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ERRATA.

Vol. XXXIV, p. 136, line 23 from top, for ROMINGA, read ROMINGER.—Vol. XXXV, in Index, p. 470, insert *W. C. Minor* on fission in Annelids, 35.

P. 243, l. 5 from bottom, for *Schiesspulver* read *Schiesspulvers.*—P. 198, line 10 from top, for P, read *p.*—P. 205, l. 6 from top, for formula, read column.—P. 209, l. 13 from top, for $s_{,,}-s_{,}=\frac{1}{2}P(e+x)^2x$, read $s_{,,}-s_{,}=\frac{1}{2P}(e+x)^2x$.—P. 210, l. 9 from top, for formed, read obtained.—P. 378, l. 19 from bottom, for "does not treat the," read "does not treat *of* the."—P. 382, l. 17 from top, for "the time," read "for the *first* time."—P. 407, line 30 from top, for *depruis* read *depuis.*—P. 408, lines 11 and 15 from top, for Flamen, read Flamm.

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[SECOND SERIES.]

ART. I.—*On Cephalization, and On Megasthenes and Microsthenes, in Classification* (being in continuation of an article on the Higher Subdivisions in the Classification of Mammals); by JAMES D. DANA.

IN the paper on the Classification of Mammals, published by the writer in the last volume of this Journal (p. 65), and also in his earlier paper on Crustaceans, the principle of cephalization is shown to be exhibited among animals in the following ways:—

1. By a transfer of members from the *locomotive* to the *cephalic* series.

2. By the anterior of the locomotive organs participating to some extent in cephalic functions.

3. By increased abbreviation, concentration, compactness, and perfection of structure, in the parts and organs of the anterior portion of the body.

4. By increased abbreviation, condensation, and perfection of structure, in the posterior, or gastric and caudal, portion of the body: as, in the greater compactness and larger number of segments combined in the sacrum of the *higher Megasthenes* than in that of *Cetaceans*, or *Edentates;* the less posterior elongation of the vertebral column and body in the *higher Megasthenes* than in *Cetaceans*, or in the *tailless Batrachians* than in the *tailed* species of the group, etc.

5. By an upward rise in the cephalic end of the nervous system. This rise reaches its extreme limit in Man. Birds thus show their superiority to Reptiles: but not to Mammals; for the bird-type, like the Reptilian, is relatively diminutive in life-

system (p. 9, beyond); its relation to the Reptilian type is much like that of Insects to the Crustacean (p. 6).

A decline in the grade of cephalization is shown by the reverse of these conditions: as (1) by a transfer of members from the cephalic to the locomotive series; (2) by the posterior cephalic organs participating in locomotive functions; (3, 4) by increased laxness, length and breadth, or spacing, among the parts of either the anterior or posterior portion of the body; (5) by increased proneness in the position of the nervous system. Also—

6. By an adaptation of the organs of the senses to locomotive or prehensile purposes: as in the case of the proboscis of the Elephant, which is a perverted nose; also the prehensile terminations of the second antennæ of many inferior Crustaceans.

7. By an abnormal multiplication of the parts in the anterior portion of the body: as in the excessive number of teeth in some *Cetaceans* and *Edentates.*

8. By an abnormal multiplication of the parts in the posterior portion of the body: as in the abnormal multiplication of members and segments in *Phyllopod Crustaceans*, *Myriapods*, etc.

9. By a further degradation of the structure before and behind, or a degeneration or obsolescence of the parts or organs: as in the absence of teeth in some *Cetaceans* and *Edentates;* the degradation of feet into fins, as in *Whales*, or their total absence; the absence of a series of abdominal members in *Entomostracans;* the absence of antennæ in Articulates, *provided* the senses corresponding to these organs are absent or comparatively imperfect; the coalescence of the head and thorax, or of these with the abdomen; the extension towards, or into, the head of the gastric viscera.

10. By excessive size of body through mere vegetative enlargement: as in the *Megatherium*, the *female Bopyrus*, *Limulus*, etc.

Degradation, or a decline *below the normal* level, may hence be—

I. *Multiplicative.*—Methods 7, 8, above.

II. *Degenerative.*—Methods 3, 4, 9.

III. *Vegetative.*—Method 10. Also IV. *Phytoid* (or *plant-like*), when animals (as *Polyps*) have (11) the power of budding, or (12) a radiate structure, or (13) attachment below; and in such cases the decephalization is often almost as complete as in plants.[1]

Examples of *cephalization* by the first method, or by a transfer of members from the locomotive to the cephalic series, (or

[1] The methods of decephalization in Crustaceans are embraced under *two* heads by the writer, in his paper on the Classification of Crustaceans, (*this Jour.*, [2], xxii, 28, and *Expl. Exp. Rep. on Crustacea*, p. 1412,) as follows:—

"*First:* A diminution of centralization, leading to an enlargement of the circumference or sphere of growth at the expense of concentration, as in the elongation of the antennæ and a transfer of the maxillipeds to the foot-series, the elongation of the abdomen and abdominal appendages, etc.

"*Second:* A diminution of force as compared with the size of the structure, leading to an abbreviation or obsolescence of some circumferential organs, as the of *decephalization*, by the reverse,) occur in the two highest subkingdoms, those of *Vertebrates* and *Articulates*. They fail in the two lower subkingdoms, those of *Mollusks* and *Radiates*, because of the absence of the necessary structure for showing it.

The examples under *Vertebrates* and *Articulates*, and the relations of the orders among *Mollusks*, may be briefly considered.

I. *Vertebrates.*—Only a single example in the class of Mammals, or even in the whole subkingdom of Vertebrates, is possible, owing to the fixed nature and simplicity of the head, and also the limited number of feet, two pairs being the maximum. This one example has already been pointed out and shown to be the basis of the grand distinction between Man and other Mammals. In passing downward from the exalted position which Man holds, there is a transfer of the fore-limbs to the locomotive series: the structure of the head in Vertebrates, even to the lowest Fishes, admits of no other case of analogous transfer.[2] In the *Walrus*, the tusks have some locomotive functions, as they serve to rest the fore-part of the animal or its head on the ice, while the body is in the water; but this is an example under the *second* method. The feet are wholly absent in *Snakes*, and the ribs aid in locomotion; but this is only a degradation of the vertebrate type, and not decephalization by the first method. In most *Fishes*, and in *Whales*, the locomotive function is transferred mainly to the elongated vertebrated posterior extremity of the body—a case of *degenerative* degradation, similar to the last, and analogous also to the *multiplicative.*

It is of sufficient interest in this connection to be repeated here, that among Mammals the four orders of Megasthenes exhibit in their *fore-limbs* four distinct grades of cephalization: in the *Quadrumanes*, these organs serve for carrying their young, supplying the mouth with food, taking their prey, and for locomotion; in the *Carnivores*, for taking their prey, and for locomotion; in the *Herbivores*, for locomotion only; in *Mutilates*, for fish-like locomotion, the members having the degraded form of fins.

II. *Articulates.*—In the subkingdom of Articulates, the three classes are *Insecteans*, *Crustaceans*, and *Worms:* the first includes

posterior thoracic legs or anterior antennæ, or the abdominal appendages (where such appendages exist in the secondary type embracing the species).

"These circumstances, moreover, are independent of a degradation of intelligence, by an extension of the sphere of growth beyond the proper limits of the sphere of activity."

[2] To the *zoological* characteristics of Man, mentioned in the writer's article on Mammals,—that is, the extreme cephalization of his system and the erect form connected therewith,—should be added the following, that, while in the Quadrumanes the feet are clasping or prehensile feet, in Man they are simply organs of support and locomotion. The former fit the Apes for their climbing habits, the latter, Man, for human duty. The discussion, now in progress, whether the hind-limbs of the Gorilla terminate in hands, or in true feet—"in no sense hands," in the words of Prof. Huxley—is of small importance in this connection.

The writer's view of the characteristics of Man depending on his *spiritual* nature are given in the last volume of this Journal, on page 452.

Air-breathing species, (*Insects*, *Spiders*, and *Myriapods*,) and the second and third, the Water-articulates. Examples of cephalization by the *first* method occur in the first two of these classes. They can not in the third, because Worms have no proper feet, and are not a type with *closed* limits, but one admitting of indefinite multiplication of parts behind and therefore *open* posteriorly.

1. *Insects*, the highest of the three orders of Insecteans, have three pairs of mouth-organs, and three pairs of legs. As the wings belong to the same segments of the body with two of the pairs of feet, they are not to be counted; for the transfer noted is, in fact, a transfer of *segments* of the body along with their appendages.

Passing down from Insects to *Spiders*, the mouth loses one pair of organs, the posterior, and the feet gain one pair, there being *four* pairs of feet in Spiders—that is, there is a transfer of *one* pair from the cephalic to the locomotive series. The absence of antennæ in Spiders is no mark of degradation, since the *senses* exist in good perfection.

Descending lower, to the *Myriapods*, the Articulate type passes below the range of normal variation into a degradational form, and one which, like that of *Worms*, admits of indefinite posterior elongation or multiplication of segments (by the *eighth* method of decephalization), and hence it has no *closed* or fixed limits, like that of Spiders or Insects. Under this loose and multiplicative condition of the system, there is no regular transfer backward of another pair of mouth-organs: the type is distinguished, instead, by the degradational character just mentioned.

2. The facts among *Crustaceans* have already been pointed out: that, descending from *Decapods*, (Crabs and Lobsters,) which have *six* pairs of mouth-organs and *five* of feet, to *Tetradecapods*, two pairs of the mouth-organs are transferred to the locomotive series, making the number of pairs of feet *seven*, and of mouth-organs *four*.

Descending further, to *Entomostracans*, or the third order, the mouth-organs lose one or more of the remaining pairs, and sometimes, as in *Limulus* (or the Horse-shoe Crab, as it is called) *all*, for the mouth-organs in this species are all true feet. The Entomostracans exemplify decephalization by degeneration (*ninth* method): as in the absence of one or two pairs of antennæ; the absence of one or two or more posterior pairs of thoracic feet; the absence of the series of abdominal members; and sometimes, as in *Limulus*, by the reduction of the abdomen to a mere spine. They are *degradational* forms, as well as the Myriapods; and, hence, the apparent difference of grade, which might be supposed to be marked by the number of pairs of mouth-organs transferred backward, can not serve to subdivide the order. The distinction of the Entomostracans from the higher

Crustaceans consists rather in their degradational characters than in any peculiarities of the mouth. In the tribe of Ostracoids (Cypris, etc.) alone, one genus has *two* pairs of mouth-organs, the rest being legs, another *three*, and another *four*, the Tetradecapod number.

III. *Mollusks.*—It has been remarked that the subkingdom of Mollusks cannot, from its nature, exemplify the *first* method of cephalization. The methods exemplified are the *third*, *fourth*, *ninth* and *tenth*. In the transition from the order of *Cephalopods*—the *first*—to that of *Cephalates* (*Gasteropods*),—the *second*—there is a loss of the feet or arms, and a diminished perfection of the senses, and activity is reduced to sluggishness. Descending to the *third* order, or *Acephals*, the antennæ fail, the eyes become imperfect or obsolete, locomotion becomes very imperfect, and in some fails altogether. Among *Bryozoans*, a still inferior order, all the organs of the senses fail, and there is the radiate structure of vegetation as well as its sessile character.

The difference in cephalization between an oyster and a clam is very strongly marked, the oyster, when placed in its normal position, having its body nearly all posterior to the beak, being merely a large gastric mass, and the clam having one-third of the body anterior to the beak, and really exhibiting something stately in mien compared with the oyster.

Other illustrations of the subject might be given; but they are not necessary to explain the general principle in view.

The *number of pairs of feet* in the subkingdoms of Vertebrates and Articulates, under those types which afford examples of the first method of cephalization, is as follows:

I. VERTEBRATES.

1, in Man; 2, in all other Vertebrates.

II. ARTICULATES.

1. *Under Insecteans:* 3, in Insects; 4, in Spiders.
2. *Under Crustaceans:* 5, in Decapods; 7, in Tetradecapods.

The number of pairs of feet in the different groups are then 1, 2, 3, 4, 5, 7. Only *one* case of typical transfer occurs in each of the three classes illustrating the subject, Mammals, Insecteans, and Crustaceans; and these cases occur uniformly between the *two highest orders* of the class.

Man's title to the place assigned him in our former paper appears therefore to be unquestionable.

The types of Vertebrates and Articulates do not admit of any homological comparisons.

