows: This chain consists of sediments 40,000 feet thick, which thin out as we go west, until at the Mississippi river they are only 4,000 feet. We may regard the whole, therefore, as originally an immense *convex* mass of submarine sediment. By *slow subsidence* of this *convex* mass the upper strata were subjected to horizontal squeezing and thereby thrown into folds. *Continental* upheaval exposed the *still somewhat convex* mass of plicated strata *as a great plateau.* Subsequent erosion formed the ranges and ridges with their intervening valleys. Thus the Appalachian chain, and, in fact, mountain chains generally, become mere fragments of denuded plateaus upheaved by continental elevation only. This, I think, is a brief but fair exposition of the theory.

Now, as Whitney† and Hunt, and Billings, and others have shown, when we recollect the breadth of the Appalachians (at least 100 miles), and therefore the gentleness of the supposed convex curve, the amount of crushing together by subsidence would be inadequate to account for the immense plication. But the degree of inadequacy is, I think, scarcely appreciated. We have already said that slaty cleavage shows in many cases a crushing of 2½ miles into 1 mile. To produce such crushing by subsidence alone, the height of the convex mass would have to be greater than its base. Or even making every allowance for the fact that the area of principal plication is only 20 or 30 miles wide, still the height necessary would be enormous.

Besides, it is certain that the sedimentation was not finished first, and then afterward the subsidence occurred, but these two phenomena went on together *pari passu*; and, therefore, the surface was never convex at all, but nearly or quite horizontal all the time. Subsidence under such circumstances might produce horizontal *tension* or stretching of the lower strata, but could not produce horizontal *crushing* and plication of the upper strata.

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* "Some points in American Geology," this Jour., May, 1861.

† Whitney, Mountain Building, p. 101. Hunt, American Geology, this Jour., May, 1861.

In addition to this, after the whole of the Appalachian sediments had been deposited, at the end of the Paleozoic era and immediately before these mountains were formed, the Appalachian region was nearly or quite on a level with the sea, being, in fact, during the Coal period, alternately a coal marsh and an estuary, and therefore *lower than the regions east and west of itself.* The mountain formation was a process entirely distinct from and subsequent to the sedimentation and the subsidence. The whole process seems to have been, first, an immense sedimentation and subsidence going on *pari passu* during the whole Paleozoic era; then, at the end of that era, a horizontal crushing together and folding of the strata, and an upswelling of the whole mass. Hall and Hunt leave the sediments just after the whole preparation has been made, but before the actual mountain formation has taken place; and, therefore, in the language of Dana, "it is a theory of mountains with the mountains left out."

Whitney,* in his admirable essay on Mountain Building, if I understand him aright, thinks plication the result of the subsidence of a mountain axis, previously elevated by other agency. The subsidence of such an elevated axis would indeed produce powerful horizontal thrust, and might therefore produce some plication. But why suppose a *previous* elevation at all, when the horizontal thrust necessary, by his own view, to produce the elevation, would itself produce the plication? Or how high must have been the axis to have produced by subsidence such plication as we often find! Or how was this enormously elevated mass supported? It is evident that the objection to Prof. Whitney's view is precisely the same as to Prof. Hall's.

Mountain chains and mountain ranges are therefore, I think, beyond question, produced by horizontal thrust crushing together the whole rock mass, and swelling it up vertically; the horizontal thrust being the necessary result of secular contraction of the interior of the earth. The smaller inequalities, such as ridges, peaks, gorges, and, in fact, nearly all that constitutes scenery, are produced by subsequent erosion.

I feel considerable confidence in the substantial truth of the foregoing statement of the mode of formation of *mountain chains.* As to the mode of formation of *continents* and *sea bottoms*, I feel less confidence. It is possible that even these may be formed by a similar unequal yielding to horizontal thrust, and a similar crushing together and up-swelling. If so, it would be necessary to suppose the amount of horizontal yielding in this case much less, but the depth effected much greater, than in the case of mountain chains. But, as we find no un-

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* Mountain Building, &c., p. 106.

mistakable structural evidence of such crushing, except in the case of mountain chains, I have preferred to attribute the formation of continents and sea bottoms to unequal *radial* contraction.

I wish next to show that this theory of mountain chains *explains* in a satisfactory manner not only the mountain elevation and the phenomenon of plication and slaty cleavage, but also *all the most conspicuous phenomena of mountain chains and of igneous agencies.* The satisfactory explanation of these become, of course, strong evidence of the truth of the theory. The further development of the theory will be best undertaken in connection with the explanation of these phenomena.

(A.) *Thick sediments of mountain chains.* It is a well-known fact, first brought prominently forward by Prof. Hall, that mountain chains are composed of enormous masses of sediments. This fact forms the basis of Hall's sedimentary theory. Prof. Whitney,* it is true, thinks that the sedimentary theorists have mistaken cause for effect,—that thick sediments are not the cause of mountains, but mountain chains are the cause of thick sediments. He believes that a granite axis upheaved out of the sea has furnished by erosion the sediments which have been deposited on their flanks. But when we remember the immense thickness of these sediments and their extent, and the comparative narrowness of the granite axis which furnished their materials, we may well ask what must have been the original altitude of this granite axis! It seems impossible that the granite axis of a chain should have furnished by its erosion the immense mass of sediments involved in the structure of the whole chain. Not only so, but in many chains the strata are not only found on the flanks, but even the highest peaks are stratified. And not only so, but many chains, like the Appalachians and the Jura, have no granite axis at all from which to obtain their sediments. Whitney regards these latter as exceptions, and as always comparatively small chains, and probably formed in a different manner from the great chains with granite axes. My own belief is that all, smaller and greater, have been formed in a similar manner. Mountain sediments, I believe, are not the débris of the *granite axis* of the chain; they are evidently the débris of *continental erosion. Mountain chains*, strata, granite axis and all, *are off-shore deposits.* To state the proposition more definitely: *Mountain chains are formed by the mashing together and the up-swelling of sea bottoms where immense thickness of sediments have accumulated; and as the greatest accumulations usually take place off the shores of continents, mountains are usually formed by the up-pressing of marginal sea bottoms.* We will make this plainer by some illus-

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* Mountain Building, &c., pp. 102 and 103.

trations taken from the history of mountain chains in North America.

*Appalachians.*—The area now occupied by the Appalachian chain was, during the Silurian and Devonian ages, the *eastern margin of the bed of the great interior Paleozoic sea.* During all this time the whole Paleozoic sea, but especially this *eastern margin*, received sediments from a continental mass to the northward (the Laurentian area), and also especially from a *continental mass to the eastward.* Besides the marks of shore deposit found abundantly in the Appalachian strata, other evidences are daily accumulating that the area to the east of the Appalachian chain, left blank in the geological map of the United States in Dana's text book—the so-called primary or gneissic region of the Atlantic slope—is Laurentian, and therefore was probably *land* during the Paleozoic times. The size of this eastern continental mass it is impossible for us now to know, as it has been partly covered by later deposits, and perhaps even partly covered by the sea; but, judging from the quantity of sediments carried into the Paleozoic sea. and especially from the thickness of the sediments (30,000 feet) along its eastern margin, derived probably wholly from this source, it must have been very large.

At the end of the Devonian age, much of the middle portion of the interior Paleozoic sea was upheaved and became land (see Dana's map, Manual, p. 133); and the Appalachian area now became alternately a coal marsh and an estuary emptying into the sea southward. Into this estuary or marsh, during the whole Coal period, sediments were brought from land north, east, and west, until 10,000 feet more had been deposited. During the whole of this time (Paleozoic era), while the 40,000 feet of sediments were depositing, this area—whether sea-margin bottom, or estuary bottom, or coal marsh—slowly subsided, so that nearly the same level was maintained. It was either shallow water or marsh all the time. This is certain for the Coal period, and almost equally certain for the previous periods. Moreover, it seems to be a general law throughout the whole geological history of the earth, that areas of great sedimentation have been also areas of subsidence *pari passu.* The same seems to be true now. Nearly all great river-deltas are slowly subsiding. In fact, in all shallow water deposits, and therefore in all shore deposits, the accumulation would soon cease, and therefore never become thick, but for the subsidence which constantly renews the conditions of deposit. The subsidence of the Appalachian area, therefore, must have been 40,000 feet vertical.

During the Coal period, therefore, the Appalachian region was still nearly on a level with the sea. So far from being a convex plateau, it was a north-east and south-west trough. So far from being a mountain chain, it was evidently lower than the regions east and west of itself. At the end of this period occurred the Appalachian revolution. *The great mass of sediments which had been accumulating for so many ages, with their included seams of coal, yielded to the horizontal thrust,* was crushed together, and folded and swelled upward to a height proportionate to the horizontal crushing. Thus was the Appalachian formed—subsequent denudation has made it what it now is. It is probable that in the process of the up-pushing of the chain (or possibly at a later time) the eastern continental mass was diminished by subsidence.

*Sierras.*—We have good reason to believe that, at least some portion of the area now occupied by the Rocky Mountains was dry land even during the Paleozoic era. To what extent or what height we do not know. I shall say nothing of the formation of this the oldest portion of the North American Cordilleras, as the history of its formation is little known. I will commence with a considerable body of land which certainly existed in this region at the beginning of the Mesozoic era. Now, during the whole Triassic and Jurassic periods, *the region now occupied by the Sierras was a marginal sea bottom, receiving abundant sediment from a continental mass to the east.* At the end of the Jurassic, this line of enormously thick off-shore deposits yielded to the horizontal thrust, and the sediments were crushed together and swelled upward into the Sierra range. All the ridges, peaks, and cañons—all that constitutes the grand scenery of these mountains—has been the result of an almost inconceivable subsequent erosion.

*Coast range.*—The up-squeezing of the Sierra range, of course, transferred the coast line farther westward, and the region now occupied by the coast range became the marginal sea bottom. This in its turn received abundant sediments from the now greatly enlarged continent until the end of the Miocene, and then it also yielded in a similar manner and formed the coast range.

Thus I think it quite certain that the places now occupied by mountain chains have been always previous to their formation places of great sedimentary deposit, and therefore most usually marginal sea bottoms, since this is the most usual place for great deposits. In some cases, however, probably in many cases, *the deposits in interior seas or mediterraneans have yielded in a similar manner, giving rise to more irregular chains* or groups of mountains. This may have been the case with some of the more irregular mountains of Europe.

(B.) *Position of mountains along the margins of continents.*—The view that mountain chains are the up-squeezed sediments of marginal sea bottoms completely explains the well-known law of continental form, viz., that continents consist of interior basins with *coast chain rims*. In fact, the theory necessitates this as a *general* form of continents, but at the same time prepares us for exceptions in cases of mountains formed from mediterranean sediments. The view is best illustrated from the American continent, because of the regular manner in which this continent has been developed. Nearly all geological problems seem to be reduced to their simplest terms, and therefore are most easily studied and understood in America.

Prof. Dana, in a paper on "the plan of development of the American continent,"* brings out some grand views on the relation of the heights of coast chains and their position, to the size and depth of the oceans which they overlook. From these formal laws, and proceeding on the hypothesis of a fluid interior, he concludes that sinking sea bottoms, determined by interior contraction, is the force by which continents are elevated. According to him, the sinking sea bottoms, together with the lateral thrust produced by interior contraction, push up the continents, at the same time crumpling up their margins into mountain chains. Such a process might certainly account for coast chains, for their position at right angles to the greatest expanse of ocean, and for their heights and crumplings being in proportion to the size and depth of the contiguous oceans; but the mechanics of the process is, it seems to me, untenable. For observe: this subsidence cannot be gravitative subsidence; for this could not raise continents. It is evidently a concave bending of the sub-oceanic earth-crust pressing on the liquid interior, and through it pushing up the continental crust. Now I have already shown that no stiffness of crust—not even if the crust were several hundred miles thick—could stand such strain over such immense areas. While I admire, therefore, the formal laws of Prof. Dana, I cannot accept his physical explanation.

(c.) *Parallel ranges.*—Whitney, in his essay on Mountain Building, already referred to, has drawn attention to the fact that the celebrated law of Elie de Beaumont, that parallel ranges of mountains are of the same age, so far from being true is nearly the opposite of the truth. Parallel ranges, at least of the same great system, are nearly always successively formed; and I would add *successively formed coastward.* He illustrates this by reference to the three great ranges of the North American Cordilleras, viz., the Rocky Mountains, the Sierras, and the Coast range—and by the several ranges forming the South American Andes. The theory I have presented at once explains this fact, and erects it into a law. It is a necessary result of the theory.

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* This Jour., II, vol. xxii, p. 335.

In this connection, I will throw out a suggestion. Attention has been often directed to the truly wonderful submarine ridges and hollows brought to light by the U. S. Coast Survey, as occurring in the course of the Gulf stream, and extending all along the coast from the point of Florida to the coast of New England.* These ridges are truly submarine mountain ranges running parallel with the coast, and to the Appalachian.

7.