The types of Insecteans and Crustaceans are modifications of a common type; yet the two are so widely different, that it is far from true that the *five* pairs in the highest Crustaceans correspond to the *four* in Spiders *plus* a preceding pair of mouth-organs. The head and locomotive part of the thorax in the Land-Articulates appear to correspond unitedly, as stated by Latreille, to the cephalic portion of the Crab, that is, to *nine* anterior segments out of the *fourteen* cephalo-thoracic. In other words, this part of the body of an Insect is an extreme concentration of the anterior portion of a Crustacean—an example of extreme cephalization; while a Crustacean is a diluted Insect, being much larger, and more numerous in segments and members.[3]

The *Lobster* (or any ordinary Macrural Decapod Crustacean) has an elongate body, and an abdomen well developed and furnished below with a full series of members. In the male *Crab*, also a Decapod, the body is very short, and the abdomen is without its members, besides being so small that it folds into a groove in the under shell of the body; this diminution of size and increased compactness are a consequence of the higher cephalization of the species (Method 4). Passing from Crabs to the still higher Articulates, *Insects*, there is an example of this cephalization carried to its maximum, it appearing in the extreme diminution of size of body and members, in the very small distinct head (comprising, normally, *a third* of the segments of the body, though so small), and in the thorax freed from the viscera and devoted mainly to locomotion. By this method, an animal is made of the highest instincts under the Articulate type.

From these examples it is evident that where there is a compacting of the body connected with rise in grade, it is not merely a *general* compacting of the different parts alike, or a general concentration and perfecting of the system, but a true *cephalization* of the system,—the compacting and perfecting showing itself primarily in a greater concentration, predominance, and domination of the cephalic extremity.

Among Articulates having feet, an *Insect* and a *Limulus* stand at the opposite poles of cephalization. The mouth-organs and feet in both correspond to those of the head (or the mouth-

[3] There appears to be no reason to doubt that, in all types, *not degradational*, each pair of members (wings excluded) corresponds to a separate normal segment of the body. Audouin & Edwards are sustained in their views on this point by the fact, that in a *Squilla*, three anterior cephalic segments (those of the eyes and two pairs of antennæ) and four posterior thoracic are actually distinct; and in an *Erichthus*, other segments, anterior to these four, are faintly indicated. (See the Author's *Expl. Exped. Report on Crustacea*, plate 41).

Assuming the number of normal segments anterior to the mouth in an Articulate from that (three) in the head of a Crustacean, the complete number in an Insect is *eighteen;* and in a Crustacean, *twenty-one, three abdominal* being present which are obsolete in an Insect. In the former, *half* (or *nine*) pertain to the head and thorax (only three to the thorax); in the latter, *two-thirds* (or *fourteen*); the rest being abdominal. In an Insect, the viscera are *abdominal*, in a Crustacean (excepting some degradational forms,) *thoracic*. The separation of the viscera from the thorax in an Insect leaves this part to higher purposes. It is to be noted, that the 10th to the 14th segments, inclusive, are *visceral segments in both Insects and Crabs*—being the first part of the abdomen in an Insect, and the last (and large foot-bearing) part of the cephalothorax in Crabs.

organs) of a Crab. But in *Limulus* there is extreme of degradation, all the members being large and stout feet, only the basal joints of the feet serving as jaws,—the body being enormously enlarged by mere vegetative growth,—the antennæ wanting, or reduced to a pair of pincers, and the animal sluggish, a sport of the waves on a beach; while in Insects, there is extreme of cephalization, the pairs of feet only *three* and those small and slender, and the body minute in comparison—the antennæ well developed and serving as delicate organs of sense—the animal active, and wonderful in its instinctive habits and knowledge.

The parallelism, above shown, between Insecteans and Crustaceans proves that *Insects*, *Spiders* and *Myriapods* are orders in a single class, and not separate classes.[4] Moreover, the orders under the classes of Insecteans and Crustaceans constitute parallel series, the first two of each being *closed* types, within the range of normal variation, and the last one of each (*Myriapods* and *Entomostracans*) being a degradational type, though different, one from the other, in kind of degradation. The parallelism between the series would be well exhibited if the orders were thus named:

Those of Insecteans, (1) *Hexapods*, (2) *Octopods*, (3) *Myriapods;*

Those of Crustaceans, (1) *Decapods*, (2) *Tetradecapods*, (3) *Colopods*, this last term (from κόλος and πους) signifying *defective feet* or *members*, which is the prominent characteristic of the order.

The parallelism extends even further than has been mentioned. The Tetradecapods are not an intermediate type between Decapods and Entomostracans: on the contrary, they lie quite out of the range of either. The Decapods, in their degradational species, pass almost into Entomostracan forms, and not into Tetradecapod forms. So among Insecteans, the Spiders have the same isolated position and defined limits. Insects, in their degradation, approximate to Myriapods, not to Spiders. In fact, Spiders stand more nearly between Insects and Crustaceans than between Insects and Myriapods.

There is, here, a cross affinity between Insecteans and Crustaceans which is of great interest. The relation of common *Spiders* to *Brachyural Decapods* or *Crabs* is seen, (1) in the general

[4] The grand distinction of the subdivision of *Insects* consists in their having three pairs of mouth-organs and three pairs of feet; of *Spiders*, in having two pairs of mouth-organs and four pairs of feet; of *Myriapods*, in having, through degradation, an indefinite number of segments and feet. Hence, to include *Spiders*, *Myriapods*, and the Hexapod group of *Pulices*, *Lepismæ*, *Pediculi*, and the like, in one division called *Aptera*, as done by some naturalists who adopt the general division of Insecteans, is a violation of all true affinities.

Professor Agassiz recognizes the same three classes of Articulates, as above, by the writer, and the same subdivisions, or orders, of Insecteans, but "from embryological data." The writer has not felt ready to deprive Spiders and Myriapods of their place in separate classes, coördinate with those of Insects, Crustaceans and Worms, (a common method among zoologists,) until recently, when the special application to these Articulates of the principle above explained occurred to him.

form or habit of body (some Crabs are called sea-spiders); and (2) in the coalescence of the thoracic and abdominal nervous ganglions into a single central thoracic ganglion. At the same time, the division of *Scorpions*, among Spiders, is correspondingly related to that of the *Macrural Decapods*, (1) in the body consisting of a series of segments; and (2) in the nervous ganglions being distinct, one to each abdominal segment. Moreover the maxillipeds are long and chelate, like the outer pair in some inferior Macrurans.

Again, the *Myriapods* are distantly related to the *Tetradecapods*, they being similar in their annulated structure, each segment having its pair of feet, and some species of the former (as those of *Glomeris*) even resembling the latter quite closely in form, articulation, and antennæ, and many of them having also the habit of some *Oniscidæ* (Tetradecapods) of rolling into a ball.

Thus, the *second* order of Insecteans is related, as regards form, to the *first* of Crustaceans; and the *third* of Insecteans, to the *second* of Crustaceans.

The earliest of Crustaceans, the *Trilobites*, one of the *comprehensive* types as styled by the writer, are, therefore, not only intermediate between Entomostracans and Tetradecapods, but also, in some respects, between these and the Myriapods. Moreover, like the latter, Trilobites are abnormal in the very large number of segments of which the body is composed; and sometimes, also, they present no distinction between the cephalothorax and abdomen.

The facts pointed out prove conclusively that Insecteans and Crustaceans constitute classes of equivalent value.

2. *Megasthenes and Microsthenes.*

The two grand divisions of typical brute Mammals, the *Megasthenes* and *Microsthenes*, are not separated by any very marked difference in type of structure; and still there is a profound fundamental difference between them,—that, to which the names refer. This is in contrast with the fact among Crustaceans, the megasthenic and microsthenic divisions of which (the *Decapods* and *Tetradecapods*) stand widely apart. But in the class of Crustaceans, the structure varies between remote extremes, while, in that of Mammals, there is a remarkable fixedness, or an extremely limited range of variation. Hence, in the distinctions of *Megasthenes* and *Microsthenes*, among Mammals, we cannot look for the marked diversity that subsists between *Decapods* and *Tetradecapods*, although the naturalness of the subdivisions is none the less real. The words *Megencephals* and *Micrencephals* (signifying *large-brained* and *small-brained* Mammals) may better satisfy the desire for names expressing something tangible in the structure. Yet they do not appear to indicate the fundamental distinction between the groups. A general structural characteristic may yet be detected corresponding to these megasthenic and microsthenic qualities; but even then, the distinctive idea of the subdivisions could hardly be better expressed than by the names proposed.

The parallelism between the Megasthenes and Microsthenes among Mammals and the Decapods and Tetradecapods among Crustaceans suggests, that if the subdivisions be called *orders* in the latter case, they should be so called in the former.

The distinction between Megasthenes and Microsthenes may, perhaps, become more intelligible, if we regard a living structure as a *life-system*, or, speaking dynamically, a *life-battery*. In order that such batteries may have a very wide range of size, two or more plans of construction, more or less different, appear to be requisite. With one plan, there is a certain magnitude which is that of most efficient action and power; and from this magnitude, there may be a series of larger and smaller sizes, reaching to the outer limits of normal perfection; and then, if these limits be passed in either direction, that is, either on the side of too great magnitude, or of too little, degradation in the structure and its powers begin to appear.

To carry the species through another range of sizes, with normal perfection of structure, another somewhat different plan is required. The Megasthenes represent one such plan, the Microsthenes another.

This idea is brought out by the writer in his chapter on the Classification of Crustaceans already referred to. He there says, speaking of the orders of Crustaceans, viz: Decapods, Tetradecapods, and Entomostracans:—

"I. Each type corresponds to a certain system of force more or less centralized in the organism, and is an expression of that force,—the higher degree being such as is fitted for the higher structures developed, the lower such as is fitted for structures of inferior grade and size. In other words, the life-system is of different orders for the different types, and the structures formed exhibit the extent of their spheres of action, being such as are adapted to use the force most effectively, in accordance with the end of the species.

"II. In a given type, as the first, for example, the same system may be of different dimensions, adapted to structures of different sizes. But the size in either direction for structures of efficient action is limited. To pass these limits, a life-system of another order is required. The Macroura, as they diminish in size, finally pass this limit, and the organisms (Mysidæ, for example) are no longer perfect in their members; an obsolescence of some parts begins to take place, and species of this small size are actually complete only when provided with the structure of a Tetradecapod.

"The extreme size of structure admitting of the highest efficient activity is generally three to six times lineally the average or mean typical size. Of these gigantic species, three or four times longer than the mean type, there are examples among the Brachyura and Macroura, which have all the highest attributes of the species. There are also Amphipoda and Isopoda three inches in length, with full vigorous powers. Among Entomostraca, the Calanidæ, apparently the highest group, include species that are three lines long, or three times the length of the mean type.

"III. But the limit of efficient activity may be passed; and when so it is attended with a loss of active powers. The structure, as in the female Bopyrus and Lernæoids, and the Cirripeds, outgrows vegetatively the proper sphere of action of the system of force within. This result is especially found in sedentary species, as we have exemplified in our remarks on the Cirripeds.

"IV. Size is, therefore, an important element in the system of animal structures. As size diminishes, in all departments of animal life, the structure changes. To the human structure there is a limit; to the quadrupeds also, beyond which the structure is an impossibility; and the same seems to be the case among Crustacea. The Decapod, as the size diminishes, reaches the lowest limit; and then, to continue the range of size in species, another structure, the Tetradecapodan, is instituted; and as this last has also its limit, the Entomostracan is introduced to continue the gradation; and, as these end, the Rotatoria begin. Thus Crustacea are made to embrace species, from a length of nearly two feet (or two hundred and fifty lines) to that of a one-hundred-and-fiftieth of a line. These several types of structure among Crustacea do not graduate, as regards size, directly from one to another, but they constitute overlapping lines, as has been sufficiently shown."

While on this subject of life-batteries, the writer would suggest that the grand dynamical distinction between Mollusks and Articulates may be this:

A Mollusk corresponds to a *quantity-battery*, but one of very weak force; that is, it is analogous to a galvanic battery of *two or three small pairs, at the most.* This is indicated, (1) by the structure of the species, especially the absence of all articulations, the animal—a locomotive digestive system—being, as it were, in one simple bag; (2) by the number of ganglions limited to three; and (3) by the sluggishness of the animal.

An Articulate, on the contrary, corresponds to an *intensity-battery*, or is analogous to a galvanic battery of *many small pairs;* for (1) the body consists of many segments; (2) there are nearly as many nervous ganglions as segments (normally, as many); and (3) the animals in the more typical species have extreme rapidity of movement, and high instincts. The small number of ganglions in most Spiders is evidently due to a coalescence of several in the one central thoracic ganglion, as in Crabs.

In the highest Mollusks, the Cephalopods (Cuttle-fish, etc.), the Invertebrate *quantity-battery* reaches its greatest power.

Vertebrates also appear to correspond to a quantity-battery (as shown by the simplicity of the nervous system); but to one admitting of vastly greater power.

ART. II.—*Observations upon some of the Brachiopods, with reference to the genera Cryptonella, Centronella, Meristella, and allied forms;* by JAMES HALL. Abstract of a paper read before the Albany Institute, February 3d, 1863.[1] (Communicated by the author.)