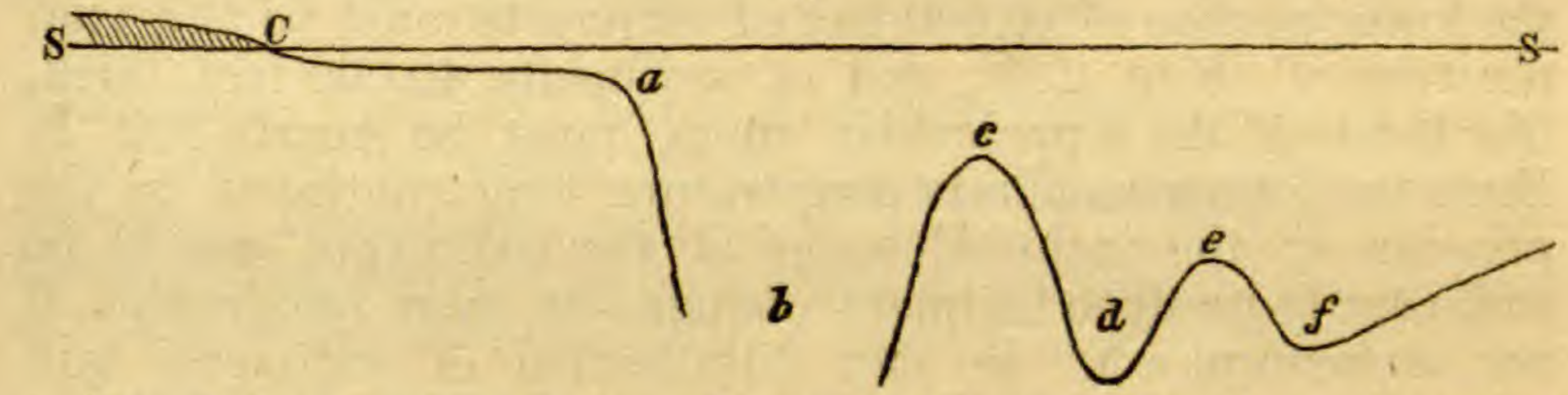

Fig. 7 is a rude section diagram illustrating the submarine configuration. Commencing at Charleston, C, the sea-bed slopes very gradually, so that at the distance of 50 miles (*a*) it attains the depth of only 20 fathoms. From this point it slopes very abruptly, so that it quickly attains unfathomable depth (*b*). At the distance of 100 miles from shore, at the depth of 300 fathoms, is found a ridge (*c*) rising from unfathomable depth on the coastward side, and 1,500 feet above the hollow (*d*) on the seaward side. At the additional distance of 20 miles is another ridge (*e*) 500 feet high, followed by another hollow (*f*) from which the bottom rises gently. The Gulf Stream is parted into three streams by these two ridges. I once (1856) threw out the suggestion that these ridges might be formed by sedimentary deposit from the Gulf Stream itself. I now throw out another, and perhaps a more probable suggestion, in connection with the theory now under consideration. May not these wonderful ridges and hollows be parallel ranges of mountains *now in the course of formation* by the process described? Or else they may be *ranges formed* long ago *on the Atlantic border of the old eastern continental area*, as the Appalachian was formed on the interior basin margin of the same area. In this case, we may suppose they have become submerged in the partial subsidence of this continental area which subsequently took place.

(D.) *Metamorphism of mountain chains.*—From the evidence thus far brought forward, I think it almost certain that mountain chains are formed by the squeezing together and upswelling of lines of off-shore deposit. But the question naturally arises: *Why does the yielding to horizontal pressure take place along these lines in preference to any other?* I believe that the answer to this question is to be found in the recent views

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* Prof. Bache, Proc. Am. Assoc., 1854, p. 140.

on the subject of *the aqueo-igneous fusion of deeply buried sediments.*

The accumulation of sediment, as first shown by Babbage, and afterward by Sir John Herschell, necessarily produces a rise of the geo-isotherms and an invasion of the sediments by the interior heat of the earth. From this cause alone, taking the increase of interior heat at 1° for every 58 feet, or about 90° per mile, and adding the mean surface temperature (60°), the lower portion of 10,000 feet of sediments must be at a temperature of about 230°, and of sediments 40,000 feet thick, like those of the Appalachian chain, must be nearly 800° F. Even the former moderate temperature, long continued in the presence of the included water of the sediments, would be sufficient to produce incipient change—at least *lithification*, if not metamorphism. In fact, lithification of sediments will probably take place under heavy pressure even at ordinary temperature, but is no doubt hastened by high temperature. The *latter* temperature of 800° is certainly sufficient to produce not only metamorphism, but aqueo-igneous pastiness, or even complete aqueo-igneous fusion. With a small quantity of alkali in the included water of such sediments, all these changes would take place at much lower temperature.

Suppose, then, sediments accumulating along the shores of a continent: The first effect is lithification, and therefore increasing density, and therefore contraction and subsidence *pari passu* with the deposit. Next, if the sedimentation continue, follows aqueo-igneous softening, or even melting, not only of the lower portion of the sediments themselves, but of the *underlying strata upon which they were deposited.* The subsidence probably continues during this process. Finally, this *softening determines a line of yielding to horizontal pressure*, and a consequent up-swelling of the line into a chain. Thus are accounted for, first, the *subsidence*, then the *subsequent upheaval*, and also the *metamorphism* of the lower strata so universal in great mountain chains. By this view, of course, the exposure of the metamorphic rocks on the surface is the result of subsequent erosion. Even the granite axis, I believe, in *most cases*, is but the *lowermost*, and therefore the most changed portion of the squeezed mass, exposed by subsequent erosion; although it is by no means impossible that in some cases the granite may be *squeezed out* as a pasty mass through a rupture at the top of the swelling mass of strata.

The theory, as will be observed, strongly inclines toward the metamorphic origin of granite, but does not require it. For there is nothing to hinder the aqueo-igneous fusion of an original granite crust by the accumulation of sediments upon it, and the consequent yielding of the crust along the line of accumulation.

(E.) *Fissures and slips.*—The enormous foldings of the strata which must occur in the formation of mountain chains by lateral thrust would, of necessity, produce fractures at right angles to the direction of thrust, or parallel to the folds, *i. e.*, to the range. The walls of such fissures would often slip *by readjustment by the force of gravity;* or else might be *pushed one over the other by the sheer force of the horizontal thrust.* The first case would give rise to those slips in which the foot wall has gone up and the hanging wall down, which are by far the most common slips in gently folded strata. The latter would give rise to those slips, often found in strongly folded strata, as in the Appalachian, in which the hanging wall has been pushed upward. The sudden rupture of the earth's crust, under the accumulating forces tending to bend it, sufficiently account for the phenomena of *earthquakes.*

(F.) *Fissure eruptions.*—The theory may, with much probability, be pushed so as to include volcanic phenomena also. There can be no doubt that the liquid and semi-liquid matters ejected by volcanoes vary in temperature and in kind of fusion in every degree from hot volcanic mud, through all stages of aqueo-igneous fusion, to pure or almost pure igneous fusion. Perhaps all the stages of aqueo-igneous fusion may be accounted for by the invasion of sediments and their included waters by the interior heat of the earth, as already explained. But the enormous temperatures often found in lavas cannot thus be accounted for. But it seems not unlikely, nay, even almost certain, that the invasion of sediments by interior heat would induce slow chemical action, which might increase the heat to almost any extent, so as even to produce true igneous fusion. If these views be correct, then beneath every great line of sediments, such as the off-shore débris of a continent, there exists a mass of partially or completely fused matter. When the line of sediment yields, and the strata are folded and fissured, the underlying fused mass is squeezed into the fissures to form dykes, or through the fissures and outpoured upon the surface as great *fissure eruptions*, which sometimes form the great mass of mountain chains.

(G.) *Volcanoes.*—There can be no doubt, I think, that the foundation of a true scientific geology was first laid by Lyell, in the study of "Causes now in operation." Nevertheless, the assimilation of agencies in previous geological times to those now in operation may be carried too far. As an example of this, I would mention the tendency among the most careful geologists to make our present volcanoes the type of all igneous ejections in all times. But I think no one who has examined the so-called volcanic rocks on this coast, both in the Sierras and in the Coast chain, but especially in the former, can for a moment imagine that these immense floods of lava have issued from craters. The lava floods of the Sierra and Cascade ranges are, it seems to me, among the most extraordinary in the world. Commencing in middle California as immense but separate lava streams, in northern California it becomes an almost universal flood several hundred feet thick; in Oregon the flood becomes universal, and at least 2,000 feet thick, and this continues through Washington Territory and into British Columbia, how far I know not. An area 700 to 800 miles long and 80 to 100 miles wide seems to be almost universally covered with lava, and the thickest part where it is cut through by the Columbia river is not less than 2,000 to 3,000 feet thick. Over this immense area are scattered a dozen or more extinct volcanoes—mere pimples on its surface. It is simply incredible that all this lava has flowed from these volcanoes. There is no proportion between the cause and the effect. I am compelled to adopt the view of Richthofen* and of Whitney, that such great masses of lava, often constituting, as it does in this case, the *chief bulk of mountain chains*, have come, *not from crater eruptions, but from fissure eruptions*,—and that volcanoes are only secondary phenomena produced by the access of meteoric water to the still hot interior portions of these great fissure eruptions. Thus, as monticules are parasites on volcanoes, so are volcanoes parasites on massive eruptions, and massive eruptions themselves parasites on an interior fluid mass. This interior fluid mass, however, according to Richthofen and Whitney, is the supposed *universal incandescent liquid interior*, while I believe it is *the sub-mountain reservoir* locally formed as above explained.

By this theory, as by every other theory of mountain formation, it is necessary to suppose that there have been in the history of the earth *periods of comparative quiet*, during which the forces of change were gathering, and *periods of revolutionary change*—periods of gradually increasing horizontal pressure, and periods of yielding and consequent mountain formation. These latter would be also periods of great fissure-eruptions, and these, during the more quiet subsequent period, would be followed by volcanoes gradually decreasing in activity. The last of these great fissure-eruption periods on this coast was the Post-tertiary. The great lava flood which forms the Cascade range, where it is cut through by the Columbia river and its tributaries, *is every where underlaid by the northern boulder drift.*† Since that time we have been in what might be called a crater-eruption period, which was once extremely active but has gradually decreased, until now only geysers and solfataros remain.

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* Richthofen, Natural System of Volcanic Rocks; Memoirs of Cal. Acad. Science, vol. i, part 2d.

† I hope soon to give the evidence of this in a separate communication.

I confess I do not see how either volcanoes or massive eruptions can be accounted for, except by the mode now explained. It is now, I think, generally conceded that lavas and other igneous ejections, at the time of their ejection, were in most cases only in a state of aqueo-igneous fusion, and therefore, cannot be regarded as evidences of the interior liquid. It must also be conceded that the focus of earthquakes and volcanoes are too superficial to have any immediate connection with an interior liquid, supposing such to exist. Volcanoes therefore must have their origin either in locally formed accumulations of liquid, as maintained in this paper, or else in local extensions of the general interior liquid, partially or entirely isolated within the solid crust.

In regard to fissure-eruptions, nothing but general contraction and a squeezing out of liquid matter can account for them. Whitney* thinks this squeezing out the result of subsidence of areas on either side of the mountain chain. I confess I do not understand the mechanics of this. Of course it could not be subsidence by weight, for this is inconsistent with the principles of hydrostatic pressure. It could only be by a concave bending of a stiff crust pressing on a fluid interior; but this over a large area is impossible, for the reasons already given in the early portion of this paper. Besides, pressure on a general interior liquid would be propagated equally to every portion of the interior surface of the solid crust, which would therefore yield not necessarily in a contiguous part, but at the weakest point wherever that may be. In fact, if we admit the interior fluidity of the earth, the mechanics of igneous agencies is surrounded with insuperable difficulties on every side. The more we try to arrive at clearness the more the difficulties seem to accumulate.

The theory which I have just presented accounts, it seems to me, for all the principal facts associated in mountain chains. This is the true test of its general truth. It explains satisfactorily the following facts. 1. The most usual position of mountain chains near continental coasts. 2. When there are several *ranges* belonging to one system, the ranges have usually been formed successively coast-ward. 3. Mountain chains are masses of immensely thick sediments. 4. The strata of which mountains are composed are strongly folded, and where the materials are suitable, affected with slaty cleavage; both the folds and the cleavage planes being usually parallel to the mountain chain. 5. The strata of mountain chains are usually affected with metamorphism, which is great in proportion to the height of the mountains and the complexity of the foldings. 6. Great

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* Mountain building, etc., p. 90.

fissure-eruptions and volcanoes are usually associated with mountain chains. 7. Many other phenomena—such as fissures, slips, earthquakes, and the subsidence preceding the elevation of mountains, it equally accounts for.

It will be remarked that the theory, though in its general features, not dependent upon, yet strongly inclines toward and is powerfully supported by, the views of Rose, Bischof, Hunt and others as to the metamorphic origin of granite and even of igneous rocks; the view that surface materials have passed by perpetually repeated cycles, through all the stages of rocks and soils; igneous rocks disintegrated to soils, carried away and deposited as sediments, consolidated into stratified rocks, metamorphosed into gneiss, granite or even into lavas, to be again after eruption reconverted into soils and re-commence the same eternal round; and thus we look in vain for the *original material.* I confess I lean strongly to this view.