(Concluded from vol. xxxv, page 406.)

IN the *Thirteenth Report on the State Cabinet* (p. 74, 1860), I proposed the Genus *Meristella*, to embrace certain species before included under the Genus *Merista*, and which were shown not to possess the peculiar shoe-lifter process, or transverse septum, characteristic of the latter genus. I remarked as follows: "Restricting, therefore, the signification of the genus *Merista* to such forms as were originally included by Prof. Suess under that name, it becomes necessary to designate those species of similar form, but without the peculiar appendage of the ventral valve, by another generic term; and I would therefore suggest the name of *Meristella*, proposed by me last year."[2]

After describing the genus, I cited as illustrations several species from the Lower Helderberg group; and gave figures of the exterior of *Meristella princeps* and *M. nasuta*, the latter species from the Upper Helderberg group.

In the same Report I described three other species of the genus, viz: *Meristella Haskinsi*, *M. Barrisi* and *M. Doris*, but without giving illustrations of these.

Since, on the one side, this genus has been claimed to be equivalent to *Athyris*, and, on the other, the same author has placed some of its species under a later created genus *Charionella*, it seems necessary to repeat some of the characters of the genus in this connexion.

Genus MERISTELLA Hall, 1860.—The genus includes *terebratuloid* or *Athyroid* forms which are ovoid, more or less elongate, sometimes elliptical in outline, and not unfrequently transverse or sub-circular; valves unequally convex, with or without a median fold and sinus, and this feature usually confined to the lower half of the shell. Ventral beak more or less closely incurved (when closely incurved apparently imperforate), terminated by an aperture, the lower side of which may be formed by the umbo of the dorsal valve, or by a deltidium. Area none.[3]

[1] From the Transactions of the Albany Institute, with some verbal corrections and the introduction of subsequent observations by the author.

[2] In the *Twelfth Report on the State Cabinet*, 1859, page 78, in referring *Atrypa naviformis* of vol. ii, *Pal. N. Y.* to *Merista*, I said: "This species, and some others of the Clinton and Niagara groups, differ somewhat from true *Meristæ;* and should these differences prove of generic importance, I propose for them the name *Meristella*."

[3] Those species with the ventral valve closely incurved are *apparently* imperforate, since no foramen is visible above the umbo of the dorsal valve. In the separated valves of these species, I have not seen any deltidium; an open triangular space exists above the points of the dental lamellæ, and this communicates with the open cavity of the valve.

Valves articulating by teeth and sockets. Surface smooth or marked by fine concentric lines of growth, not lamellose, and indistinct or obsolescent radiating striæ, which are usually more

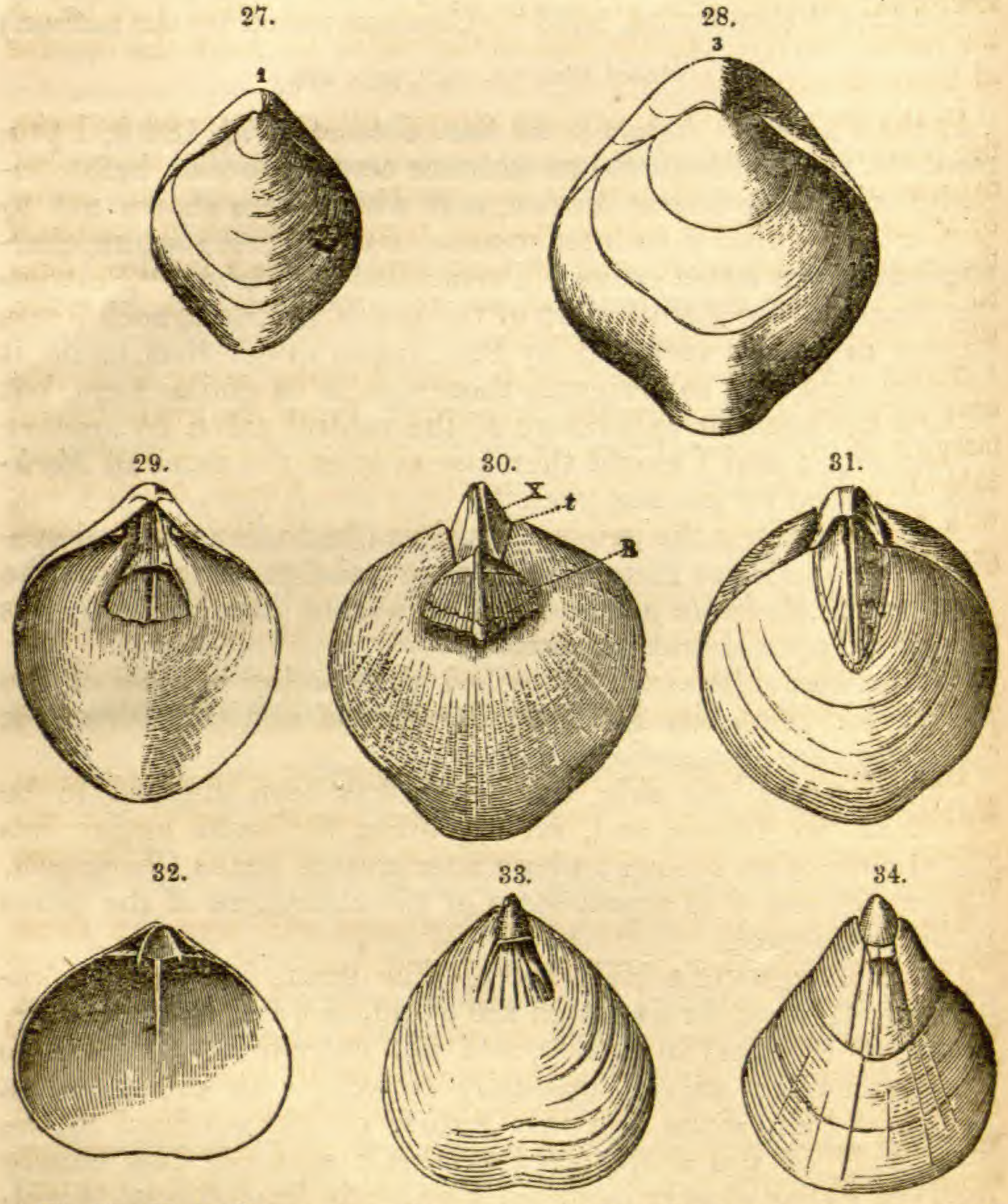


Fig. 27. *Meristella nasuta* = *Atrypa nasuta*, Conrad.—Dorsal view of a young individual.—Fig. 28. An older individual.—Fig. 29. Interior of the ventral valve.—Fig. 30. Cast of the ventral valve.—Fig. 31. Dorsal view of the same species.—Fig. 32. Interior of the dorsal valve of *M. arcuata*, showing the hinge plate and median septum.—Fig. 33. Cast of ventral valve of *M. Barrisi*.—Fig. 34. Cast of ventral valve of *M. Haskinsi*.[4]

conspicuous in the cast or exfoliated surfaces than on the exterior. Shell fibrous.

The ventral valve is much thickened on each side towards the beak, and the rostral cavity margined by flattened dental

[4] The casts of *M. Barrisi* and *M. Haskinsi* are obtained from solid specimens by removing the shells, and therefore have not that sharpness of the muscular markings which we find in weathered casts.

lamellæ, which extend downwards to the commencement of the muscular impression, and terminate at the edge of the shell in blunt tooth-like processes. The muscular impression forms a somewhat broadly triangular depression in the valve just below the rostral cavity. In the cast of this valve we have the reverse of these features.

In the dorsal valve there is a strong hinge plate or process, the prominent part of which is broadly triangular, somewhat depressed or spoon shaped in the centre, and supported below by a median septum which reaches from one-third to one-half the length of the valve, and on each side, marked by deep dental fossets, while the anterior angles are produced into the crura which support the internal spires.

Spires arranged as in *Athyris* and *Merista*, being a double cone with the apices directed outwards. From the lower lateral margins of the cardinal process or hinge plate, there is a callosity extending beneath and anterior to the dental fossets, and joining with the thickened margin of the valve, as in the other allied genera.

In the cast of the dorsal valve we have the mark of the median septum, with an elongate, lanceolate muscular impression, reaching nearly to the middle of the valve. The imprint of the triangular process, and the cavities made by the crura are often preserved.

The species of this genus may be readily distinguished from *Merista* by the absence of the shoe-lifter process, which, in numerous specimens compared, constitutes the principal difference between the two genera.[5]

The illustrations on the preceding page will serve to show more clearly the characteristics of this genus.

In the dorsal valve of *M. Barrisi* we have a hinge plate, with a median septum reaching more than one third the length of the shell, and the same characters exist in *M. Haskinsi.* In *M. Doris* the rostral cavity and muscular impression of the ventral valve are much elongated, and resemble what I have heretofore shown in *Meristella levis.*[6] The dorsal valve has a strong extended median septum and hinge structure as in the other species.[7]

The proportions of length of rostral cavity and muscular impression, vary in different species; and the muscular impression becomes much stronger and deeper in the older shells, when the valve as before remarked becomes thickened at the sides and

[5] On plates 39 and 41 of *Pal. N. Y.*, vol. iii, may be found some illustrations of the casts of species of this genus.

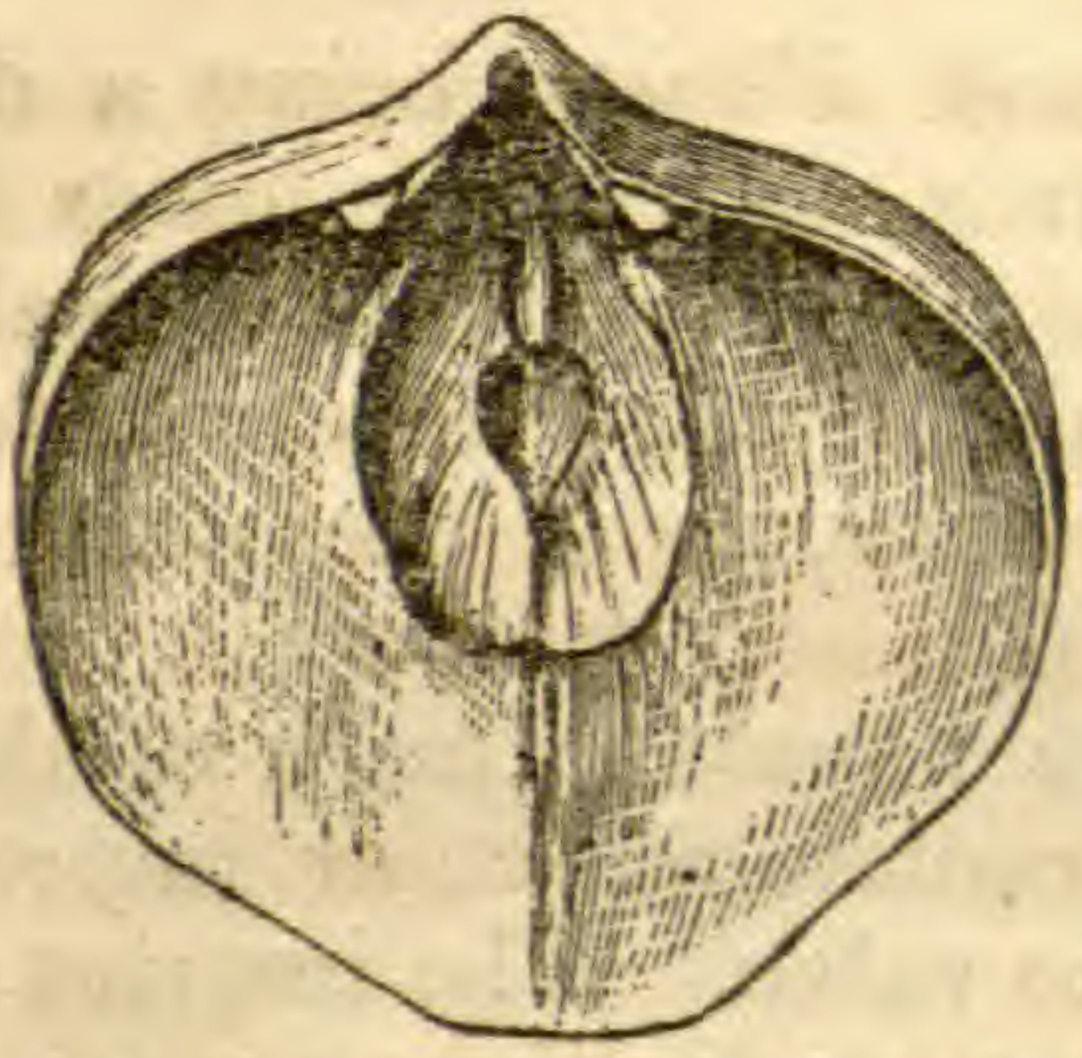
[6] *Palæontology of New York*, vol. iii, plate 39.