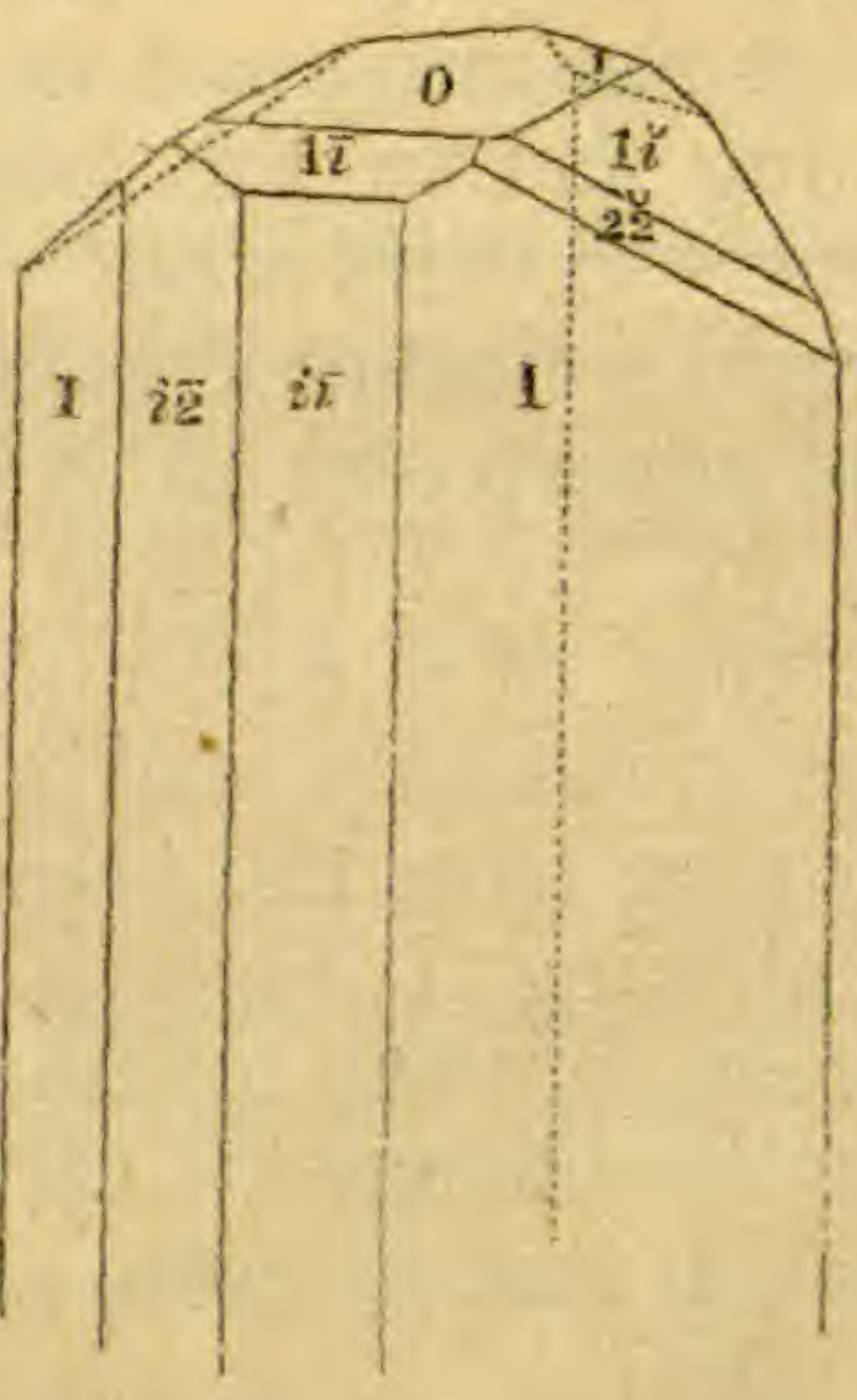
I am fully aware that there are some phenomena of movement of the earth's crust which are not explained by the foregoing theory. I refer especially to those great and wide-spread *oscillations* which have marked the great divisions of *time*, and have left their impress in the general unconformability of the strata. The last of these great oscillations took place during the Post-tertiary period. I cannot explain these oscillations. I am also painfully aware that the theory just presented, rests upon an insufficient knowledge of the structure of the earth. It is possible that the state of knowledge is not yet such as to warrant any attempt at a general theory. I feel quite sure that a perfect or even a satisfactory theory is not yet possible. I can only hope therefore that the theory here brought out may at least look in the right direction and will therefore serve as some guide in further investigation; that amid the modifications which theoretic geology must undergo in the advance of knowledge, some remnants of its outline will still remain visible. In any case, even if entirely wrong, it is at least a little more definite than anything we have. It is at least something tangible which may be attacked and overthrown by facts and by physical reasoning. We have had enough of vague theorizing in geology; of vague shadows through which the trenchant sword of science passes with no effect. It is time that the more perfect methods of physics were applied to geology.

**Oakland, Cal., May 15, 1872.**

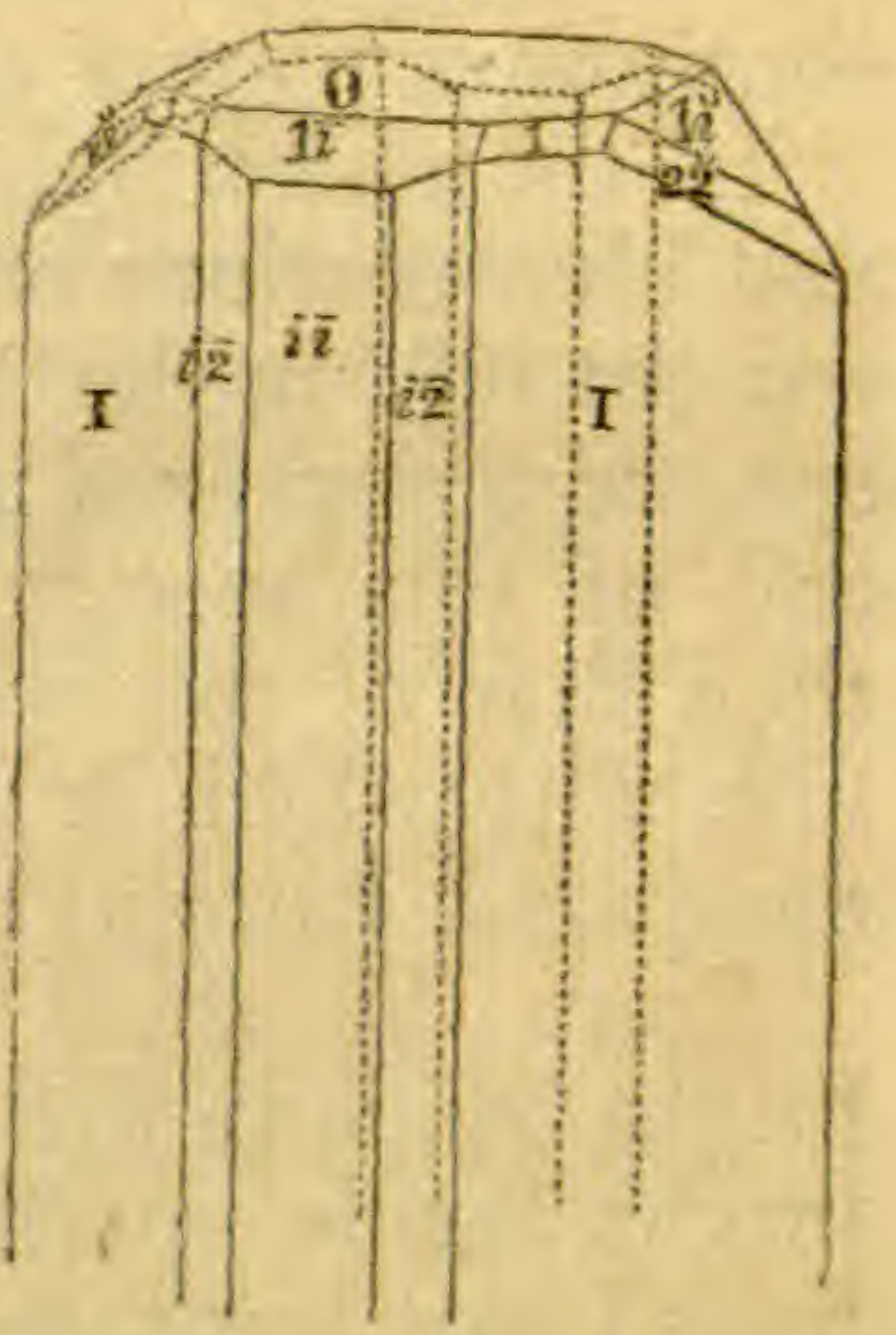
## ART. LVII.—*On a crystal of Andalusite, from Delaware Co., Pa.*; by EDWARD S. DANA.

THE annexed figures represent a remarkable crystal of andalusite, from Upper Providence, Delaware Co., Penn., received by Professor Dana from Dr. George Smith, and now in the Yale College Cabinet. Figure 1 shows the crystal (natural size) with the planes as actually occurring. It will be noticed that while all the known planes, with one exception (ĭ2̆), are present, there is an irregularity in their occurrence almost amounting to a kind of hemihedrism; 1-ī and ī2̄ appear but once, instead of twice, and ĭ2̆, 2-2̆, and 1 once, instead of four times. Figure 2 shows the crystal in its theoretical form, with all the planes as they would regularly occur. $I \wedge I$ gave 88° 15′ (that is, 91° 45′), and $O$ on the macrodiagonal section the angle 93° to 94°; this obliquity, however, is not in the right direction to explain the partial hemihedrism.

1.



2.



By the kindness of Mr. Vaux and Mr. Joseph Willcox of Philadelphia, I was enabled to examine other fine specimens from the same locality. The number of planes occurring on these crystals (one of which weighed more than 7 lbs.), was small, and there was nothing in their manner of occurrence to suggest that the peculiarity of the crystal figured was anything more than an accidental irregularity. In all of these specimens there was a great diversity in the prismatic angle, and obliquity in the angle of $O$ upon the diagonal sections was very common.

A word should also be added in regard to the cleavage in the specimens from Delaware Co. In most cases it was irregular, many of the crystals having a fibrous, tremolitic structure, and in others it was radiated. The regular cleavage parallel to the prismatic faces, however, did occur, and a chemical analysis of several of the specimens is needed to determine whether in the former case any change in constitution had taken place.

## ART. LVIII.—*Spectrum of Lightning;* by EDWARD S. HOLDEN, Lieut. of Engineers, U. S. Military Academy, West Point.

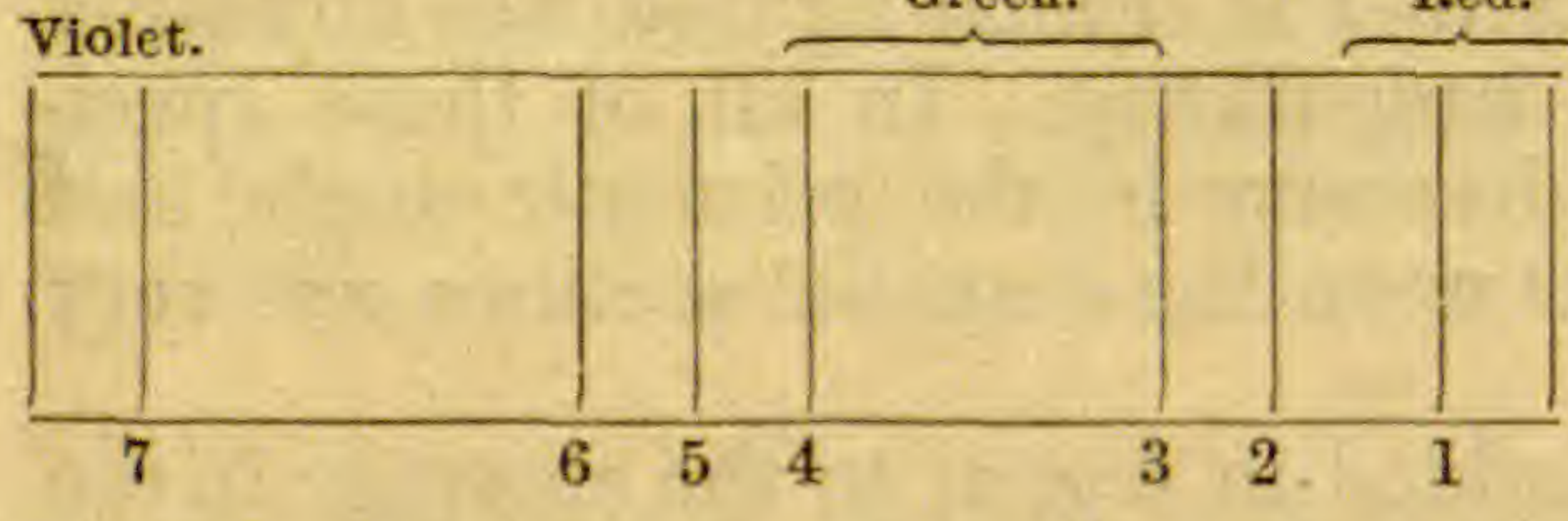
I DESIRE to communicate to you a few observations on the spectrum of lightning, which I could wish to be more complete. The instrument was a pocket spectroscope of Hawkins & Wales. The first set was made in Philadelphia shortly after sunset, on the evenings of August 13, 14, and 15, 1872. There was a continuous play of sheet lightning and frequent vivid flashes.

In the sheet lightning and in the fainter flashes the green and blue portions of the spectrum were visible, the violet and red cut off; in the brighter flashes a complete and continuous spectrum appeared and superposed on it bright lines. The red end of this spectrum (of vivid flashes) seemed to be shorter than that of the spectrum of a common gas-jet turned down low, with which it was constantly and almost instantaneously compared, without moving from the place of observation.

From the sheet lightning I repeatedly obtained series of bright bands in the green, but the width and intensity of these bright bands continually changed. Of the bright and sharp lines I saw but three:—1, line in green; 2, line in blue; 3, line in violet (or extreme blue?).

These were seen frequently, and sometimes those of one flash would be immediately succeeded by those of the following flash, thus giving me a means of assuring myself that the *same* lines appeared as well in *position* as in color.

The second set I regard as trustworthy. The observations were taken at West Point, N. Y., August 22d, 1872, at 6.00 P.M., looking toward the east through a violent rain. The spectroscope was first directed toward the sky and the spectrum with dark lines, which was constantly seen, was mapped as below, and the following notes taken:



*Notes*—The red ends slightly beyond 1.
2 = D line,
3 bounds yellow line.
5 near boundary of green.
6 seems to be sometimes green and sometimes blue.
7 violet = H.

The spectroscope was then turned to the lightning, and with the above dark lines as reference lines the following bright lines were mapped: (*a*) bright line *less* refrangible than red end (border) of spectrum, i. e., *extra red;* (*b*) bright line slightly *more* refrangible than 4 (see fig.); (*c*) bright line near 5 or 6 (fig.) "green *or* blue"; (*d*) bright line in blue between 6 and 7, once seen, bright purple.

The green portion seemed to have variable limits and to be disproportionately bright, but no green bands were seen. In my note book I have marked (*a*) (*b*) and (*c*) "sure of."

U. S. Mil. Acad., West Point, N. Y., Oct. 9, 1872.

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## *Letter from* Dr. B. A. Gould, *Director of the Observatory at Cordoba, to the Editors*, dated Cordoba, Sept. 4, 1872.

Scarcely a mail has gone out homeward for many months without my having experienced a strong desire to tell you of our progress, yet I have allowed sixteen months to pass, rather than repeat the old story of obstacles and delays; for, although we have all worked to the utmost of our power, this interval has served to show how erroneous were my estimates of what could be accomplished within a given interval, in a new country and at a distance from those facilities to which we are so thoroughly accustomed at home that it is difficult to feel how indispensable they are, or to make allowance for their entire absence. And while anxious to fulfill my promise of writing to you, I was unwilling to send tidings unaccompanied by accounts of something done toward the fulfillment of my original plan.

It lacks but a few days of two years since my arrival in Cordoba, when I anticipated that the work upon the zones would commence within six months and be completed within two years thereafter. Yet the sound of the hammer is still heard within the building; and the two years will have elapsed before the carpenters can take their leave. But if the institution thus established, and devoted to astronomical research in this clear and transparent sky, can now commence an era of full activity, I shall not feel that these years of toil in the joint capacity of architect, surveyor, master-builder, engineer and mechanician, as well as astronomer, have been in vain.

The various unexpected embarrassments, which have till now delayed the commencement of the zone observations, have not interfered with the Uranometry, which has advanced as uninterruptedly as moon and clouds would permit. During the first fourteen months of our sojourn, the survey of the entire sky south of the 10th degree of north declination was completed, the stars identified from the catalogues and their positions reduced to the mean equinox of 1872·0; or for those few which could not be identified, their positions fixed by estimation with sufficient sharpness to permit of recognition and accurate determination when the large instruments should be in working order. The scale of magnitudes above the 6th was carefully deduced from that of Argelander within the belt between 5° and 15° of N. declination and expanded to tenths of a magnitude. For stars below Argelander's limit, we had no trustworthy basis; and although a very large proportion of our stars were noted as 7 by Lacaille, and many even fainter than 7 by Lalande, Taylor and Brisbane, I was indisposed to believe that stars really below 6½ or 6·6 could be visible to the naked eye. Experiments made with considerable minuteness, to deduce the true magnitudes by determining the smallest aperture through which the stars could be distinctly seen, failed to give a satisfactory result, and thus I fixed upon 6·5 as the limit of average vision on an average night; and upon this basis the magnitudes below 6 were estimated, stars seen by the unaided eye being considered as below 6·7. The work had far advanced before the arrival of any means of accurately testing the exactness of this assumption; and my suprise was great when after a comparison of the faint stars within the type-belts with Argelander's "Durchmusterung" and Bessel's zones, there remained no room for doubt that the magnitude which we had been calling 6·5 was in reality not more than 7·0; and that on the clearest nights stars not brighter than 7·2 could be distinctly seen, while a considerable number which had been seen and recorded are not above the 7·5 magnitude. This is beyond all question, and indicates at once the transparency of our sky, and the sharp sight of our observers.

At first I had fixed upon the magnitude 6·5 as the most appropriate inferior limit for stars to be included in the Uranometry, but this discovery led to a total change of plan, and I now think that the limit ought not to be brighter than 7·0. It was not difficult to translate the magnitudes recorded into the corresponding true ones, since most of the stars had been observed two or three times, and by two or three observers; still as the progress of the building did not yet permit the erection of the meridian circle, I decided to submit the sky to a new revision. In the first scrutiny the whole region included in our work had been divided into 17 maps; 5 of which were assigned to Mr. Thome and 4 each to Messrs. Rock, Davis and Hathaway. The boundaries were so arranged that each of these maps lapped very largely upon the adjacent ones (which were of course assigned to other observers), so that there were few stars which did not fall within the limits of at least two observers, while there were several regions which belonged to the common domain of all four. Thus the scrutiny seemed good in so far as regards the detection of all stars within the limits of vision; while any systematic tendency to diversity in the estimates of the several observers could be at once recognized and remedied. The diversity of results in consequence of errors of observation or differences of judgment was small, and in most cases indicated that the true value was an intermediate one.

Nevertheless as I have said, a revision of the whole work has been undertaken, which is a far less severe undertaking than the original one, inasmuch as the stars have been already identified and their places reduced to the adopted equinox. For this revision a large number of additional faint stars have been added to the catalogue in the type-belt—those only being adopted as standards of magnitude to which all four observers assigned the same value. The scale thus established has been similarly transferred, by the accordant estimates of all, to two regions on opposite sides of the pole, and at about 60° declination, so that an abundant series of trustworthy standards of magnitude may be found within a convenient distance of any part of the sky. For the revision with this new scale, I divided the hundred degrees of declination which compose the field of our Uranometry into 35 charts, each mapped upon a scale nearly double that of the first series, which was easily attained by diminishing the amount of their overlapping. These have been so distributed as to assign to each observer regions different from those which he had previously observed, and each one is now engaged in repeating his former work, with the new standards of magnitude, and in observing the new charts of the revision series. Thus I think we may believe that no star brighter than the 7th magnitude will escape notice, that the misidentifications will be few, and that the final results for the magnitudes deduced from so large a number of observations, free from systematic discordance, will be entitled to a high degree of confidence.

This revision, as well as the repetition of the original work, are both of them more than half completed, and I see no reason to feel otherwise than very well satisfied with the results. My great nearsightedness has prevented me from taking any personal share in the observations, a circumstance which at first caused me deep regret; but this regret has disappeared since experience has taught me that the amount of labor entailed by the general arrangement of the work, and by the combination and scrutiny of the results, is quite incompatible with any considerable amount of direct observation. And I doubt whether this could have been more zealously or faithfully accomplished than by the gentlemen engaged in the work.

Of course this process has brought to light a considerable number of variable stars, of which I had hoped to be able to prepare a list before now; and has moreover served to fix reasonable suspicion upon a much larger number. Before long I trust to find opportunity to collect and arrange our data. You may well suppose that in the pressure of our labors there has been small opportunity to follow up the variations of these stars, so as to determine their periods. That duty must be left for a season.

Among those which have come to light I may mention one especially, since it belongs to the northern hemisphere. It is the star in *Monoceros*, No. 507 of the Hour VI. in Weisse's Bessel, the place for 1872·0 being

$$6^h\ 18^m\ 19^s + 7^\circ\ 9'{\cdot}2.$$

Bessel called its magnitude 7, and Mr. Davis noted it as 6·1 in the beginning of 1871; but his subsequent observations have shown it to fluctuate between the limits 6·2° and 7·3° in a period of about 31 days.

But enough of the Uranometry, which must soon be brought to a conclusion.

With the meridian circle I have already accomplished a very fair amount of work in determining the positions of stars unidentified for the Uranometry, and of others regarding which there is discordance in the existing catalogues. The observations for latitude are completed and will give a value which can probably not be essentially improved without an investigation of the division errors of the circle. I have not yet completed their discussion, but the resultant latitude will not differ much from

$$-31° \; 25' \; 15''{\cdot}4.$$

In connection with Señor Moneta, Chief of the Corps of National Engineers, I have already carried out two series of longitude-determinations; the one with the city of Rosario, the other with Buenos Aires. With each of these places time-signals have been exchanged on several nights, and with results that indicate that Cordoba is in fact more than a minute of time to the westward of the position given it on the best maps. The telegraph across the Andes to Chile was completed about a month ago, and Professor Vergara, of the National Observatory at Santiago, is now constructing a branch line to his observatory for the purpose of effecting a series of longitude measurements with Cordoba. And since the longitude of Santiago de Chile from European meridians is doubtless better determined than that of any other point in South America, the proposed undertaking should not only give us a very trustworthy result for this observatory, but likewise improve the adopted values for Buenos Aires, Rosario and Montevideo. The value which I am for the present adopting is

Cordoba $0^h \; 51^m \; 33^s$ E. from Washington,
or 4 16 39 W. from Greenwich.

All obstacles to the commencement of the zone-work have, I think, at last been overcome, and I am already practising the preliminary drill. The last hindrance was a peculiarly vexatious one. There is no convenient place in our meridian room for the proper support of an astronomical clock, since my plan contemplated a clock mounted upon a pier in another room, and provided with a telegraphic dial on the same plan as those which I employed at the Dudley Observatory in 1857. Upon unpacking the dial and placing it in position, I found that the one sent me could only be actuated by a commutator reversing the current every second, while the clock itself was only furnished with a break-circuit arrangement. Elsewhere the embarrassment might not have been serious, but here it was otherwise. I might have sent home for another dial, or a fitting commutator, but this would have entailed a delay of at least four or five months; so, ignoring the want of local opportunities, we have been struggling since May in an attempt to construct one of the "home-made" sort, which should be sufficiently nice in its mechanical execution never to miss a second, and yet interfere as little as possible with the clock rate. This is at last accomplished, thanks to the persistent efforts of Dr. Sellack, and the dial is now performing its functions in a satisfactory manner.

With the equatorial I was able to follow the comet discovered by Tempel Nov. 3, 1871, on every clear night but one from Jan. 17 to Feb. 21, in spite of its extreme faintness. This comet was observed in Europe only for about ten days, and I think the Cordoba observations will prove to be the only ones made in this hemisphere. The comet traversed the southern hemisphere, passing within about 5° of the pole, so that it has not yet been possible to determine all the comparison stars with the circle; but as soon as possible I hope to complete the series of determinations. It is on such occasions that one feels most keenly the intensity of the existing need of a southern catalogue; for of all the comparison-stars employed, I succeeded in identifying only two as having been previously observed.

Among my most cherished plans in connection with this expedition has been that of obtaining photographic impressions of prominent star-clusters in this hemisphere, for measurement and computation of the same kind as that bestowed, before leaving home, upon Mr. Rutherfurd's photographs of the *Pleiades* and *Præsepe.* With this view I made an earnest but unsuccessful effort in Boston to obtain the needful means by subscription. But in December last some of my near connections supplied the necessary funds for the expenses and equipment of a trained photographer, and through the exertions and friendly care of Mr. Rutherfurd, the services of Dr. C. S. Sellack were secured for a limited period, and an adequate supply of chemicals and apparatus provided. Dr. Sellack left the United States in December, but having been separated from his boxes in Rio Janeiro did not reach Cordoba until the middle of March. As soon as possible a little photographic laboratory was constructed, and after a little less than a month the photographic object-glass was unpacked from the box in which it had remained since leaving New York. To our dismay, on unpacking the glass, the flint lens was found broken in two with a cruel fracture of irregular form, which divided it into two not very unequal segments. Since then we have been engaged in constant efforts to render it serviceable; but were it not for the extreme ingenuity and unwearied persistency which Dr. Sellack has brought to the work these endeavors must have proved fruitless. Month after month our faith and hope have dwindled, as one device after another failed to attain the end desired, or developed some new difficulty in the way; but I am so happy as to be able to say at last that the difficulties seem to be essentially surmounted, so that I do not despair of accomplishing something after all. A photograph taken three days ago gives most encouraging prospects, for we have on a single plate images of thirty-six stars in the cluster in Scorpio, and although the images of the brighter stars are slightly elongated, they are certainly good enough to afford accurate results. And as the chemicals were accompanied by the priceless gift from Mr. Rutherfurd of the beautiful micrometer which has in his own hands done such exquisite work, I cherish some hope of being able to send you the palpable demonstration that by patience and ingenuity something may be done even with a fractured lens.

The great scientific importance of a study of the singular meteorological relations of this country has made me unwilling to neglect any opportunity of furthering such investigations; although greatly indisposed to sacrifice any time which might be devoted to astronomical researches. With this feeling I have lost no opportunity of urging on the Government the high importance of an organized system of meteorological observations, and a bill is now pending in the Argentine Congress, with every prospect of a speedy passage, to establish a Meteorological Bureau, and to provide competent observers in various parts of the country with the necessary instruments. I have provisionally undertaken the organization and management of this Bureau, but with the hope of being able before long to resign it into some competent hands.

I have also undertaken the Commissionership of Weights and Measures, hoping thus to contribute something toward the furtherance of the great international movement toward the unification of weights, measures and currency. And I am glad to announce that as a preliminary step toward the practical introduction of the metric system, it has been ordered by the Government that from and after Jan. 1st, 1873, all the measurements and records of the custom houses of the nation are to be made in metric units. At present every one of the 14 provinces has its own measures of length and capacity, each differing from the others, and all differing from those of Spain, whence they were derived. It will not be a difficult matter, I am convinced, to bring the metric units into practical use throughout the country.

Of other scientific news I have but little to tell. A very beautiful meteor passed over the city of Tucuman at about 5½ A. M., on the 21st August, dazzling those who were in the streets, and alarming them not a little. It exploded with a loud report. The motion is said to have been toward the east, but no more definite information could be obtained.

We are in the midst of the tempestuous but rainless season of the year. Two or three times a week, hurricanes pass over the city, rendering the air opaque with dust and doing much injury to trees and houses. This is one of our great troubles, in consequence of the harm done to the instruments by the penetrating clouds of fine hard clay-dust. There has been no rain for many months, and the bed of the Rio Primero is dry, below the upper part of the city; a not very uncommon phenomenon.