[7] In reclaiming these species of *Meristella*, I am not impugning the validity of the genus *Charionella* of Mr. Billings, for none of these have the characters of the dorsal valve of *Charionella* as represented on page 274, No. 33 of the *Canadian Journal*, which is not only clearly unlike *Meristella*, but very distinct from any genus of *Spiriferidæ* before described.

towards the beak. This character pertains to the limestone specimens, while those in the Hamilton shales, as figs. 7 and 8, have thinner shells, and less deep and strong muscular impressions.

I have already (*Thirteenth Report on the State Cabinet*, pp. 73–75, and illustrations on p. 93) pointed out the distinction between *Athyris=Spirigera* and *Meristella.* This difference is everywhere clear and unmistakable, in the external lamellose surface of the one, and the almost smooth character of the other. The muscular impressions of the ventral valve of *Athyris* are at once distinguishable from those of *Meristella;* as may be seen on comparison of figs. 35 and 36 with figs. 29 and 30.

35.



36.

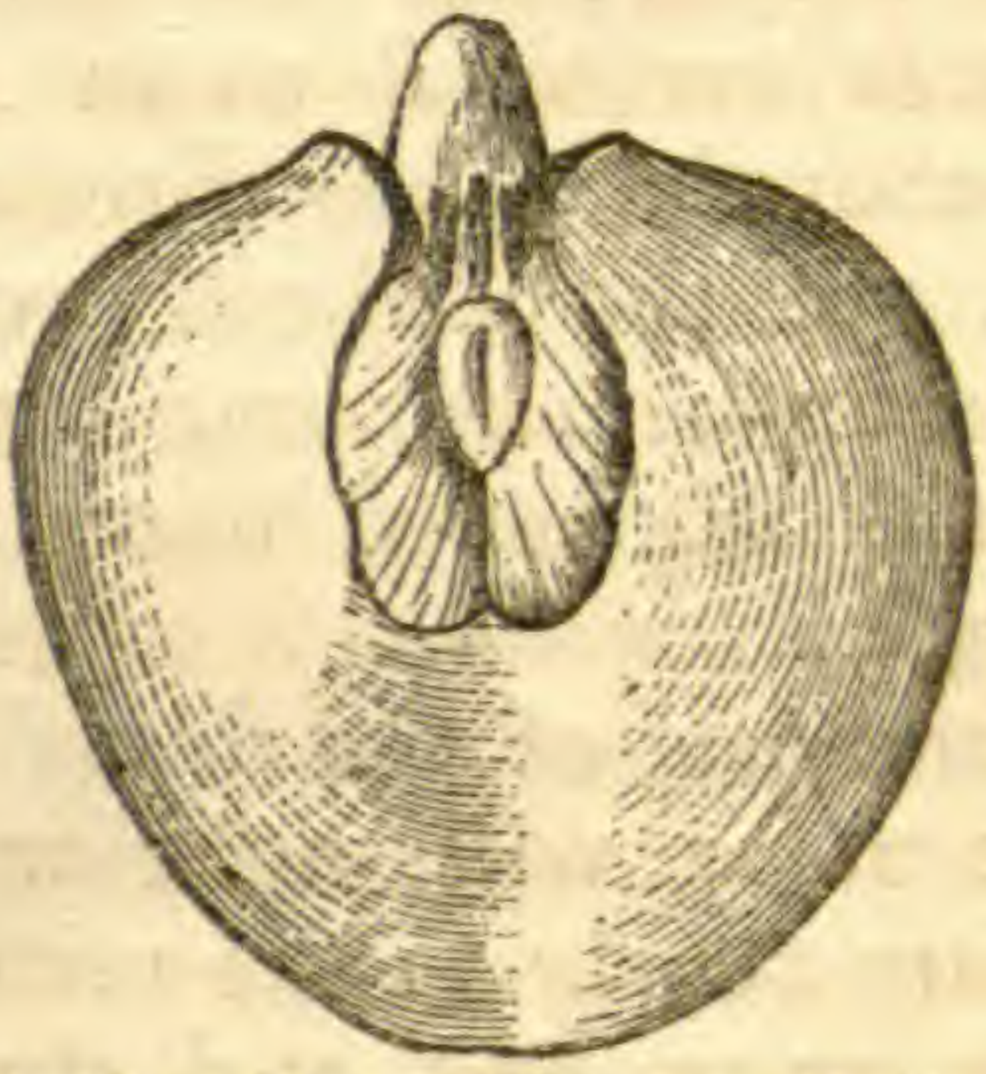


Fig. 35. Interior of ventral valve of *Athyris spiriferoides.*—Fig. 36. Cast of same.

In the dorsal valve, the muscular impressions differ from *Meristella;* the hinge plate is of somewhat different character, and the median septum is scarcely developed.

Note on the Genus Leptocœlia.—Among the specimens sent to me by Dr. Rominger, are two individuals of *Leptocœlia concava,* showing the existence of internal spires; and a careful examination of my own collections from the Lower Helderberg group has shown several specimens possessing these internal organs which have their apices directed obliquely outwards, and are connected near their origin by a strong vinculum on the dorsal side. After repeated examinations of a large number of the Oriskany sandstone species, from which the characters of the genus were mainly drawn, I have failed to detect internal spires. The form of the internal loop, as represented in the figures of the genus, was ascertained, as stated by me, mainly from cavities remaining in the crystalline filling of the shell. There were no appearances of spires, nor does a re-examination of the specimen afford any farther information, or indicate in any manner that spires have ever existed. The crura can be traced to the division at the process, and below this is a flat cavity.

A critical re-examination of the fossils referred to this genus shows that there are at least three distinct types, in their external form and features, which, in the absence of positive knowledge of the internal structure, were grouped together. A farther examination shows some peculiarities of hinge structure in each one of them, which are probably connected with more important difference of the internal parts. One of these types is indicated in the two strongly plicated species of the Oriskany sandstone, which have a median sinus near the front of the ventral valve, with two of the plications often closely incurved. Another type is that of *Leptocœlia concava*, and allied forms, which are more finely plicated, and where there is a sinus on the dorsal valve, though not distinctly defined. The third type is represented in *Leptocœlia* (*Atrypa*) *planoconvexa*, which has a somewhat undefined depression on the dorsal valve, and a form of cardinal process unlike the other species. The internal structure of this species is still unknown.

The *Leptocœlia imbricata* proves to be a *Trematospira*, and the same characters are apparent in *L. disparilis* of the Niagara group, the concavo-convex form of the shell being the only apparent deviation from typical forms of that genus. The *Terebratula lepida* of Goldfuss, as shown in the collections of Dr. Rominger, possesses internal spires precisely similar to those of *Trematospira camura*.

37.

Cœlospira concava.

The *L. concava*, both in its external characters and in the arrangement of the crura and vinculum, differs from *Trematospira*, and with the knowledge at present possessed, I am compelled to separate this species from those last named, and from the *L. flabellites*, *L. fimbriata* and *L. acutiplicata*, I would propose to indicate forms of this external character with similar crura and spires as *Cœlospira*.

The difficulty constantly attending the references of the Brachiopoda, to establish genera from external form and characters, renders it very desirable to search for the interior organization and appendages; but the condition of specimens does not always admit of satisfactory investigations, and not unfrequently the specimens possessed are so few as almost to preclude examinations of this kind.

As an example of the diversity of internal structure in similar external forms, I may mention the *Terebratula altidorsata* of Barrande, which so nearly resembles the *Centronella Glans-fagea* that it might readily be mistaken for that shell. On cutting and macerating specimens of the former, they prove to possess internal spires arranged as in *Meristella*, removing it from the family of the *Terebratulidæ*. I have not been able to determine whether the shell of this species is punctate or fibrous, from the specimens I possess.

ART. III.—*Hydraulics of the Report on the Mississippi River of Humphreys and Abbot;* by Prof. F. A. P. BARNARD.[1]

IN former notices[2] of this important Report, we have exhibited the general scope of the work, and presented some of its interesting details in regard to the physical geography of the great hydrographic basin to which it relates. We propose, in the present article, to examine the hydraulic investigations which occupy the greater portion of its bulk, and for the sake of which the laborious survey, of which it furnishes the history and the results, was originally instituted. We are induced to do this with some particularity, because, in the first place, the report itself is not so generally accessible as could be desired; and because, secondly, its conclusions, though novel and in some respects paradoxical, are yet so convincingly established upon the broadest possible basis of experimental proof, as undoubtedly to be destined to revolutionize the whole science of river-hydraulics, if in fact they may not properly be said to have, for the first time, created it. This may be considered strong language; and, whether regarded as an encomium upon the present work or a disparagement of what has been done in the same department heretofore, it will probably be pronounced extravagant. Lest, however, we should be thought to have spoken too lightly of the labors of former observers or theorists in this department of physical investigation, we cite here, in justification, the following sketch, by a master hand, of the condition of hydraulic science, theoretical and practical, as it existed sixty years ago; and as, without the slightest substantial improvement, it has continued to exist from that time to the present. The sketch is by the eminent Dr. Robison, of Edinburgh, himself a very felicitous, though not entirely an original, writer upon the same subject. Dr. Robison, after pointing out the extent to which the interests of every civilized people are involved in questions relating to the control and distribution of the running waters of the globe, and the great variety of ways in which we are constantly engaged in endeavoring to secure such control and distribution, proceeds as follows:

"Such having been our incessant occupations with moving waters, we should expect that, while the operative artists are continually furnishing facts and experiments, the man of speculative and scientific curiosity, excited by the importance of the subject, would, ere now, have made considerable progress in the science; and that the professional engineer would be daily acting from established principle, and be seldom disappointed in

[1] Report upon the Physics and Hydraulics of the Mississippi River; upon the Protection of the Alluvial Region against overflow; and upon the Deepening of the Mouths. By Capt. A. A. HUMPHREYS and Lieut. H. L. ABBOT. Submitted to the Bureau of Topographical Engineers, War Department, 1861. 4to, pp. 456 and cxlvi.

[2] This Journal, xxxiii, 181, xxxv, 223, 234.

his expectations. Unfortunately, the reverse of this is nearly the true state of the case: each engineer is obliged to collect the greatest part of his knowledge from his own experience, and by many dear bought lessons to direct his future operations, in which he still proceeds with anxiety and hesitation: for we have not yet acquired principles of theory, and experiments have not yet been collected and published by which an empirical practice might be safely formed. * * * The motion of waters has been really so little investigated, that hydraulics may still be called a new study." And again,

"As to the uniform course of the streams which water the face of the earth, and the maxims which will certainly regulate this agreeably to our wishes, we are in a manner totally ignorant. Who can pretend to say what is the velocity of a river, of which you tell him the breadth, the depth and the declivity? Who can say what swell will be produced in different parts of its course, if a dam or weir of given dimensions be made in it, or a bridge be thrown across it; or how much its waters will be raised, by turning another stream into it, or sunk, by taking off a branch to drive a mill? Who can say with confidence what must be the dimensions or slope of this branch in order to furnish the water that is wanted; or the dimensions and slope of a canal which shall effectually drain a fenny district? Who can say what form will cause or prevent the forming of elbows, the pooling of the bed, or the deposition of sands? Yet these are the most important questions. The causes of our ignorance are the want or uncertainty of our principles; the falsity of our theory, which is belied by experience; and the small number of proper observations or experiments, and difficulty of making such as shall be serviceable."

In asserting that this extract continues to represent the state of the science of river hydraulics at the present day, as completely as it did at the close of the last century, when it was written, we rest upon the notorious fact that these very questions, and every one of these questions, which are here set forth as unsolved in the time of the writer, are as far from finding their solutions in the works of the highest authorities on the subject, at the present time, as they were then. And though Dr. Robison himself proceeds to set forth a system—theoretic and practical—mainly adopted from Dubuat, which he concludes with affirming that he has "established," and which he assures us "may be confided in as a just representation of nature's procedure;" and though many eminent laborers, since him, in the same field, have put forth other systems, each believed to be an improvement on the last; yet nothing is better known than that the opinions and practice of our ablest engineers have been, and are yet, most widely at variance in regard to the simplest problems which present themselves relating to the control and management of our natural streams; or that, just in proportion as such streams are large, or are fed by numerous tributaries, drawing their waters from regions subject to diversified climatic vicissitudes, in the same proportion they are defiant of all the dogmas of the books, and all the laws, however carefully elaborated, of hydraulic theorists. Indeed, in the very year in which the survey, of which we have in this report the results, was commenced, there was submitted to the Bureau of Topographical Engineers, at Washington, a report covering a portion of the same ground, viz: the question of the best means of preventing the overflows of the Delta of the Mississippi, by a gentleman reputed to be one of the ablest civil engineers the country has produced—the late Col. Ellet—in which all the received formulæ for determining one of the most important elements in the inquiry—the mean velocity of the stream—are set aside, and a new one introduced; and in which the conclusion is reached that protection by levees in the lower parts of the valley, is entirely impracticable. The latter view is one which many others, both before and since, have strongly held: and inasmuch as this river is a subject which has more or less occupied the mind of every man in the country having any pretensions to engineering skill, or any taste for this class of physical inquiries, it is a view which has been just as strongly discountenanced and as stoutly controverted, as it has been confidently maintained. Now if the science of river hydraulics had not been in the condition of uncertainty and imperfection which we have presumed to impute to it, how could it be possible that a great practical problem like this, the very foremost in magnitude of importance that could be stated in regard to the grandest of our rivers, could thus divide for years the opinions, not merely of the inexperts who dwell upon the banks of the stream and suffer from its ravages, but of the mathematicians and philosophers of the whole country, who exhaust, for its solution, all the resources of science; and of the practical men whose daily occupation it is to deal with precisely such cases as this, and who make the subjugation and control of unruly waters their profession!