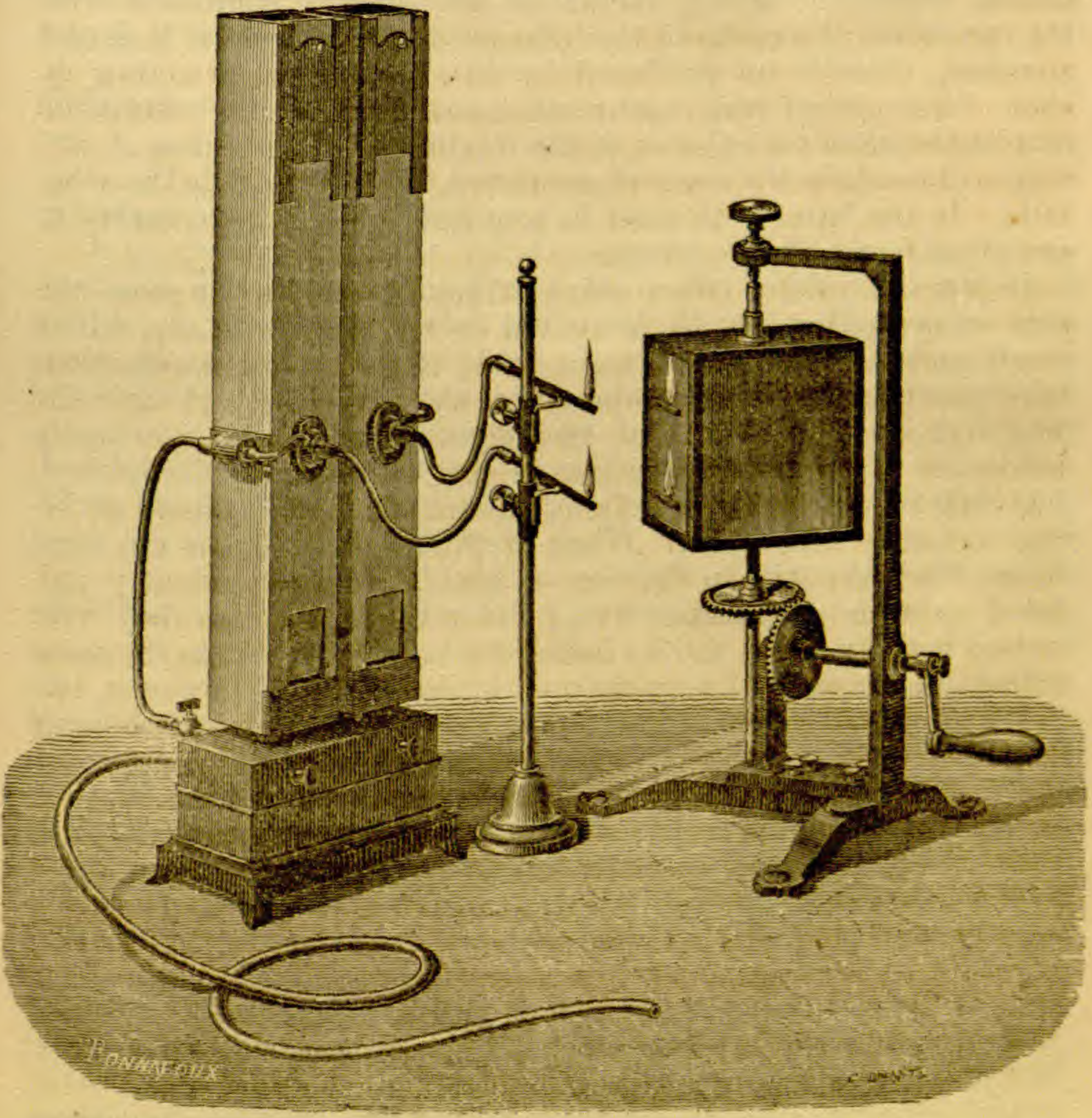
# SCIENTIFIC INTELLIGENCE.

## I. Chemistry and Physics.

1. *On Manometric Flames;* by Dr. R. König, of Paris.—[The following is a translation of the latter part of Dr. König's admirable paper "On manometric flames," recently published in *Poggendorff's Annalen*, Bd. 146, S. 165. The figures annexed are from electrotypes which accompanied the acoustic apparatus recently sent by Dr. König to the Stevens Institute of Technology. —A. M. Mayer.]

*Interference Phenomena.*—In describing the results obtained by the combination of the notes of two organ pipes, I did not make mention of unison. The combination of two notes in unison is of special interest on account of the communication of the vibrations and the attending phenomena of interference. I therefore prefer to describe them in connection with other similar experiments.

1.



If we take two organ pipes in unison, attach to them two flames, and sound one of them, the flame of the other will show that the included column of air has had the vibrations communicated to it, and that this communication continues even when the organ pipes are no longer exactly in unison, but give beats when sounded together. We must remark, however, that in this case, the second pipe has no vibrations of its own, but only such as are exactly in unison with the first, so that the beats can neither be heard nor observed in the flame. If we now sound the second pipe also, and thus induce vibrations of its own, these will combine with the resonance vibrations, and the flame violently indicates beats which can also be distinctly heard.

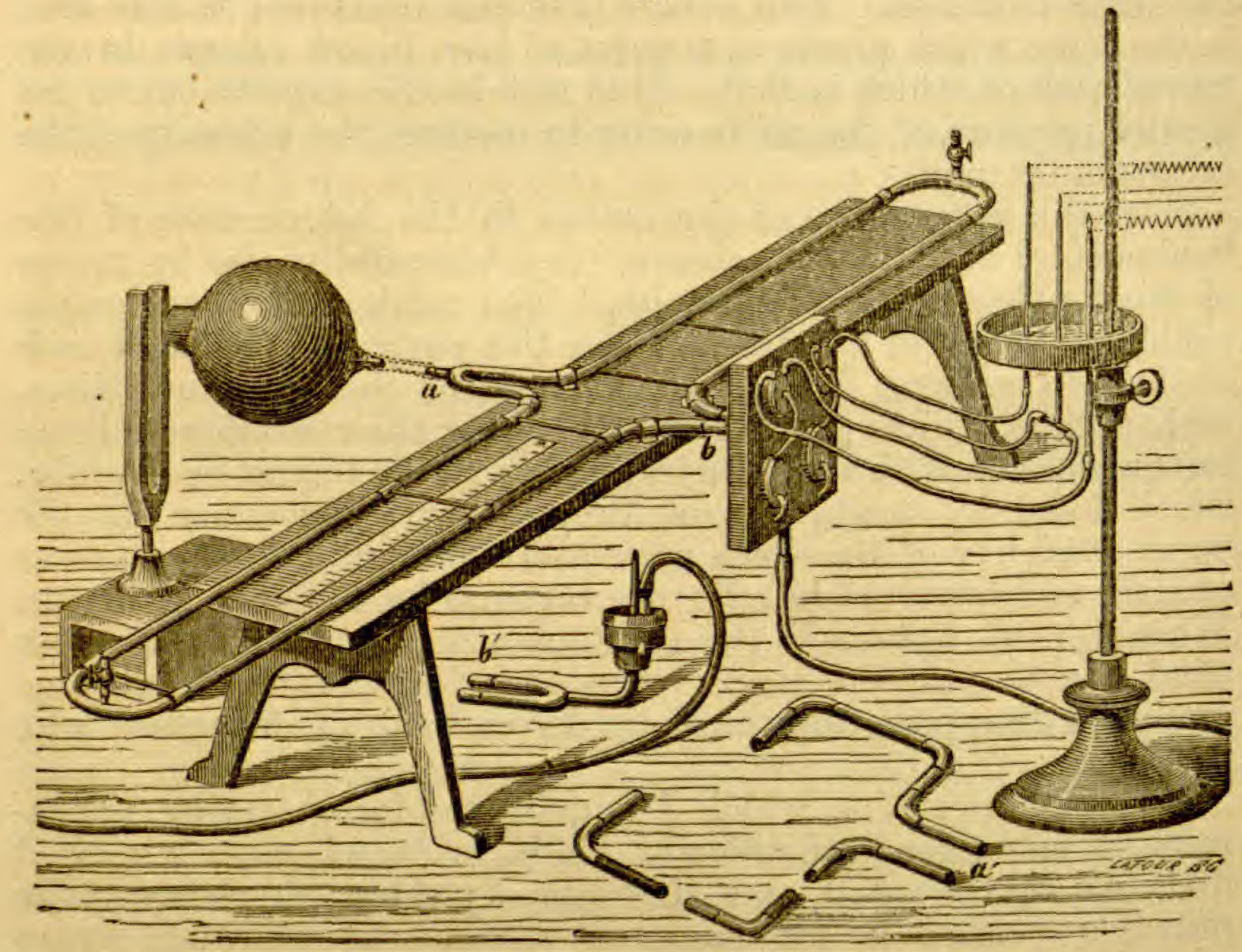
I draw particular attention to this isolated appearance of resonance vibrations in the column of air, because this does not happen in the case of two violin strings stretched over the same sounding board, where the string vibrations are always combined with the resonance vibrations in the influenced string, even if it is not sounded. The beats produced by two such strings acting on each other are of such nature that one reaches the maximum amplitude while the other is at the minimum. The flames of two organ pipes show the same phenomenon, one rising while the other falls. In the latter both must be sounded, while it is necessary to sound only one of the strings.

In pipes of perfect unison whose vibrations make the same mutual compensation that the beats did before, that is to say, where condensations take place in the node of one while rarefactions take place in the node of the other, we can plainly observe the whole process by means of two flames, one placed vertically below the other. The vibrations of the flames are undiminished, but their images in the revolving mirror alternate instead of being one under the other. When both notes act upon the same flame, the flame will, in the case of beats, be more violently agitated than the two flames were; for in the latter case they were caused both by direct and by induced vibrations, which in the same column of air were of very unequal intensity; here, however, two notes of almost equal intensity are produced directly in two equal columns of air. If the two notes are made gradually to approach unison we shall observe that we cannot retard at will the beats as in the case of tuning forks; but when we come to a certain point they suddenly cease, and the two columns of air vibrate like a system, that is, like two differently tuned bodies which are so intimately connected, and act upon each other so strongly, that neither can emit its peculiar note; the consequence being that but a single note, a mean between the two, is heard. This note is stronger than that of a single organ pipe and causes the flame to contract in the center, and to rise above a non-luminous blue broad base. As we approach pure unison the height of this dark base increases, the luminous contraction disappears and when unison is reached the flame appears at rest. At the same time the strong fundamental note of the organ pipes has almost entirely disappeared, and we clearly perceive the first overtone, since, as we well know, the even overtones are strengthened and the uneven ones destroyed whenever the difference of half a phase of vibration occurs in the two notes in unison. This octave [the first overtone] is also seen in the flame which produces a series of low broad images in the mirror each of which is cleft. It is well in this experiment to use greater pressure of the air in order to increase the intensity of the octave in the pipe.

Since this appearance of the octave in the interference of two fundamental notes may be shown very beautifully also by means of the double syren of Helmholtz, I also made this phenomenon visible by means of the flames. For this purpose I provided each of the two resonant boxes, which are over the revolving discs, with a tube which permitted me to bring their interior in direct communication with the tube leading to the manometric capsule; which I did by means of gum tubes in such a manner that the upper wind box of the syren preserved a limited mobility so as to produce and interrupt interference through its different positions. Whenever we approach the point of interference of the upper wind box we see the large vibrations of the fundamental tone gradually disappear, and the short cloven flame of the octave take its place.

A special apparatus, which I constructed for observing phenomena of interference of different kinds, is based upon a method first used by Herschell, and after him by other physicists. The principle consists in producing interference by allowing waves coming from the same source of sound to traverse two different directions, differing in length by half a wave-length, and then uniting them again. A tube is used branching off in two directions, the branches being so constructed as to allow lengthening and shortening. (Fig. 2). In order to obtain a very complete interference, we must introduce as simple a note as possible into the tube by connecting it with a resonator before which the corresponding tuning-fork is sounded. If we now lengthen one of the arms until the difference between them amounts to half a wave-length of the note of the tuning-fork, the waves coming through the two conduits destroy each other at the other end of the tube, and if we allow this to terminate in a small cavity, above which is a manometric capsule, we shall see, on lengthening one of the arms, how the deeply cloven flames in the revolving mirror are gradually changed into bands of light when the difference of half a wave-length is reached. Interference may be shown much more beautifully by means of another arrangement. Instead of allowing the united arms to act upon the capsule, I apply to the ends of them a small apparatus connecting them with separate capsules. These two capsules, whose mutual action is canceled by the two auxiliary capsules, are connected with two gas pipes instead of one. On a stand are three burners of different heights, the middle one being provided with two gum tubes. I now connect one gas pipe of one capsule with the highest burner, one pipe of the other capsule with the lowest, and by means of the other two pipes I connected both capsules with the middle burner. If I now

2.



sound the tuning forks, both branches being of equal length, the three flames in the revolving mirror appear as three series of flames cloven to an equal depth one above the other. On lengthening one branch half a wave-length of the note, the middle one alone becomes a simple band of light, while the other two continue to vibrate with unchanged intensity. Here we observe at the same time the effect produced by waves of sound coming through each arm separately and when they are reunited.

If in these experiments we use an open organ pipe, instead of a tuning fork with resonator, the vibrations of the octave appear again during the interference of the waves of the fundamental note; provided the organ pipe is not of too great diameter. In the same manner as the fundamental note we can also remove any overtone of a note by means of interference. This can be shown very nicely by means of a closed pipe. I introduce its sound into the apparatus by connecting it to the latter by means of gum tube attached to its terminating capsule, after removing the gas burner. If I then pull out the tube so far that interference is produced for the note 3, the middle flame in the mirror will show the simple flames of a fundamental note, while the other two will show the same image as in the combination of the notes 1 and 3. In the same manner we can separate whole series of overtones from the vowel sounds, and this forms a very fruitful method of studying these sounds. In these experiments the arrangement of the three flames is particularly useful, because the constant images of the upper and lower flames render the slightest variations of the middle flame very perceptible. If U [German = oo] is sung to the note $\bar{c}$ in the apparatus, the fundamental tone is but weakly accompanied by the octave; if we then arrange the apparatus so that the waves of $\bar{\bar{c}}$ interfere, every trace of this octave disappears; while on interference of the fundamental note two narrow flames of almost equal height take the place of each broad one, which represents the octave which exists now almost alone. If we sing O to the same note, where the octave is stronger than with U, we can make the same experiments, but here the tone 3 appears on interference of the octave, the broad flame of the fundamental note changing into three points successively diminishing in altitude. If the waves of the octave interfere we get a group of five peaks of flame which indicate the tones 1, 3, 5. If we suppress the fundamental tone and with it the tones 3, 5, etc., we get a simple series of flames caused alone by the octave. These phenomena are not, however, always so simple, especially in more composite groups of flames pertaining to the lower notes. I will therefore remark, that on lengthening one tube of the apparatus we often suddenly see very considerable changes in the images of the flames, while it is between the points of interference of two successive overtones of the note. It is then the point of interference of the lower octave or 12th of a higher overtone of the note which is separated in this manner.