The Mississippi has undoubtedly been, in this country, the standing opprobrium of hydraulic science. Lesser streams, in their occasional outbursts of disobedience, when they roar defiance at the artificial laws set up to govern their behavior, though they may embarrass and annoy, yet from their inferior force and volume, fail so utterly to astound the unhappy engineer whom they set at naught, as this monster of the Mississippi. Even blunders in hydraulic constructions may not glaringly betray themselves, when the force to be contended with is moderate. But the Mississippi will submit to no trifling; and the projector or the constructor who shall prescribe to it a rule which is not its own rule —that is to say, the rule of nature—will find his plans confounded or his works swept off in one wild ruin, the very first time the giant rises in his might. All this is so well known that it has become habitual with the engineers, professional or amateur, whose notions and speculations this most independent of streams so constantly disconcerts, to shift the blame of the contradiction from their own shoulders to those of the river-god himself; and to speak disparagingly, perhaps we should say bitterly, of the Mississippi, as a lawless river, a capricious river, an inconsistent river, a congeries of anomalies which will never be understood, because, like Proteus, it never presents itself twice under the same aspect.

All this depreciatory and disrespectful language, is however sadly misapplied. The Mississippi is not a lawless river: it is only a large river. It is not a capricious river; but, draining as it does an immense hydrographic basin or system of basins, its hydraulic pulsations faithfully respond to every meteorological vicissitude in all that vast region, and present therefore phenomena which, at the time and place, may not always furnish their own immediate explanation. It is not an inconsistent river; for though it may sometimes seem, to the hydraulic engineer who studies its deportment, to conform itself with a docility truly gratifying to the formulæ which he has been taught to suppose should represent its movements, and at others may contradict them in a manner the most unceremonious and the most provoking; yet it is quite an error to draw, on that account, a conclusion injurious to the character of the stream. The true mode of looking at the phenomenon is this:—The formula is indeed inconsistent with the river, but not the river with itself. For the river being one of the forms of embodied nature, if the formula fails to represent it, then the formula is inconsistent with nature: and were the river to conform itself to the formula, it would be truly an inconsistent river.

It is probably in the fact that the Mississippi is a large river that we shall find a clew to the reason that it has been pronounced lawless. The laws by which it is presumed that flowing waters are governed, though partially deduced from theory, have been modified in their expression, by the accidental peculiarities of the minor streams whose habits they were designed to represent. Reduced to formulæ, they may exhibit no wide discordance with the phenomena of those particular streams, or of others which nearly resemble them. We by no means intend to deny to them this local or limited value. But these peculiarities may so mask the action of other and more general laws, that when the same formulæ are applied to the vast volumes of water which roll along the beds of the Ganges, the Amazon or the Mississippi, where the influence of such accidents bears to the great living force of the flood a ratio much less appreciable, they may be found to fail altogether. If it is true that flowing waters are governed in their movements by determinate laws, which human observation is competent to detect, and which the human understanding is capable of comprehending, then certainly we may most reasonably hope to arrive at the discovery of those laws by attending to the examples in which their operations are illustrated upon the grandest scale. The Mississippi is therefore a river, which, so far from being lawless, is likely to afford us the most magnificent revelations of law: and its successful study is far more likely to afford us an intelligent understanding of the phenomena of lesser streams, than any amount of labor expended on such streams, to conduct us to a comprehension of the Mississippi. The correctness of this view is, we think, fully established by the Report before us; of which we will now attempt to present a succinct analysis.

The properly hydraulic portion of the Report commences with a concise exhibit of the existing state of hydraulic science, including an exhaustive catalogue of the writers who have treated of the subject. This is followed by an elaborate detail of the operations of the survey, both in the field and in the office, with a statement of the conclusions to which they successively led. Then succeeds an examination of the schemes which have been suggested for the protection of the alluvial lands of the valley against inundation; by straightening and shortening the channel of the river; by relieving it of a portion of its burthen of waters, through the diversion of tributaries or the creation of new outlets; and by confining the waters within levees or embankments higher than the highest floods. The first of these divisions we pass over. The third may form the subject of a future notice. It is the second which embraces the matter which interests us at present. In presenting the conclusions of the report as it regards the laws which govern the movements of flowing water, we shall depart from the order of the report itself so far as to group all these conclusions at once together, apart from, and in anticipation of, any explanation of the methods by which they are deduced. In this form their bearing upon each other, and their completeness as a system, will better appear. The authors, in their development of the subject, have strictly and very properly pursued the inductive method, by which the investigation was originally conducted; each conclusion being preceded by the full detail of the processes through which it was reached.

It must be premised that the problem of the flow of water in natural channels is complicated by many conditions subject to no definite law; such as the variations of cross-section, the number and magnitude of bends, and the irregularities and obstructions which may exist in the bed. In the first step toward the solution, the complications and uncertainties which the consideration of these particulars would introduce must be avoided. The laws which we are about to state, are, therefore, to be understood of waters moving uniformly in channels straight and regular. With this explanation, we cite from the report, in a form condensed from that in which we find them, the following propositions:—

In a uniformly flowing stream, the maximum velocity of the water, in any vertical plane parallel to the current, is not found at the surface, but at a point situated a little more than three tenths of the depth below the surface.

To whatever cause it may be owing, there is a resistance to the flow of water at the surface, similar in kind to that which takes place at the bottom, though usually less in degree. This resistance is propagated downward, according to a law of diminution similar to that with which the resistance at the bottom is propagated upward.

If the velocities in the same vertical plane, parallel to the current, be plotted as ordinates, and the depths at which they are observed as abscissæ, the curve drawn through the points thus determined is sensibly a parabola, having the filament of maximum velocity for its axis, which is, of course, horizontal.

This parabola varies its curvature with the changes in the mean velocity of the river; the curvature being at its maximum when the velocity is greatest. When the velocity is zero, the parabola becomes a straight line.

The law which governs the curvature is determined by the proposition, that the reciprocal of the parameter of the parabola varies as the square root of the mean velocity of the river. The reciprocal of the parameter of sub-surface velocity is therefore the ordinate in another parabola, in which the mean velocity of the river is the abscissa.

The parameter of the parabola of parameters is, for rivers generally, sensibly constant. It is, however, a function of the depth; but such a function that the variations are nearly inappreciable for river formulæ, except for depths less than twenty or perhaps twelve feet.

The variations of velocity in horizontal planes, at the surface or below it, follow the same law as in vertical planes, the curve which represents them being a parabola having its axis in the thread of maximum velocity.

The depth of the axis of sub-surface velocities in vertical planes is affected by the wind, being depressed when the wind is up-stream, and elevated when the wind is down-stream. The amount of displacement is directly proportional to the force of the wind and the depth of the river, and is sensibly the same for the same wind-force, in either direction.

Neither the velocity at the surface nor the velocity at the bottom, nor, generally, the velocity at any determinate depth, is a function of the mean velocity of the river only. The methods of gauging which have been founded on the assumption of a simple ratio, existing between some particular observed velocity and the mean velocity, are therefore all erroneous.

The ratio between the observed velocity, at any depth in any vertical plane, and the mean velocity, in the same vertical plane, is a function of three variables, which are the mean velocity of the stream, the depth of the river, and the force of the wind. There is one particular depth at which the ratio becomes independent of this last variable, and sensibly so of the depth of the river. This is the depth midway between surface and bottom.

The simplicity of the relation, between the mid-depth velocity and the mean velocity in the same vertical plane, suggests a method of gauging rivers, by which the labor of the process is greatly diminished, and its accuracy promoted. A method may be founded upon the observed velocity at *any* depth, provided the variables which affect the ratio between that and the mean velocity in the same plane are duly considered. Such a method was employed in the measurements of the Mississippi, during a considerable period of the operations of the survey.

The foregoing are the principal laws which govern the habitudes of water flowing uniformly in straight and regular channels. The following relate to the relations which exist between the cross-section, slope, and mean velocity of the stream.

The area of the cross-section, the wetted perimeter, the width, the slope, and the mean velocity of the river, are connected with each other by such relations, that, when the first three are ascertained by measurement, together with the discharge per second, the other two may be determined. For practical purposes, the wetted perimeter and the width may be treated as a single variable. The variables will then be four; and of these, if any two be given along with the discharge, unless the cross-section and mean velocity happen to be given together, the others may be found.

If a sensible addition be made to the waters of the river in any given stage, all the variables will be increased at once. The changes of the cross-section and the perimeter are, however, simple functions of the increase of depth and the inclination of the banks; or, the latter being disregarded as of trivial influence in a large river, of the increase of depth only. The change of slope is a function, but a less simple one, of the same unknown quantity. If, during a rise, the stand of the river above the extreme low water line be taken as the ordinate of a curve, and the increase of slope divided by the increase of depth be taken as the abscissa, the curve to which these coördinates correspond is a parabola. The parameter of this parabola is constant at the same locality, but its values at different localities are different.

By assuming for the change of stand, or the rise of the river, a hypothetical value, the new slope and new mean velocity may be computed; from which, in turn, a *calculated* value of the rise may be obtained. Thus, by a simple system of trial and error, the true value of the rise will easily be determined; and the resolution of involved equations avoided. In a similar manner, the depression of level, or the fall of the river, may be ascertained, in case any determinate portion of its waters be withdrawn by opening a new outlet.

The treatment of this problem by the authorities generally has the advantage over this of being greatly more simple; but it has also the disadvantage of not in the least representing nature.

These statements embrace, in brief, the substance of the contributions of this valuable report to the advancement of hydraulic science; and the basis of the new methods of practice which its authors have introduced. In estimating the effects of bends in the stream, they have adopted the principle of Dubuat, derived from observations on the flow of water in pipes, which makes the loss of living force proportional to the sum of the squares of the sines of the bending; the total amount of curvature being divided into angles below forty degrees. The agreement of the results of computation, upon this principle, with those of the observations instituted to test its correctness is very close, and is entirely satisfactory.

Whoever claims to have discovered a new law of nature, or a new truth in science, must expect the claim to be subjected to a severe scrutiny. This scrutiny will be directed equally to the processes by which he professes to have been led to it, and to the results which follow from its application in cases where deductions from it may be tested by direct observation, or by the degree of their accordance with other truths already known. This has been anticipated by the authors of this report, and they have accordingly furnished, in the amplest form, the material for applying either of the tests above suggested. We can only indicate in outline the nature of the material, referring those who would sift it thoroughly to the report itself.

Measurements of the daily discharge of the river were continuously made for periods of twelve months at Carrolton, Louisiana, of eleven months at Columbus, Kentucky, of ten months at Vicksburg, and one and a half months at Natchez. Similar observations were made upon the Arkansas at Napoleon, for eleven months; and, besides these, many others less protracted were made upon the main river and its tributaries and outlets, from the Ohio to the Gulf. These measurements required the determination of the cross-section and mean velocity at each station for every day. The cross-sections were determined by soundings made at frequent intervals, in a line at right angles to the stream. The place of each sounding was fixed by observation with two theodolites, from the extremities of a base-line of from four hundred to one thousand feet in length, measured upon the bank. Two independent sections were sounded, two hundred feet apart; and soundings repeated on the same lines, at different intervals of time, showed that the bed underwent no sensible changes. Lines of level were also run up at the banks to points above the highest floods. From these data, with the daily gauge reading, showing the stand of the river, the cross-section was known for every day.

Velocities, both at the surface and beneath it, were ascertained by means of floats. In a river of such depth and power as the Mississippi, no kind of current-meter, or other contrivance involving mechanism, is available for sub-surface observations on velocity. The submerged floats were connected with surface floats very much smaller, by means of cords. The surface floats carried small flags which were observed in their transit across two lines at right angles to the river, two hundred feet apart, by means of theodolites at the extremities of a base of the same length on the shore. The point in which each float crossed each section line was thus fixed. As many observations as possible were made at all depths, and these were as equally distributed as possible throughout the breadth of the river. The paths of the floats were then plotted and grouped, by dividing the entire width of the river into spaces or divisions, each two hundred feet wide. The mean of the velocities of all the floats in each division was taken as representing the mean velocity of that division. For the shore divisions, when the floats were not well distributed through them, a slight correction was sometimes used. These mean velocities were then multiplied by the areas of their respective divisions of the cross-section; and the sum of the products was divided by the area of the entire cross-section, for the mean velocity of the river.

The process here described was employed in all the observations of 1851, the first year of the operations of the survey. When the work, after a long suspension, was resumed in 1858, the floats were all confined to the constant depth of five feet below the surface. The mean velocities at this depth, multiplied into their corresponding division areas, gave a result called "approximate discharge," which was reduced to the true discharge by being multipled into the ratio between the velocity at this depth, and the mean velocity in the entire vertical plane parallel to the current. This ratio had not been discovered while the earlier observations were in progress.

For the determination of the law of velocities below the surface, elaborate series of observations were made at Carrolton and Baton Rouge by means of floats at different depths, from boats anchored in the stream at various distances from the shore. All the observed velocities of each set, that is, from each anchorage, were then plotted in curves. Subsequently, for the purpose of eliminating errors of observation and accidental irregularities, plots were made of the means of selected groups of observations, each group embracing the observations which corresponded to nearly equal depths and nearly equal velocities of the river. The resulting curves sufficiently indicated the existence of law, without clearly betraying its nature. Still another mode of combination of observations was resorted to. This consisted in forming a grand mean curve by taking the means of all the velocities at *proportional* instead of absolute depths—that is at every tenth, every two-tenths of depth, and so on. This was done by plotting the mean curves before obtained, on a scale which so exaggerated the differences of velocity, as to make one-thousandth of a foot an appreciable quantity, and then drawing through them parallel lines at each tenth of depth, taking the values corresponding to the points of intersection as the velocities due to those depths. Each point in this grand mean curve was fixed by two hundred and twenty-two observations: a number sufficient to obliterate, almost completely, the remaining irregularities. Its form suggests the probability that it may be one of the conic sections. In order to test this suspicion, and, in case of its truth, to determine to which of the conic sections the curve is to be referred, the general equation,

$$y^2 = 2Px + R^2x^2$$

is assumed; which is the equation of an ellipse when R^2 is negative, of a hyperbola when it is positive, and of a parabola when it is zero. If, in this equation, we give to x and y each two determinate values, represented by $x_{,,}$ $y_{,,}$ $x_{,,,}$ $y_{,,,}$ we may eliminate P, and obtain the value of R^2 in terms of $x_{,,}$ $y_{,,}$ $x_{,,,}$ $y_{,,,}$. As the equation assumes the origin of coördinates to be at the vertex of the curve, the plotted or tabulated velocities are not themselves the values of x required. Regarding the curve as a graphic representation of the condition of things in a vertical plane parallel to the thread of the current, it will be obvious that it must present its convexity down-stream; and that its vertex will be the point where a tangent is perpendicular to the horizon, the axis being in the line of maximum velocity. This is at about three-tenths of the depth below the surface. The abscissas will then be the differences between the maximum velocity and the velocities at other points of the curve; and the ordinates will be the distances of those points from the axis, in decimals of depth of the river. The values of R^2 as computed from these data for every tenth of depth, though never absolutely zero, are always small, and are positive in a part of the curve and negative in the rest. A mathematically regular curve was not to be expected; but the approach to a parabola is so near as to warrant the conclusion that this is the curve according to which the differences of velocity are regulated, and according to which, therefore, the resistances are distributed through the moving mass.

The equation of the parabola is easily deduced. In the equation of the common parabola, if we assume particular values, $x_{,,}$, $y_{,,}$, for the coördinates, we shall obtain the expression,

$$x = \frac{x_{,,}}{y_{,,}^2} y^2 ;$$

and if an arbitrary increase, $= x_{,}$, be given to all the abscissas, this will become

$$x - x_{,} = \frac{x_{,,} - x_{,}}{y_{,,}^2} y^2 ; \quad \text{or} \quad x = \frac{x_{,,} - x_{,}}{y_{,,}^2} y^2 + x_{,}.$$

In applying this equation to the observations, $x_{,}$ is to be replaced by the maximum velocity, and $x_{,,}$ and $y_{,,}$ by the values of those coördinates at the points most distant from the axis. The values of x are then to be computed for all the depths at which actual observations of velocity have been made. The sums of the computed and observed values are then to be compared, and their difference, if any, divided by the number of points observed, is to be applied as a correction to $x_{,}$ and to all the values of x: an operation which amounts to moving the whole curve slightly along the axis, without altering its curvature. By varying the depth of the axis, or the position of the point $x_{,,}$ $y_{,,}$, it may easily be found where the closest accordance between the observations and the computations can be secured.

The equation of the grand mean curve of subsurface velocities having been obtained by the processes here described, the degree of its accuracy may be tested by comparing severally the values of the velocities computed by means of it, for all the points beneath the surface at which velocities were actually observed, with the tabulated mean observed values. The comparison as made furnishes the following results: The actual maximum velocity observed being in feet 3·2611, the greatest difference found between any computed and any observed mean value is only ·0066, and the least is ·0006. The sum of all the differences, taken without regard to sign, is only ·0245, which is less than three-tenths of an inch. This test is certainly very satisfactory; but it is corroborated by others to be presently mentioned.

The forms of the parabolas of subsurface velocities at high and low water, and at intermediate stages of the river, indicated a change of curvature consequent upon a change of mean velocity. The next object was to determine the law of change. The data which presented themselves were the high water mean, the low water mean, and the grand mean curves; to which another might be added by considering that if the mean velocity of the river be made zero, the parabola becomes a straight line. An attempt was made to investigate the relation of the parameters of these curves; but the data proved to be too limited for the purpose. The idea was then conceived of attempting the same investigation in regard to the velocities in *horizontal* planes; or at the surface of the river. Data for this inquiry had been amply furnished by the observations for daily discharge. These observations, which were made at a depth of five feet below the surface, were grouped according to the even feet of approximate mean velocity of the river; and thus were obtained material for eight mean curves, corresponding to as many different mean velocities. From these was deduced a grand mean curve, as in the case of subsurface velocities. The result was a very clear disclosure of the parabolic law.

In proceeding to the study of the law of variation of curvature, equations were deduced for each of the eight mean curves. The reciprocals of the parameters of these parabolas were plotted as ordinates, the corresponding mean velocities of the river being the abscissæ—the reciprocal of the parameter of the limiting parabola, or straight line, which is zero, indicating that the curve intersects the axis of abscissæ at the origin of coördinates. A curve resulted which conformed closely to the parabolic law; and thus furnished a general expression for the reciprocal of the parameter of any parabola of surface velocities corresponding to any given mean velocity of the river. This result was tested by applying it to the formation of a general equation for the curve of velocities five feet below the surface; and employing this equation to recompute the velocities corresponding to the eight mean curves which had been used as components in forming the grand mean. The differences were all small, though larger than those in the grand mean subsurface curve; a consequence probably of the fact that each of these eight curves was founded upon a much more limited series of observations than that. A law of parameters having been thus deduced for the horizontal curves, the probability naturally suggested itself that a similar law governs those of the vertical curves also. Materials which are insufficient to reveal the existence of an unknown law are often ample for the purpose of testing the fact of its existence, after it has been once suspected. We have seen that the only materials for a curve of parameters for subsurface velocities consisted of four pairs of coördinates. Of these the parameter and mean velocity of the grand mean curve were the pair best determined, next to those of the straight line, which fixed the intersection of the curve with its axis at the origin of coördinates. A parabola, passed through the two points thus ascertained, furnished means for constructing a general equation of subsurface velocities, similar to the general equation of surface velocities before formed. The general expression for the reciprocal of the parameter, in such an equation, is, of course, the ordinate of the parameter curve corresponding to the variable mean velocity of the river (v). Or, putting b for the parameter of the curve of parameters, and $\frac{1}{2P}$ for the ordinate,

$$\left(\frac{1}{2P}\right)^2 = bv; \quad \text{or} \quad \frac{1}{2P} = \pm (bv)^{\frac{1}{2}}$$

The value of b deduced from the equations of subsurface velocities, was 0·1856.

Referring to the equation of the parabola before given, as adapted to the case in which the curve intersects the axis at a point of which the coördinates are $x = x_{,,}$ $y = 0$, viz:

$$x = x_{,} + \frac{x_{,,} - x_{,}}{y_{,,}^2} y^2,$$

it will be seen that this value of $\frac{1}{2P}$ is the coëfficient of y^2, or

$$\frac{x_{,,} - x_{,}}{y_{,,}^2} = -(bv)^{\frac{1}{2}},$$

the negative value being required by the nature of the case, since $x_{,}$ is the maximum velocity in the vertical curve. Putting then V for the general value of the velocity in the subsurface curve, and $V_{d,}$ for the particular velocity at the depth $d_{,,}$ which denotes the depth of the axis, the general equation of the subsurface velocities is

$$V = V_{d,} - (bv)^{\frac{1}{2}} d_{,,}^2,$$

where $d_{,,}$ denotes the distance of the point whose velocity is V from the axis, expressed in decimals of the depth taken as unity.

This equation was subjected to a very thorough testing, by being applied to the mean high water and low water curves, and also to other series of observations not included in the formation of those curves, and to observations on the bayous of La Fourche and Plaquemine. In the mean high water curve, the mean difference between the observed and computed velocities, expressed in decimals of a foot, was ·0074, the maximum velocity in the curve being 3·8371 feet. In the mean low water curve, the same mean difference amounted to ·0127, the maximum velocity being 2·2523. This curve was not so well determined as the other, yet the mean difference is not a sixth of an inch. In medium-stage curves, deduced from 52 observations at Columbus and 20 at Vicksburg, the mean differences were ·0056 and ·0155 respectively; the maximum velocities in the curve being 4·1958 and 4·5709. In bayou Plaquemine the mean difference was ·036 on a maximum velocity of 6·491; and in bayou La Fourche ·003 on a maximum velocity of 3·250. In these two cases, the curves are deduced from the results of a single day's observation.

The same equation was also further tested by being applied to the original curves, which had been combined, as above stated, on the principle of *proportional* depths. The correctness of that principle of combination had not been quite certain; but the results of this final test left no doubt of its legitimacy. The mean discrepancy between the computed and observed velocities amounted in only one case to so much as one per cent of the maximum velocity in the curve; and in this instance, the absolute discrepancy was but a little over an inch. Usually the agreement was much nearer.

But a still more remarkable test of the accuracy of the law of parabolic velocities is furnished by a comparison of its results with observations made by Capt. Boileau at Metz, upon the flow of water in an open wooden trough only about two feet wide and one foot deep—the depth having been subsequently reduced to two-thirds of a foot. In these cases, the velocities observed in the vertical curve varied, in the first instance, from below two feet to nearly three; and in the second, from about one and a half to over two feet. The observations are recorded in the one case at fifteen different depths, and in the other at thirteen. The mean discrepancy, between these observations and the results of computation for the same points from the parabolic equation founded on them, amounted, in decimals of a foot, to only ·0330 and ·0243 for the two cases respectively. The largest of these mean errors is less than four-tenths of an inch.

It will be noticed that in all the computations made for the subsurface velocity of the Mississippi, in its different stages, and for the bayous, one and the same equation was constantly employed, viz:

$$V = V_{d_{\prime}} - (bv)^{\frac{1}{2}} d_{\prime\prime}{}^{2} = V_{d_{\prime}} - (0{\cdot}1856v)^{\frac{1}{2}} d_{\prime\prime}{}^{2};$$

the value of $V_{d_{\prime}}$ being, in each case, taken directly from the observations, and v being known by the measurements of discharge. In this proceeding it was evidently assumed that b is a constant, and always equal to 0·1856. The equations derived from Boileau's trough furnished data, not quite exact but nearly so, for obtaining a new value of b. One of the necessary data was wanting, which was the value of v, the mean velocity of the stream, but as this will not be very far in error if taken at eight-tenths of the velocity on the surface, it may be assumed to be well enough known for the purpose in hand. The numerical coëfficient of $d_{,,}^2$, in the equation of the parabola, being then divided by the so-assumed value of $v^{\frac{1}{2}}$, will give the square root of the value of b which is sought. This value is found from the equation of the trough to be a little above unity. In the equation employed in the previous computations, being that which had been derived from the grand mean curve of the Mississippi, the numerical value of b, as we have seen, was 0·1856. It appeared probable, therefore, that this quantity, that is, the parameter of the parabola of parameters, varies inversely as some function of the depth. For the sake of further testing the truth of this supposition, careful observations were made upon a feeder of the Chesapeake and Ohio Canal near Washington, having a depth of 7·1 feet and a width of 23; being, in these dimensions, much smaller than the river, and much larger than the trough. The mean maximum velocity observed was a little over two feet and a half, and the mean difference between the computed velocities for the several points observed and the means of the observations themselves at those points was less than a quarter of an inch. The equation of the parabola deduced from the observations gave a value of b equal to 0·58. The value of b (or of the parameter of the curve of parameters) changes, therefore, very slowly at considerable depths; and is practically at its minimum value for rivers when it equals 0·1856. The following expression is given as representing the observations:

$$b = \frac{1{\cdot}69}{(D + 1{\cdot}5)^{\frac{1}{2}}},$$

in which D denotes the depth of the river.