Instead of the branching tube, in which the sound was introduced in the preceding experiment, we can use two separate tubes exactly equal in length and shape, each of which consisting of three pieces united as in a telescope, so that the two open ends may be turned in any direction without change of length or curvature. This apparatus allows a sound, proceeding from two parts of the vibrating body to be introduced into the apparatus; as, for instance, from parts of a plate having different signs, or from the two sides of the same part. In these two cases interference takes place on passage of the sound through the two conduits of equal length, and the sound appears only when interference is destroyed by pulling out one arm. In order to make the apparatus do for determining the wave-length of a tone in different gases, and for the experiments of Zoch, I have provided the tubes with two stop-cocks by which they may be filled and emptied. When we experiment with a gas other than the atmospheric air the resonator cannot of course remain connected with the interior of the tube, and we must therefore introduce a small hollow vessel separated in the middle by a membrane. One half is connected with the tube the other with the resonator. Besides this we must cover the ends of the tubes with india rubber rings to prevent the gas from escaping at these points.

Of course this apparatus will do for direct observation of the different phenomena of interference with the ear, and for repeating the experiments of Mach, Quincke and others. For this pur pose we must put in the place of the flame apparatus one of the forked tubes and connect this with the ear by means of an india rubber tube.

2. *On the light emitted by the phosphorescent compounds of uranium.*—Becquerel has examined the phosphorescence spectra of some of the compounds of uranium, and has arrived at the following results:

(1.) The compounds of protoxide of uranium hitherto studied (chloride and sulphate) did not exhibit any appreciable phosphorescence. But although some compounds of the sesquioxide are equally inactive, this is not the case with the greater number, which when properly treated give rise to a more or less vivid emission of light.

(2.) The greater number of these phosphorescent substances give a series of groups of luminous and dark bands which appear in a part of the spectrum extending from about C to beyond but near F. These groups are 5, 6 or 7 in number, and the bright and dark bands formed by them are not in the same places for the different compounds, but preserve the same positions in the case of the same substance.

(3.) If the succession of luminous groups in the spectrum characterizes in general the compounds of uranium, the acid in the compound determines the disposition of the bright and dark bands of each group, which disposition may differ greatly for the different compounds.

(4.) In the double salts of the same class, in the sulphates and double sulphates for instance, the composition of each group remains the same, but the index of refraction of the corresponding bright and dark bands is different. Sometimes these groups are transferred a short distance from the more refrangible or from the less refrangible side, according to the simple or compound uranic salt. Thus in the case of the double chloride of uranium and ammonium, the lines or bands are a little more refracted than the corresponding lines or bands presented by the double chloride of uranium and potassium, while the contrary is the case when we compare the double sulphates of the same bases. With the double oxalate of ammonia and uranium, the groups similar to those which the simple oxalate gives are less refracted than in the case of this last, and the difference is greater than that observed with the sulphates.

(5.) If we consider the characteristic lines or bands of each group in the same compound (which may be the center of a bright band or a dark line), we find that from the first group to the seventh, the distance measured by the aid of the micrometer of the spectroscope increases with the refrangibility; on the other hand, the differences between the wave-lengths of the corresponding luminous rays diminishes; the ratio of these differences to the mean wave-lengths also diminishes, but the ratio of these same differences to the squares of the mean wave-lengths changes little, changes for the same compound between the extreme degrees of refrangibility, and may be regarded as sensibly constant. Moreover, with different compounds this ratio only varies between limits but little removed from each other. Thus we have for the mean value of this ratio:

| Substance. | Ratio $\frac{d}{\lambda^2}$ |
|---|---|
| Chloride of uranium, | 0·000081 |
| Chlor. of uranium and potassium, | 83 |
| Fluor. of uranium and potassium, | 81 |
| Sulphate of uranium, | 85 |
| Sulphate of uranium and potassium, | 84 |
| Oxalate of uranium, | 86 |
| Phosphate of uranium and lime, | 82 |
| Nitrate of uranium, | 88 |
| Arsenate of uranium, | 83 |

(6.) There does not appear to be any simple relation between the wave-lengths corresponding to the homologous lines or bands of the same luminous group in different compounds and the chemical properties of these substances.

(7.) When we illuminate the solid compounds of uranium with transmitted violet or ultra-violet light, we observe in the most refrangible part of the spectrum groups of absorption bands which differ for each compound, and which appear to correspond in this part of the spectrum to less refrangible groups of bright phosphorescent bands, and to continue their succession.—*Comptes Rendus*, lxxv, 296. W. G.

3. *On the spectrum of the Aurora Borealis.*—VOGEL has made an attempt to identify the spectrum of the aurora with that of air, and has arrived at results which, if not absolutely conclusive, render the identification at least probable. The author employed a direct vision spectroscope with 5 prisms, collimator and observing telescope, which last by means of a micrometer screw could be moved so as to bring different parts of the spectrum into the center of the field of view. The distances between the spectral lines could be read off in fractions of a revolution of this screw. By repeated measurements of about 100 lines, aided by Angström's atlas, the micrometer readings could be converted directly into wave-lengths. With this instrument Vogel determined the wave-lengths of 7 lines with the following results:

| Wave-length. | Probable error. | Remarks. |
|---|---|---|
| 0·0006297 | 0·0000014 | Very bright band. |
| 0·0005569 | 0·0000002 | Brightest line. |
| 0·0005390 | | Very faint line. |
| 0·0005233 | 0·0000004 | Pretty bright. |
| 0·0005189 | 0·0000009 | Bright where the red line appears; otherwise faint. |
| 0·0005004 | 0·0000003 | Very bright. |
| 0·0004694<br>0·0004663<br>0·0004629 | 0·0000003 | Broad band somewhat less bright in the center; very faint where the red line appears. |

For the purposes of comparison the author determined the wave-lengths of the positive lines in oxygen, hydrogen, nitrogen and air, employing for this purpose Geissler tubes, the discharge being that of a weak inductorium. The spectra of both the narrow and the wide portions of the tubes were observed both as regards wave-length and intensity of light, and finally the spectrum of rarefied air saturated with aqueous vapor was also noted. The first auroral line ($\lambda = 629{\cdot}7$) appears to correspond with one of a system of lines in the spectrum of nitrogen, the wave-lengths of which range from 662·0 to 621·3. The brightest line in the spectrum of the aurora ($\lambda = 556{\cdot}7$) is found in the spectrum of nitrogen as a faint line. The line ($\lambda = 523{\cdot}7$) exists both in the spectrum of nitrogen and in that of air. The fifth line of the aurora corresponds with the third line in the spectrum of oxygen ($\lambda = 518{\cdot}9$). The sixth line of the aurora corresponds very accurately with the nitrogen line seen in the spectra of certain nebulæ ($\lambda = 497{\cdot}5$). Finally the broad band of light between 469·4 and 462·9 corresponds to several lines in the spectrum of nitrogen as well as in that of air. As the general conclusion to be deduced from his work, the author believes that the spectrum of the aurora may with great probability be regarded as a modification of the air spectrum, the variability of the spectra of gases under different circumstances of temperature and pressure being well established.—*Pogg. Ann.*, cxlvi, 569. W. G.

4. *On the heat of expansion of solid bodies.*—In the cases of a number of different solids and also of water, BUFF has compared the quantity of heat absorbed in producing expansion with the whole quantity absorbed, or, in other words, with the total specific heat. The author sets out with the extremely probable assumption that the coefficients of expansion and compression are equal, whether the expansion or compression be linear or cubic. A simple calculation, then, shows that in the case of iron, as an example, the quantity of heat necessary to raise the temperature of one cubic centimeter of iron, 1°·374 C., corresponds to a work of 100 kg. × 0·0000481 centimeters, or 4·81 centimeter-grams. This quantity of heat is found in units by multiplying the weight of 1 c. c. of iron, 7·757 gr., by the specific heat of iron, 0·1098, and by the increment of temperature, 1·°374 C., which gives 1·17 units. The total work which this quantity of heat is capable of producing is 42000 gr.-cm. × 1·17 = 49140 centimeter-grams, while the actual work of expansion is only 4·81 centimeter-grams. Hence the heat of expansion is to the total heat as 0·98 is to 10,000, or about 0·01 %. The following table gives the author's results, with the different substances to which the calculation was extended:

| | $a$ | $\beta$ | $\delta$ | $s$ | $\lambda$ |
|---|---|---|---|---|---|
| Iron, | 0·0000481 | 0·0000350 | 7·757 | 0·1098 | 0·980 |
| Copper, | 0·0000951 | 0·0000515 | 8·936 | 0·0949 | 1·446 |
| Silver, | 0·0001401 | 0·0000573 | 10·301 | 0·0577 | 2·378 |
| Gold, | 0·0001791 | 0·0000466 | 18·035 | 0·0324 | 1·899 |
| Platinum, | 0·0000628 | 0·0000265 | 21·166 | 0·0324 | 0·920 |
| Lead, | 0·0005634 | 0·0000854 | 11·165 | 0·0314 | 5·800 |
| Glass, | 0·0001451 | 0·0000262 | 2·446 | 0·1770 | 1·441 |
| Water at 16° C., | 0·0045854 | 0·0001600 | 0·999 | 1·000 | 3·810 |

In this table the column $\alpha$ gives the cubic coefficient of extension referred to the millimeter as the unit of length; column $\beta$ gives the cubic coefficient of expansion for 1° C.; column $\delta$ gives the density of the body; $s$ its specific heat, and $\lambda$ the quantity of heat which becomes latent in ten-thousandths of the whole quantity. It will be seen that it forms a very small fraction of the whole. It is thus easy to understand why it has not yet been possible to raise the temperature of a solid by compression. We also see that the latent heat of expansion exerts a very small influence on the specific heat of the atoms of solid bodies.—*Pogg. Ann.*, cxlv, p. 626. W. G.

## II. GEOLOGY AND NATURAL HISTORY.

1. *Wyoming Coal Formations.*—Prof. E. D. COPE describes, in an interesting paper read recently before the Am. Phil. Soc. Philad., a large *Dinosaurian*, discovered during the past summer by Mr. Meek and Dr. Bannister, at Black Butte Station, on the Union Pacific Railroad, in Wyoming Territory. In this, as well as in a later paper published in the *American Naturalist* on the age of the Wyoming coals, Prof. Cope remarks, that the determination of the affinities of this Saurian proves that these coals, which hold a lower position, belong to the Cretaceous age, and not to the Tertiary, and he writes as if all others had been in error on the age of the deposits. Prof. Cope was doubtless not aware that Mr. Meek had, in 1871, referred Dr. Hayden's collections from this formation on Bitter Creek, at Point of Rocks, to the Cretaceous;* and that this same careful paleontologist had also referred the coal-bearing rocks of the same great series at Coalville, Utah, and at Bear River City (Sulphur Creek), Wyoming, to the Cretaceous in 1870, as did also Mr. King and Mr. Emmons.† Indeed, as long back as 1860, Mr. Meek, in connection with Mr. Engelmann, referred Capt. Simpson's collections from these rocks, including the coal at Sulphur Creek, Wyoming, to the Cretaceous.‡ Prof. Marsh had also referred coal beds on Brush Creek, Wyoming, to the Cretaceous in 1871.‖ Two fossils, and only two, from one part of the formation mentioned, were referred by Mr. Meek to the Tertiary, but this was from a misapprehension in regard to the locality and stratigraphical position of the Hallville coal mines, which, we are informed, he had then never visited, and supposed to be located 20 to 30 miles farther eastward, and at a much higher horizon; and these fossils, the only species found at Hallville, are just such forms as might be either Tertiary or Cretaceous, and are nearly allied to Tertiary species of Europe.

2. *Decaisne's Monograph of the Genus Pyrus.*—A volume of Decaisne's great work—or rather of one of his great works—*Le Jardin Fruitier du Muséum*, un Iconographie de touts les Espèces et Varietes d'Arbres Fruitiers cultives dans cet Établissement, &c.

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* Hayden's Report of 1871, p. 375.

† King's 4to Report of 1870, p. 461.

‡ Proc. Ac. N. Sci., Philad., 1860, p. 130.

‖ This Journal, March, 1871, i, 195.

(produced in first-rate style by Firmin Didot Frères), devoted to the genus *Pyrus*, is now before us. It is a complete monograph of the species of this genus, taken in its restricted sense, illustrated by figures of the wild types, and also of the cultivated races of those cider-pears known in France under the name of *Sauger*. There is a list of the cider-pears cultivated in the different provinces of France, a general alphabetical catalogue of all the published varieties of pears, and a table in which the synonyms are referred to the names severally adopted. The other volumes, and the illustrations of the edible varieties of pears, may have more interest for the horticulturist. But the present attracts the special attention of the scientific botanist.