It being once established, that the curve of velocities in the vertical plane parallel to the stream is a parabola, and the equation of the parabola being known, the *mean velocity in the whole vertical curve* is easily deduced. The area representing the sum of all the velocities is made up of a rectangle and a parabolic segment above the axis, and of another rectangle and parabolic segment below. Dividing the sum of these areas by the total depth will give the mean velocity required. But the dividend in this case is an expression necessarily involving the depth of the axis as an element; and, although this mean depth had been found to be nearly constant, the actual depth is observed to vary. This variation is apparently dependent on the direction and force of the wind.

In the investigation of the effects of wind-force, the selected observations were divided into three classes—those in which the wind blew up-stream, those in which it blew down-stream, and those when it either did not blow at all or blew directly across the stream. The wind-forces were estimated according to the usual scale of notation, 0 being a calm and 10 a hurricane. For the first two classes of observations, the sum of the products of the numbers of observations at each point by the force of the wind was made out for each, and the difference between the two sums, divided by the total number of observations at all the points, was presumed to give the effective force of the wind. Five sets of determinations were thus obtained, in each of which the number of observations, the depth of the axis, the mean velocity of the river, and the resultant force and direction of the wind were given.

If x be the unknown depth of the axis due to a calm, and d, $d_{,}$, $d_{,,}$, &c., the observed depths, and if y denote the amount of movement of elevation or depression of the axis which would be produced by a wind of force 1, we may presume that for movements not greater than y the changes will be sensibly proportional to the force. Also, the weight of the several wind determinations will be proportional to the number of observations from which they are deduced. Finally, the legitimate effect of an up-stream wind will be to depress the axis, and of a down-stream wind to raise it. Let the resultant wind-forces among the data be denoted by f, $f_{,}$, $f_{,,}$, &c., then the movements of the axis will be fy, $f_{,}y$, $f_{,,}y$, &c. Accordingly, we shall have

$$x \pm fy = d, \quad x \pm f_{,}y = d_{,}, \quad x \pm f_{,,}y = d_{,,}, \text{ \&c.},$$

the negative sign being used when the wind is down-stream, and the positive, when it is up. Multiplying both members of each of these equations by the number of observations from which its constants were deduced, and adding the whole, member for member, we obtain one equation containing both x and y. But if we consider that the sum of all the movements of the axis, taken without regard to sign, must be equal to the sum of all the differences between x and d, x and $d_{,}$, &c., taken positively—that is, taking $x-d$ when x is the greater, and $d-x$ when d is the greater, we shall, by multiplying once more all these differences and the corresponding movements (fy, $f_{,}y$, &c.) by the numbers of observations to which they respectively belong, and adding the products as before, obtain a second equation containing both x and y. From these equations combined, both x and y are determined. The deduced value of x is ·317 which is the depth of the axis when the wind force is zero, in decimals of the total depth of the river. The deduced value of y serves to reduce the observed depths of axis to the position of calm. A comparison of the values of x so obtained (which should all agree with each other, and with the value of x given by the equation) verifies the general correctness of the procedure; the differences found being all very slight.

In these cases, the outstanding force of wind, after balancing opposing forces, was never much above 1. Velocity observations had, moreover, been found to be impracticable, with a wind above 4. The results of the investigation, so far, would hardly therefore justify the assumption that the movement of the axis is proportional to the force, for all degrees of force. Data necessary to set at rest the doubt which here arose seemed to be wanting. A happy expedient however presented itself for the removal of the difficulty. The daily "approximate discharges" of the river had been computed for each day's observations at Columbus, Vicksburg and Natchez, by taking the sum of the products of the several division areas by the velocity observed in each, five feet below the surface. These discharges, if accurately determined, would undoubtedly vary continuously; and if plotted as ordinates to a curve of which the dates are abscissas, should present a smooth and regularly varying curvature. But if the velocity five feet below the surface is affected by the wind, we may expect to see serratures in the curve, forming prominences when the wind is down-stream, and depressions when it is up. By examining the curve at the points of serrature, the amount in cubic feet of discharge may be estimated, which, by being added or subtracted, would restore the regularity of the curve. From these data and the wind record, it is easy to see that the amount of effect on discharge due to winds of different determinate forces, as 1, 2, 3, &c., may be deduced. This amount is found to vary with the mean velocity of the river.

In applying the principle just indicated to the determination of the effect, in raising or depressing the axis, of winds of different degrees of force, observations were selected of days when the wind record showed the several forces, 1, 2, 3, and 4, up or down the river. For these days a mean cross-section, a mean approximate discharge, a mean depth or *radius* of the river, a mean approximate mean velocity and a mean velocity five feet below the surface, were computed; and also the corresponding empirical correction of discharge was deduced from the observations of the same days. The unbalanced wind force was determined in the same manner as in the former investigation.

From the mode in which the approximate discharge is computed, it is evident that—the cross-section remaining the same—this discharge may be taken as proportional to the mean velocity five feet below the surface. Hence, if the originally computed or recorded approximate discharge be increased or diminished by the product of the unbalanced force of wind into the empirical correction corresponding to that force, the sum or difference will be the approximate discharge due to a calm, and may be called the *corrected* discharge. The truth of the following proposition will then be manifest:—As the recorded discharge is to the corrected discharge, so is the observed mean velocity, five feet below the surface, to the velocity which would have been observed at the same depth had it been calm. But, had it been calm, the axis would have been at the depth ·317. In the general equation of velocities, therefore,

$$V = V_{d_{,}} - (bv)^{\frac{1}{2}} d_{,,}^{2},$$

if $d_{,,}$ be put for the distance from the known position of the axis to the point five feet below the surface, and the velocity found for that point by the last proportion be put for V, there will remain only one unknown quantity, which is $V_{d_{,,}}$ or the maximum velocity in the vertical curve. This velocity is therefore easily deduced, and, being substituted in the same equation, will enable us to compute the *mean* velocity in the entire vertical plane. For this mean velocity, being derived from the areas of the rectangles and parabolic segments, above and below the axis, which form the figure bounded by the curve of velocities at one end and by a vertical line at the other, the extreme length being the maximum velocity in the plane, and its breadth the depth of the river, is determined when the velocities at surface and bottom are given along with the maximum velocity and depth. The last named velocity is that which was just found; and, by the help of this, the equation gives the other two, when the proper values of $d_{,,}$, viz., distance from the axis to the surface and distance from the axis to the bottom, are substituted.

This operation was performed for each of the wind-forces, 1, 2, 3, and 4. In each case, the unbalanced wind-force of the observations was but a fraction of the total force of the wind at the given intensity. It was desirable to know the effect due to the entire force; and, in order to arrive at this, it was only necessary to reverse the operation. Thus, taking the corrected approximate discharge as the discharge due to a calm, and increasing and diminishing it by the amount of the empirical correction corresponding to each wind-force successively, we may obtain the hypothetical discharge due to the full wind-force in question, down or up the stream. Then the proportion may be stated:—As the corrected discharge is to the hypothetical discharge, so is the velocity in calm, five feet below the surface, to the velocity at the same point under the assumed wind-force. The value so determined may be substituted for V as before, or for U on the left of the more general equation following; which differs from the former only in replacing V, the velocity in a particular plane, by U which is intended to denote the velocity in the mean of all planes parallel to the axis of the river,

$$U = U_{d_{,}} - (bv)^{\frac{1}{2}} d_{,,}^{2} = U_{d_{,}} - (0{\cdot}1856v)^{\frac{1}{2}} d_{,,}^{2}.$$

In this equation, $U_{d_{,}}$ and $d_{,,}$ are both unknown. The equation for *mean velocity in the vertical plane* contains (as above stated) values of U at the surface and at the bottom, both of which are expressible, from the equation just given, in terms containing only the same two unknowns, $U_{d_{,}}$ and $d_{,,}$.[2] There will therefore be two independent equations, involving only two unknowns, and from these the depth of the axis, on which the values of $d_{,,}$ depend, may be determined.

This determination having been made for the four wind-forces independently, the amount of displacement of the axis due to each wind-force is ascertained. The result is, that the displacement is directly proportioned to the wind-force, that it is equal for the same wind-force in opposite directions, but that it is a depression for an up-stream wind and an elevation for a down-stream wind. It also appears that the amount of displacement is independent of the mean velocity of the river. And although, in this investigation, the data for determining the effect of each wind-force are entirely independent of those for the others, yet the results exhibit a remarkably close agreement. As a result of the whole we obtain the following formula which is a general expression for the depth of the axis, (denoted by $d_{,,}$) f representing the force of the wind, and r the radius, or mean depth of the river:—

$$d_{,}=(0{\cdot}317+0{\cdot}06f)r.$$

Since the process just described furnishes the means of expressing the velocity at the surface or at any depth below it, in terms containing all the variables which affect its value, it is manifestly practicable to deduce a system of gauging a river in which the true discharge shall be obtained from observations at one unvarying depth. But, as the ratio of the so observed velocity to the mean velocity is a varying one, no system of gauging founded on this method of observation can be relied on, in which account is not taken of this variation.

There is, however, one velocity—the velocity at mid-depth—which bears a ratio to the mean velocity in the vertical plane which is nearly constant. In order to show the evidence of this, we must actually state the equation for this mean velocity to which we have several times referred. It will be necessary therefore to adopt the following symbols:

[2] The symbol $d_{,,}$ has, apparently, two distinct values as employed above:—it being used to stand both for the distance from the axis to the surface, and for the distance from the axis to the bottom. But, as the depth is known, these are simple functions of a single unknown quantity. We have endeavored, in the explanation of the principles of the report, to avoid the introduction of many symbols. We have not therefore, thus far, actually stated the equation for *mean velocity in the vertical plane* above referred to. The system of symbols employed in the report is very clear and expressive; but we have chosen to defer an account of it, until after completing our outline of principles and processes.

D=depth of river. d=distance below the surface (variable).
$d_{,}$=depth of axis (line of maximum velocity in the vertical plane).
m=depth of line of mean velocity in the vertical plane.
V=velocity at any point in the vertical plane.
$V_{d_{,}}$=velocity at the axis, or maximum velocity.
V_m=mean velocity in the vertical plane.
V_0=velocity at the surface. V_D=velocity at the bottom.

Then $V_m D$ will be the value of an area equal to the two rectangles and two parabolic segments concerned in the determination of the mean velocity. The truth of the following equation is therefore manifest:—

$$V_m D = \tfrac{2}{3}(V_{d_{,}} - V_0)d_{,} + V_0 d_{,} + \tfrac{2}{3}(V_{d_{,}} - V_D)(D - d_{,}) + V_D(D - d_{,}).$$

Which, reduced, becomes,

$$V_m = \tfrac{2}{3}V_{d_{,}} + \tfrac{1}{3}V_D + \tfrac{1}{3}\frac{d_{,}}{D}(V_0 - V_D).$$

Take now the general equation for the vertical plane, heretofore given,

$$V = V_{d_{,}} - (bv)^{\frac{1}{2}} d_{,,}^2 = V_{d_{,}} - (bv)^{\frac{1}{2}}\left(\frac{d - d_{,}}{D}\right)^2$$

and substitute in it the value of d at the surface, $=0$, and at the bottom, $=D$, and we have the two expressions following:—

$$V_0 = V_{d_{,}} - (bv)^{\frac{1}{2}}\left(\frac{d_{,}}{D}\right)^2. \quad V_D = V_{d_{,}} - (bv)^{\frac{1}{2}}\left(\frac{D - d_{,}}{D}\right)^2.$$

These values of V_0 and V_D being introduced into the expression for the value of V_m, we shall have, after reduction and transposition,

$$V_{d_{,}} = V_m + (bv)^{\frac{1}{2}}\left(\tfrac{1}{3} + \frac{d_{,}(d_{,} - D)}{D^2}\right).$$

And substituting this value of $V_{d_{,}}$ in the foregoing general equation for velocity in the vertical plane, there will result after reduction,

$$V = V_m + (bv)^{\frac{1}{2}}\left(\frac{D^2 - 3d_{,}D - 3d^2 + 6dd_{,}}{3D^2}\right).$$

Divide the identical equation, $V_m = V_m$, member for member, by this expression, and we obtain finally the general ratio of the velocity at any depth to the mean velocity in the vertical plane, viz:

$$\frac{V_m}{V} = \frac{V_m}{V_m + (bv)^{\frac{1}{2}}\left(\frac{D^2 - 3d_{,}D - 3d^2 + 6dd_{,}}{3D^2}\right)},$$

in which $d_{,}$ must be replaced by its value as deduced from the investigation of the effect of wind-force, in order that all the variables may explicitly appear.