As stated in the Introduction, Prof. Decaisne entered upon his great undertaking more than twenty years ago, when, in the year 1850, he became the Professor of Culture. He cites the instructions under which the separate collection of fruit-trees was constituted, and the professor of culture was charged with its management, and was directed to bring together all the known varieties, with all their names, "afin d'établir une uniformité de nomenclature necessaire pour toutes les parties de la République." This is a decree of the National Convention, June 10, 1793. The collection which Decaisne has so diligently and acutely studied actually dates from the year 1792, when the fruit-garden of the Chartreux of Paris was broken up, and two trees of each variety transported to the Jardin des Plantes. In 1793 it contained 185 varieties. In 1824, when Thouin died, there were in it 265 varieties of pears alone; it has now more than 1400 varieties of this fruit. It is interesting and important to know that the collection still preserves the greater portion of the very types described a century ago by Duhamel. For seven years Prof. Decaisne studied the incomparable collection under his charge, making drawings and analyses, in which he is so skillful, and an herbarium of their flowers and foliage, before he commenced the publication of the *Jardin Fruitier du Muséum*, which he is now bringing to a close.

As to giving a correct nomenclature and available characters, this is difficult enough, as all botanists know, for the species themselves (which must needs have, or be assumed to have, real distinctions) in any large genus, such as *Quercus*, *Rosa*, *Rubus*, and the like; how much more difficult, even to impossibility, it must be in the case of cultivated varieties, of ever increasing numbers, usually named without system, sometimes of mixed origin, and often too like each other to be distinguished by any available descriptions. Here colored plates are a necessity; and those of this great standard work, upon which no pains have been spared, leave little to be desired that art can supply.

In France alone they count about 800 sorts of pears; the origin of most of them is unknown, and many are undoubtedly very ancient. Indeed, according to Jordan and his school these differences are primitive, and the so-called races and varieties, both of wild and cultivated plants, represent so many closely related species. But M. Decaisne, not content with the *reductio ad absurdum* of having about 2000 species of pears to be dealt with, proceeded to an experimental demonstration of the variability of the cultivated races. He sowed the seeds from four very distinct varieties in 1853, the *Poire d'Angleterre*, the *Bosc*, the *Belle Alliance*, and the *Cirole*. Of the last the four trees raised bore fruit of four different forms. From the *Belle Alliance* he obtained, in this first generation, nine new varieties, none of them representing the parent, neither in the form, size, color, nor even the time of ripening of the fruit. The *Bosc* equally produced new varieties. Of the *Angleterre* nine trees produced as many new forms, one of them a winter-pear similar to the *Saint Germain*, another apple-shaped fruit identical with one which was raised from the *Belle-Alliance*. On plate 33, Decaisne gives figures of six different pears raised from the *Angleterre*. These results even led him to doubt the cases cited by Darwin of the reproduction of certain pears from seed. He insists, moreover, that very bad fruits may be raised from choice cultivated pears, and that good varieties may be obtained from the seeds of wild pears. The latter is not what one would expect in the first generation.

Our author proceeds to state that the trees raised from seed taken from the same fruit differed, not merely in their fruits and in the time of ripening, but no less in their flowers and in the form of the leaves. Some were thorny, others thornless; some produced slender shoots, others thick and stout shoots, &c. It is worth noticing, however, that no mention is made of any precautions to prevent cross-fertilization of the flowers from which the seeds planted were derived, which might have influenced the product through the now well-ascertained influence of the pollen upon the pericarp. We perceive, however, that he would regard this as unimportant, since pear-varieties are of the lowest grade, incapable of propagating fruit by close-fertilization, and, therefore, wholly unlikely to impress by their pollen any characteristic upon the pericarp of another variety.* A large part of the Introduction is occupied with further evidence that the Pear-trees of cultivation are all of one species, from which have proceeded six races, completely fertile *inter se*, and varieties *ad infinitum*. In this respect the Pear-tree has but followed the example of most fruit and fruit-trees, and of the Grains, &c., which had apparently diverged into

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* Yet the apple, which is in the same case, does so. An interesting instance of this kind lately came under our notice, an apple from a spitzenberg tree, one-half (at least as to the surface), *spitzenberg* the other half *russet*. A tree of the latter fruit stood about 200 yards off. Several cases of this sort are known, in which, as in this, the division is into two exactly equal parts of the circumference, and the line of demarcation abrupt. This is quite unexpected, as the Secretary of the Smithsonian Institution, who sent us the fruit, remarked; for as the styles and carpels were five, we should have expected the division to be into fifths, and according to the number of the stigmas which were acted upon by the foreign pollen. It is, moreover, to be noticed that the action of the pollen in this case is manifest upon what is morphologically the calyx, not upon the pericarp. The apple we refer to was grown in the orchard of William Wicksham, of Washington Co., Penn. A. G.

races, or distinct but closely related types, in very early times, and those under cultivation have themselves varied and subdivided more and more. Finally, M. Decaisne maintains, seemingly with good reason, that to combine into one genus the Apple, Pear, Quince, Sorb and Mountain Ash, as done by Linnæus and followed by the latest authorities, is to misconceive the laws of the natural system; that "to unite generically these plants, which differ in the character of their wood, the vernation of their leaves, their inflorescence, the æstivation of the corolla, and the structure of their fruit," logically leads to the combination of all *Pomaceæ* into one genus. He accordingly restricts the genus *Pyrus*, or (restoring the classical orthography) *Pirus*, as did Tournefort and Jussien, to the Pear proper. To the organography of this restricted genus, from the wood to the embryo, a full chapter is devoted. In the course of this the relative systematic value of characters observed is brought out. He notes that the vernation of the leaves is involute in *Pyrus*, but not in *Cydonia*, *Mespilus* and *Aria*; that the cottony-leaved varieties, no less than the smooth ones, are glabrous in the seedling stage; that all varieties of the common Pear blossom at Paris whenever, in the month of April, the mean temperature reaches about 10° Centigrade, without perceptible difference between the earliest and the latest-ripening varieties; that the æstivation of the corolla is convolute in *Cydonia*, but imbricate in the Pear, although ordinarily quincuncial in other *Pomaceæ* (but in the two diagrams of Pear-flowers on Plate A, one has the quincuncial, i. e., in our view typically imbricative æstivation of the corolla; in the other, there is only one wholly outer and one inner petal—a combination of the quincuncial and the convolute modes which often occurs, but which need not be taken as the type of imbrication); that there are two types as to size of the corolla in the common Pear, the smaller flowered type comprehending most of the cultivated varieties; that the odor of Pear-blossoms is rather disagreeable than otherwise, in contrast with those of *Malus*, which are sweet-scented. Moreover, the anthers in the Pear genus are tinged with violet; those of the Apple genus are yellow.

As to the morphology and development of the gynæcium, Decaisne reproduces in full the note which he published in the Bulletin of the Botanical Society of France in 1857. From his investigation it appears that the five carpels in their early development are free and distinct in the concave center of the flower; that at a later stage, when the concave receptacle has become much deeper, a cellular tissue develops from its base and inner face, moulds itself around and over the carpels, so as separately to envelope them, except at their inner angle, while it carries up the petals and stamens, and forms the perigynous disk upon which they are inserted; this forms the *core* or central part of the flesh of the fruit, which we have always regarded as receptacle, never ceasing to protest against the still prevalent notion (continued in the latest general works), that the cartilaginous or bony "cells" are "endocarp." But, while we were disposed to regard the whole exterior flesh as calyx, Prof. Decaisne (no doubt correctly) regards it as mainly receptacle or axis,—an *hypanthium* which in common pears is largely a hypertrophy of the peduncle, after the fashion of *Anacardium.*

In the proper Pear genus, the ovules never exceed a single pair; this should therefore enter into the generic character.

"Theophrastus had already remarked that the older the Pear tree, the more prolific, and every day's experience confirms the justice of this observation." The gritty grains or lignified cells which are so abundant in the flesh of many sorts of pears are not wholly absent from any of them. To them is due the roughish surface of the skin, as contrasted with the smooth skin of apples. It is curious to remark that Meyen, in his Pflanzen-Pathologie, considered the gritty grains to be a disease which attacked pears and quinces.

It appears that pear-growers are able to produce fruits of abnormal size by supporting the growing pear from underneath, instead of allowing it to hang on the peduncle. M. Decaisne has seen *Poires de Livre* of a kilogram, *Goulu-Morceau* of 600 grams, and a *Chaumontel* of 700 grams weight, produced in this way.

The testa of all Pomaceous seeds is smooth and more or less mucilaginous, except of a *Photinia*, in which it is reticulated. The cotyledons are accumbent relative to the rhaphe, except in a *Photinia*, *Cotoneaster*, *Pyracantha* (*Cratægus Pyracantha* Pers.), and *Eriobotrya*, in which they are incumbent. At first there is a thin layer of albumen, which disappears at maturity of the seed.

Pears are commonly grafted upon a Quince stock. But it is confidently asserted, and generally supposed, that there are more than forty varieties which absolutely refuse this union, and which are therefore managed by surgrafting upon a pear stock of a proper sort which has itself been engrafted upon the quince. But, as Prof. Decaisne remarks, horticulturists are too apt to generalize their impressions and to limit nature to the narrow horizon of their own practice. Upon the first trial of the experiment under his own observation, he succeeded with twenty of these antipathetic varieties without the least difficulty; but some (among which are the *Clairgeau* and the *Bosc*) obstinately refuse to unite with the quince stock. He naturally discredits the assertion made by Cabanis and by Downing (cited by Darwin), that when certain pears are grafted on the quince, their seeds produce trees of types different from those which they do when they are raised upon a pear stock. Decaisne found, as already stated, that pear-seeds produce indifferently new varieties in any case; that these varieties are not at all fixed into races. He regards as wholly unproven all the assertions that the fruit is ameliorated or in any degree altered by grafting upon a quince or any other stock. He records a very exceptional instance in which the antipathy of the pear to the apple as a stock was so far overcome that the graft survived at least six years, but without vigor, and bore fruit; still this antipathy confirms the generic difference between *Pyrus* and *Malus.*

We must pass over the sections on the diseases of the pear, and the parasitic plants and insects hurtful to it; while as to that on the classification of the pears of cultivation, we may mention merely the conclusion, which is, that a natural classification of pears is thus far an impossibility; and that in practice nothing better can be done than to follow the example of the older pomologists, who arranged them according to the period of ripening. A general list of the adopted names of the published varieties of cultivated pears, alphabetically arranged, fills four pages of the volume. A list of their synonyms, in which each is referred to the adopted name, fills over 12 pages! Then follows a list of pears classed according to the period of maturing, and in which the best varieties are designated.

Finally comes a botanical monograph of the genus *Pyrus*, with a full generic character, and descriptions and figures of the *races*, as he would term them, considering as he does all known forms of the restricted genus as a single and very polymorphous species.

The six races are: 1. The Celtic, *Proles Armoricana*, of three quasi-species, *P. cordata*, *Boissieriana* and *longipes.* 2. The Germanic, *Proles Germanica*, or *Pyrus communis*, including our common pears, both pear-shaped and apple-shaped, "both forms being often met with upon the same tree." Under this head Prof. Decaisne gives some interesting pages upon the history of the cultivation of pears in France, which cannot be ancient, and of cider (perry) as a drink. It appears that it took the place of beer in the north of France in the fifteenth century or later, and is now giving way to wine and perhaps beer again; and that pears would have disappeared before this from a part of Normandy, were it not that *they are carried in immense quantities to Epernay, where they are used in the manufacture of champagne.* 3. The Hellenic Race, which comprises *P. parviflora* and three other subspecies. 4. The Pontic Race, *P. salicifolia* and its allies. 5. The Indian Race, *P. Pashia* and its relatives. 6. The Mongolian Race, *P. Sinensis* and its varieties. As one turns over the excellent plates one can hardly be persuaded that such extremely diverse forms can practically be regarded as of one species.

A list of the species remanded from Pyrus to other genera shows that the result of our author's prolonged and sagacious study is to increase the genera about as much as he diminishes the species of the Linnæan Pyrus. A. G.

3. *Botanical supplement to the fifth Annual Report of the U. S. Geological Survey of the Territories for* 1871; by M. LESQUEREUX.—This supplement by M. Lesquereux (prepared in May, 1872) contains the description of a number of species of fossil plants from specimens which were received when the report was already in print. The essential points marked by the author as resulting from the examination of these plants are as follows:

1st. It adds to our list of fossil species of the Tertiary 20 new forms, and describes 21 others known already from the Miocene of Europe, but not as yet observed in our Tertiary flora. The number of its species is thus increased to 231.

2d. It fixes the geological horizon of three localities in different stages of the Tertiary and marks the location of a group of specimens of as yet unknown origin.

3d. It more distinctly points out the relation of some important strata for ascertaining contemporaneity or difference of age.

4th. It indicates more positively modifications in the characters of the Tertiary flora of the North American continent, according to climatic differences at different degrees of latitude, and at the same time, it recognizes identity of the characters of this vegetation at wide distances under the same latitude.

5th. It shows a more intimate relation between the present flora and that of the Tertiary by the discovery of new types identical in both.

This relation is especially indicated by the fossil plants of Green-River station, which from their more recent facies are referable to the Upper Miocene. Among species of *Salix*, *Myrica*, *Ilex*, and *Rhus*, whose representatives are intimately related to species of our time, the fossil flora of Green River has an *Ampelopsis* and a *Morus* which by their marked affinity indicate in the Tertiary the origin of our now so predominant and widely distributed Virginian Creeper and Red Mulberry.

## III. ASTRONOMY.

1. *Elements of Alceste;* by Prof. C. H. F. PETERS. (Editorial Correspondence, dated Litchfield Observatory of Hamilton College, Clinton, N. Y., Nov. 9, 1872).—The following elements of *Alceste* (124) have been computed from observations of Aug. 23, Sept. 22, Oct. 21:

Epoch: 1872·0 Berlin mean time.

$M_0 = 22^\circ\ 28'\ 1''{\cdot}94.$ $\varphi = 4^\circ\ 26'\ 58''{\cdot}80.$

$\pi = 346\ \ 59\ \ 47{\cdot}82 + 50''{\cdot}236,\ t.$ $\mu = 834''{\cdot}47.$

$\Omega = 186\ \ 19\ \ 39{\cdot}65 + 52{\cdot}573,\ t.$ $\log a = 0{\cdot}419063,\ t.$

$i = 2\ \ 55\ \ 47{\cdot}48 - 0{\cdot}455,\ t$—counting $t$ in Julian years from 1872·0.

## IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Analysis of the Meteoric Iron of Los Angeles, California;* by Dr. C. T. JACKSON, State Assayer of Massachusetts. (Communicated to one of the editors).—Having received from Mr. E. N. Winslow a slice of the recently found meteoric iron of Los Angeles, have made a chemical analysis of it, which I now communicate to you. The original mass is stated to have weighed 80 lbs. The slice I received weighed 30 grams. Its specific gravity is 7·9053.

It shows, when acted upon by dilute nitric acid, innumerable scales of Schreibersite, but not the usual figures.

In the chemical analysis I found in the insoluble matter, on reduction by blowpipe, a minute globule of tin. The iron was separated by succinate of ammonia and the nickel by pure potassa.

The following are the results of the analysis per cent.

| | |
|---|---|
| Metallic iron, | 80.74 |
| Metallic nickel, | 15.73 |
| Metallic tin, | 0.01 |
| Phosphorus and other undetermined matters, | 3·52 |
| | 100.00 |

This analysis, although not quite complete, shows beyond doubt the meteoric nature of the Los Angeles iron.

Boston, Sept. 26, 1872.

2. *Tables and Diagrams relating to non-condensing Engines and Boilers ;* by W. P. Trowbridge, Prof. of Dynamic Engineering, Sheffield Scientific School of Yale College.—This publication gives the results, in tables and diagrams, of a laborious and costly series of experiments made at the Novelty Iron Works, New York, during the time when Prof. Trowbridge was Vice President of the Novelty Iron Works, to determine practically the expenditure of steam for different degrees of expansion in the non-condensing engine. The experiments occupied several months, and were made with an engine and apparatus constructed especially for the purpose.

An extended list of engines is published, embracing powers from 5 horse-power to 350 horse-power, amounting in number to over 1000, and embracing all combinations of pressure of steam, speed of revolution, and degree of expansion. For each engine in the list the amount of water or steam required per hour for each horse-power is given and also the total expenditure of steam per day, and the cost of the power per year.

Corresponding with this list, a table of steam boilers, is given with the amounts of water which each boiler will evaporate in one hour. A discussion of the questions of horse-powers of boilers, and of boiler explosions, safety valves, &c., is added. A new and very simple improvement to the safety valve, suggested by the author, is also given.

The work throughout is intended to be practical but at the same time it adds to existing knowledge of the subjects discussed the results of the latest reliable experiments.

3. Bloxam (Chas. Loudon): *Chemistry, Inorganic and Organic, with Experiments.* Second Edition (American reprint). Lindsay & Blakiston. Philadelphia, 1872. 8vo, pp. 666.—Bloxam's Chemistry has been a familiar and valued laboratory and lecture-room companion for several years. The present edition (July, 1872), differs from its predecessor in being adapted to the atomic notation, which has required a considerable recast of the old text. The illustrations of apparatus are very good, embracing many which are original with the author. The teacher finds on almost every page some experimental suggestions which enable him to give force and attractiveness to his prelections. The text is written in simple language, as devoid as possible of technicalities, and designed to lead the learner on by easy steps in a knowledge of chemical principles. The author's position as a

lecturer in the Royal Military Academy at Woolwich has very properly led him to give more than usual prominence to those subjects which are of interest to the military student, as, combustion and fuel, gunpowder, gun-cotton and other explosives, the process of bread making, glass, pottery, the chemistry of building materials, and kindred subjects.

4. *Report of the Mt. Uniache, Oldham, and Renfrew Gold Mining Districts*, with Plans and Sections; by HENRY YOULE HIND, M.A. Made under instructions from the Hon. Commissioner of Public Works and Mines. 62 pp. 8vo. With maps and sections. Halifax, N. S., 1872.

5. *Astronomical Engravings, by the Observatory of Harvard College.*—Fourteen plates, announced on page 243 as in course of preparation for publication, have been issued. They are views of Jupiter, craters of the moon, the sun's protuberances, the sun in the total eclipses of August, 1869, and of December, 1870, etc.; and are very beautiful. The series will consist of thirty plates. Subscriptions are solicited; the charge for the series is ten dollars.

### OBITUARY.

JOHN F. FRAZER, Professor of Natural Philosophy and Chemistry in the University of Pennsylvania, died on the 12th of October, in the sixty-third year of his age.

Prof. Frazer was born in 1809 in the vicinity of West Chester of this State. His grandfather, General Persifor, took a prominent part in the leadership of the American Revolutionary troops at the battle of Brandywine. In his early youth he attended the celebrated military academy of Captain Parrott, and here acquired the military taste that clung to him through life, which exhibited itself in his membership for a long time of the 1st City Troop. He marched with this organization to suppress the riots of '44 in this city, and also was present as a soldier in the celebrated "Buck-shot" war at Harrisburg. He was educated and graduated in the institution whose chair of physics he so nobly filled for thirty years. During his student days he was the favorite scholar of Dr. Samuel Wylie, professor of Latin and Greek in the old university, and spent some time in his family after graduation. He assisted Dr. Robert Hare in the laboratory, and subsequently became a most intimate friend and valued companion of that distinguished contemporary and rival Prof. Silliman, of Yale, the two being the most illustrious chemists of their day. At the same time Prof. Frazer was also the assistant of Prof. Alex. Dallas Bache, of the Coast Survey, during his observation at the then new physical observatory for magnetic phenomena at Girard College. In both these instances he was a voluntary assistant.

In 1836 he became the assistant of Professor Henry D. Rogers, one of the first geologists of the country, and Professor Booth, of the Mint, in the geological survey of Pennsylvania, ordered by the Legislature. Professor Frazer resigned this position as First Assistant State Geologist after one year, and then took up the study of law in the office of Hon. Wm. M. Meredith. Having concluded his law studies, he realized the cherished dream of his youth in his elevation under the regime of Provost Ludlow to the chair of natural philosophy and chemistry in the academical department of his *Alma Mater.* This position he filled with the highest honor to his college and to himself for over thirty years.

Soon after assuming the gown of the professor, he became the able editor of the well-known scientific *Journal of the Franklin Institute.* He resigned his editorial duties in 1866 in consequence of ill health, and crossed the ocean for a two years' tour on the Continent. He resided in Naples for the first year, and then traveled extensively until he returned to his college duties in the summer of 1868, Professor Morton, now president of the Hoboken Institute, having occupied his chair while away.

Professor Frazer's study was the center of physical science in the city, and was the rendezvous for the most distinguished scientists in this and foreign countries when they visited Philadelphia. He was known throughout Europe by his constant writings as most accurate in science and universally learned in his department. He was a member of the Academy of Natural Sciences of this city, and also one of the four secretaries of the mother society of science in this country—the American Philosophical Society of this city.

Although holding the chair of chemistry and natural philosophy, his favorite science was that of mathematics as applied to mechanics. This branch brought out his close study and keen appreciation of all the basic laws of mechanics, and raised him to a deservedly high rank as a transcendental mechanician.

Professor Frazer had a remarkably genial disposition, was full of life and humor among his fellows, and though brusque in manner and decided in his intercourse with his pupils, he was generally liked by all who came in contact with him. He was bold and outspoken in all his feelings, being an ultra Democrat—in its original sense—in his principles. His self-sacrificing spirit was a remarkably noticeable point in his character, and, in addition to the respect and confidence which his great learning compelled, gained him the love of all whom he could benefit in his walk through life.—*The Press*, *Philadelphia*, Oct. 14.

Notes of an Ornithological Reconnoissance of portions of Kansas, Colorado, Wyoming and Utah; by J. A. Allen, 72 pp. 8vo. July, 1872. Being No. 6, vol. III, of the Bulletin of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass.

Problem of Rotary Motion presented by the Gyroscope, the Procession of the Equinoxes and the Pendulum, by Brevet Maj-Gen. J. G. Barnard. 48 pp. 8vo. No. 240 of the Smithsonian Contributions to Knowledge.

Intermembral Homologies. The Correspondence of the Anterior and Posterior Limbs of Vertebrates; by Burt. G. Wilder, M.D., Prof. Comp. Anat. and Zool. in Cornell Univ., Ithaca, N. Y. 88 pp. 8vo. From the Proceedings of the Boston Soc. Nat. Hist., vol. xiv.

# INDEX* TO VOLUME IV.

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* The Index contains the general heads, Botany, Geology, Mineralogy, Zoology, and under each the titles of Articles referring thereto are collected.

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## ERRATA.

Vol. ii, p. 180, *for* "(near Chatham Four-corners)" *read* "(near Galesville, Washington Co., N. Y.)."

Vol. iv, page 53, 8th line from top, *for* "particularly" *read* "partially."
" " 123, last line, *for* "*Mastodon anceps*," *read* "*Tinoceras anceps*."
" " 185, line 18 from top, for *district* read *distinct*.
" " 242, " 10 " " for *tilted* read *silted*.
" " 323, 20 lines from bottom, for "very small" read "narrow."
" " " 13 lines from bottom, for "but five" read "six."
" " 370, 19 lines from foot, before "Trenton," insert "Quebec."
" " 389, line 10 from top, for *ventral* read *nodal*.
" " " " 12 " for $UT_8$ read $UT_3$.
" " 451, " 9 " for 500 read 300.

# APPENDIX.

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## *Summation of Series by Approximative Fractions.*

### By R. J. Adcock.

This method of summation of series consists in putting the series into a continued fraction by common algebraic division, and then finding its approximating fractions until one is found which gives the sum of the series either approximately or entirely.

*Ex.* Find the sum of the series $a + ar + ar^2 + ar^3 +$ &c.

$$\begin{array}{llll}
a+ar+ar^2+ar^3+\&c.) & 1 & \left(\frac{1}{a}\right. & \\
 & \underline{1+r+r^2+r^3+\&c.} & & \\
 & -r-r^2-r^3-\&c.) & a+ar+ar^2+ar^3+\&c. & \left(-\frac{a}{r}\right. \\
 & & \underline{a+ar+ar^2+ar^3+\&c.} & \\
 & & \text{no remainder.} &
\end{array}$$

Then

$$a+ar+ar^2+ar^3+\&c.=\frac{a+ar+ar^2+ar^3+\ \&c.}{1}=\frac{1}{\frac{1}{a}+\frac{1}{-\frac{a}{r}}}=\frac{a}{1-r},$$

which is the entire sum of the series and the exact algebraic expression from which it is derived. To find the sum of $n$ terms, it is to be observed that beginning with the $(n+1)$th term, the series is $ar^n+ar^{n+1}+ar^{n+2}+$ &c. $=$ by the same process, $\frac{1}{\frac{1}{ar^n}+\frac{1}{-ar^{n-1}}}=\frac{ar^n}{1-r}$. Hence $a + ar + ar^2 + ar^3 + \ldots\ldots ar^{n-1} = \frac{a}{1-r}-\frac{ar^n}{1-r}=\frac{a(1-r^n)}{1-r}$, the common formula for the sum of $n$ terms of a geometrical series.

The superiority of this method of summation over others is:

First, its comparative simplicity, on account of which it is worthy of a place in common algebra.

Second, it is more generally applicable than any method known to me.

Third, the facility with which it gives the entire sum of a series when capable of being expressed in finite terms, and the rapidity with which it approximates to the sum of those capable of being expressed only approximately.

## *Equilibrium of a fluid mass in the form of an ellipsoid rotating about its shorter axis;* by R. J. ADCOCK.

A FLUID mass, in the form of an ellipsoid, rotating about its shortest axis, under the action of the attraction of its own particles and their centrifugal forces, is in equilibrium; and this is the only form of equilibrium.

In my work on Gravitation, and in a paper* presented at the Dubuque meeting of the American Association for the Advancement of Science, it is shown that the ellipsoid is a form of equilibrium, and the expression found, for the force at any point of its surface. It remains, then, only to demonstrate the last part of the proposition, that the ellipsoidal form is the only one of equilibrium, under the given conditions.

The proof of this depends upon two propositions.

*First.* The attractions of two similar bodies for any two exterior points similarly situated, are as their similar dimensions, and consequently as the distances, of the attracted points from the centers of the attracting bodies.

*Second.* Ellipsoidal shells which have their outer and inner surfaces concentric, similar, and similarly placed, are the only ones which have their attractions for an interior point zero. This follows from formula (1) of Gravitation, and the proposition that these shells are the only ones which have the distances through them in opposite directions from any point within equal, as is shown in problem 4th, page 211, Senate House Problems, by Ferrer and Jackson, Cambridge, 1850.

Then when any rotating fluid mass is in equilibrium under the attractions of its own particles and their centrifugal forces, any one of similar figure and same density and rotating in the same time, is also in equilibrium. For the ratio and directions of the attractions and centrifugal forces are the same in both, and therefore their resultants as their similar dimensions. Hence, if the less one be enveloped by a shell which is the difference of the two, the entire figure thus formed, which is every way equal to the greater, will be in equilibrium. Hence, the attraction of the enveloping shell has no effect on the interior mass, a property just shown to belong only to the ellipsoidal shell whose outer and inner surfaces are concentric, similar, and similarly placed. Hence, the ellipsoidal form is the only one, under the given conditions, of equilibrium.

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* This paper and the one presented at the Dubuque meeting of the American Association are to be inserted in my work on Gravitation, which will then contain the only correct formula ever published for determining the figure of the earth on the hypothesis of fluidity, whether that figure be an ellipsoid of three unequal axes or two.

Lenox, Warren County, Illinois, Sept. 12th, 1872.

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