If any value could be assigned to d in this expression, which should reduce the fraction in brackets to zero, it would follow that the depth m (which would then be d) is independent of the mean velocity of the river, v. This cannot be done; but if d be made $=\frac{1}{2}$D, the ratio is greatly simplified, and the equation becomes

$$\frac{V_m}{V_{\frac{1}{2}D}}=\frac{V_m}{V_m+\frac{1}{12}(bv)^{\frac{1}{2}}}.$$

Since both D and d, disappear from this expression, the ratio of the mid-depth velocity is independent both of the depth of the river and of the force of the wind. If the velocities in the mean of all vertical planes parallel to the current be represented by U instead of V, we shall have

$$\frac{U_m}{U_m+\frac{1}{12}(bv)^{\frac{1}{2}}},$$

in which, in a channel of rectangular cross-section, U_m would be equal to v. In point of fact, for the Mississippi, it is equal to $\cdot93v$, the coëfficient remaining sensibly constant. Substituting this value, the authors of the report have tested the formula, by computing the ratio,

$$\frac{\cdot93v}{\cdot93v+\frac{1}{12}(bv)^{\frac{1}{2}}},$$

for every even foot of velocity from 1 to 8, and employing the results in the computation of mean velocities from many mid-depth velocities actually observed. The observations include a number on the Mississippi and the outlet bayous, in different stages of the water, and also those made, as before mentioned, on the feeder of the Chesapeake and Ohio canal, together with others by Messrs. Hennocque and Defontaine on the Rhine, and finally those by Mr. Boileau on his experimental wooden trough. The position of the axis, among these data, varied from the surface to a point below mid-depth, and the mean velocities varied from a foot and a half to more than four feet. The differences between the observed and computed values of $\frac{V_m}{V_{\frac{1}{2}D}}$, however, were, for the most part, practically insensible, and in the few cases in which this was not true they amounted to but two or three per cent.

The near approach to constancy, and equality of the ratio between the mid-depth velocity and the mean velocity, is easily intelligible when the fact is once detected. The resistances to motion proceed from the perimeter; that is, from the bottom and the surface. The discharge remaining sensibly constant, and, in a uniform channel, the cross-section also, whatever retards the movement at one surface necessitates an acceleration at the other.

An up-stream wind, by diminishing the velocity at the surface, creates a slight increase of slope; and this would increase the mean velocity of the stream, but for the fact that, as much as the new slope would add, the wind destroys, so that the mean velocity remains constant. What the wind destroys at the surface must therefore be compensated at the bottom; and, as these effects are propagated through the mass according to the same laws as the ordinary resistances, the curve of velocities continues to be parabolic, but the level of its axis is changed.

The simplicity of this ratio suggests an improved method of gauging streams; but into these practical details it is not important that we should enter here.

We may here conveniently present, in a single group, the most important of the formulæ which result from the investigations of which we have been endeavoring to give an account. Those which have not been fully explained, are easily deducible from those which have.

$$V = V_{d_{\prime}} - (bv)^{\frac{1}{2}}\left(\frac{d - d_{\prime}}{D}\right)^2. \quad V_0 = V_{d_{\prime}} - (bv)^{\frac{1}{2}}\left(\frac{d_{\prime}}{D}\right)^2,$$

$$V_D = V_{d_{\prime}} - (bv)^{\frac{1}{2}}\left(1 - \frac{d_{\prime}}{D}\right)^2. \quad V_{\frac{1}{2}D} = V_m + \frac{1}{12}(bv)^{\frac{1}{2}}.$$

$$V_m = \frac{2}{3}V_{d_{\prime}} + \frac{1}{3}V_D + \frac{1d_{\prime}}{3D}(V_0 - V_D).$$

$$V_{d_{\prime}} = V_m + (bv)^{\frac{1}{2}}\left(\frac{1}{3} + \frac{d_{\prime}(d_{\prime} - D)}{D^2}\right).$$

$$V = V_m + (bv)^{\frac{1}{2}}\left(\frac{D(\frac{1}{3}D - d_{\prime}) + d(2d_{\prime} - d)}{D^2}\right).$$

$$d_{\prime} = (0{\cdot}317 + 0{\cdot}06f)r. \quad U_m = 0{\cdot}93v.$$

$$U_0 = 0{\cdot}93v + (0{\cdot}016 - 0{\cdot}06f)(bv)^{\frac{1}{2}}.$$

$$U_r = 0{\cdot}93v + (0{\cdot}06f - 0{\cdot}350)(bv)^{\frac{1}{2}}.$$

$$U_{d_{\prime}} = 0{\cdot}93v + \left((0{\cdot}317 + 0{\cdot}06f)^2 - 0{\cdot}06f + 0{\cdot}016\right)(bv)^{\frac{1}{2}}.$$

$$U = 0{\cdot}93v + \left(\frac{dr(0{\cdot}634 + 0{\cdot}12f) - d^2}{r^2} - 0{\cdot}06f + 0{\cdot}016\right)(bv)^{\frac{1}{2}}.$$

$$v = \left((1{\cdot}08U_{\frac{1}{2}r} + 0{\cdot}002b)^{\frac{1}{2}} - 0{\cdot}045b^{\frac{1}{2}}\right)^2.$$

(To be continued.)

ART. IV.—*Observations on some of the Double Stars;* by MARIA MITCHELL.

THE instrument with which these observations were made is a five-inch equatorial telescope, by Alvan Clark.

I have not attempted to measure the double stars of a distance less than 2″, even where I considered the telescope capable of the work, supposing that I should better meet their difficulties after longer practice in micrometrical measurements. Previous to October, 1861, the observations were made at Nantucket, in long. $4^h\ 40^m\ 25^s$, and lat. 41° 1′; since that time they have been made at Lynn, and I have called the longitude of my observatory $4^h\ 43^m\ 44^s$, the latitude 42° 28′. The measurements have been made near meridian passage, except in the case of the few northern stars. In determining the angles of position, the stars have been brought between the parallel threads of the micrometer. The powers used have varied from 75 to 200. Other things being the same, the later measurements are the more reliable, the skies of Lynn being much clearer than those of Nantucket.

I have taken great pains to notice the colors of the stars before my eye was fatigued, and have frequently noticed comparative colors. The terms yellow, pale yellow, ruddy, &c., are very vague; Sirius, α Geminorum and Capella are called white by observers, but they are decidedly unlike in color. If a color-scale, made from certain stars, could be adopted, to which other stars could be referred, the errors of eyes and observers would be eliminated; but an analysis of the ray from each star can alone decide the question of real likeness.

Name of Star.	Date.	Angle of Position.	No. of Obs.	Distance.	No. of Obs.	Remarks on color, &c.
35 Piscium,	1860, Jan. 2,	152°·2		11″·9	2	The color of the small star is peculiar; there is a brown mingling with its reddish light. The large star is light yellow. The two resemble those of γ Arietis. Air very good.
" "	1860, Jan. 3,	154 ·4	2			
	1862, Nov. 23,	149 ·4				Small star reddish-brown. Air not clear.
	1862, Nov. 30,	150 ·1		11 ·9	3	
38 Piscium,	1860, Jan. 9,	243 ·5	4			The stars differ some in color. The large one is yellow, the small one has a crimson hue mingled with the yellow. The night is good.
" "	1862, Nov. 23,	237 ·9	6	3 ·4		These observations are marked "good."
	" Dec. 4,			3 ·3	6	
35 Cassiopiæ,	1860, Nov. 8,	354 ·3	3	59 ·6	3	The colors are yellow and purplish-red.
	" Nov. 16,			59 ·6	5	The small star is of a very pretty dark red color.

Table continued.

Name of star.	Date.	Angle of Position.	No. of Obs.	Distance.	No. of Obs.	Remarks on color, &c.
179 P. I. Arietis,	1861, Jan. 22,	171°·1	2			Large star pale yellow, small one lilac. The stars do not differ much is size, but much in brilliancy.
98 P. III. Eridani,	1861, Jan. 27,	230 ·6	3	6″·3	2	The night poor. The large star is orange-yellow, the small one gray.
" "	1861, Jan. 30,	237 ·7	8			The air good, but the wind high. The large star is orange, strikingly so, when compared with the star 12′ south of it.
" "	1861, Jan. 31,	238 ·7	3			The night is good, and the measurements are considered good.
32 Eridani,	1861, Jan. 31,	347 ·2	4	5 ·6	2	The small star is pale blue, the larger orange-yellow.
" "	1862, Dec. 28,	350 ·3	4			The colors are very decided, yellow and pale green.
" "	1863, Jan. 1,	350 ·8	5			Colors yellow and green.
" "	1863, Jan. 2,	347 ·2				Air poor; the stars ran together.
" "	1863, Jan. 3,			6 ·3	7	
Σ 479,	1861, Feb. 4,	130 ·2	4	7 ·2	2	Night good. The smaller star is a little ruddier than the larger. The small star mentioned by Admiral Smyth is easily seen.
Σ 494,	1863, Jan. 17,	186 ·3	4			Stars alike yellow.
Σ 546,	1861, Feb. 17,	188 ·6	3	6 ·8	3	
Σ 559,	1861, Feb. 16,	93 ·1	3	3 ·8	3	Stars much alike, color pale yellow.
"	1861, Feb. 17,	97 ·1				The night is good, and the measurements are better than those of Feb 16.
Σ 653,	1861, Feb. 21,	227 ·5	4	18 ·1	2	A faint star, seen only by glimpses, precedes.
278 P. IV. Orionis,	1861, Feb. 6,	48 ·1	4	12 ·0	3	Colors pale yellow and ruddy lilac. Admiral Smyth gives the difference in size only half a grade, in 1833; they certainly differ more at present.
118 Tauri,	1860, Feb. 9,	195 ·9	2	5 ·6	2	The large star is yellow, the small one bluish. Night good.
" "	1860, Feb. 27,	195 ·0	2			Night poor. The large star pale yellow, the small one reddish.
A Orionis,	1860, Feb. 20,	43 ·2	3			Colors yellow and bluish.
23 Orionis,	1860, Mar. 6,	30 ·4	2			The small star is of a darkish color.
δ Orionis,	1860, Mar. 11,	A. B. 88 ·4	2			A pale yellow, B bluish, C yellow.
		D. F. 22 ·7	2			
Castor,	1863, Mar. 20,	241 ·0	3			The North and preceding star is ruddier and decidedly smaller than the other.
"	1863, Mar. 21,	242 ·7	4	5 ·6	2	I call the colors warm yellow and pale yellow.
Σ 1282,	1863, Mar. 26,	257 ·9	5	3 ·3	4	A fine pair. The colors are pale yellow; the smaller has the more color.
"	1863, Mar. 27,	257 ·6	5			Small star ruddier in color than the large one.
"	1863, Mar. 30,	259 ·7	3			Air poor.
τ′ Hydræ,	1863, Mar. 29,	2 ·4	3			High wind.
	1863, Mar. 30,			66 ·8	5	The stars are much winged.
Σ 1273,	1863, Apr. 1,	220 ·5	3			The star was difficult and the night very poor.
Σ 1311,	1863, Apr. 21,	201 ·9	3			

Table continued.

Name of star.	Date.	Angle of Position.	No. of Obs.	Distance.	No. of Obs.	Remarks on color, &c.
35 Sextantis,	1863, Apr. 27,	238°·9	3	6″·8	3	A blue color has been noticed in the small star, which I do not see.
	1863, May 1,	239 ·9	4	7 ·6	6	The large star is orange in color, the small star is pale, it may be called lilac.
ζ Ursæ Maj.,	1860, Apr. 30,	103 ·5	3			The stars are both yellow, but the small one has the deeper hue.
δ Corvi,	1862, May 25,	212 ·6	4			Air not very good.
	1862, May 26,	214 ·1	4			By daylight. The measurements are considered very good.
γ Virginis,	1861, June 14,	171 ·9	4			The air is tremulous and the stars run together. Both yellow. Observations marked "good."
	1861, June 18,	167 ·6	3			
	1862, June 17,	170 ·1	3			By daylight. The wind is high and the stars run together a good deal.
ξ Bootis,	1862, July 5,	320 ·6	3			The large star is pale yellow, the small one orange.
	1862, July 11,	324 ·3	4			By daylight. The measurements are not very good.
28 Aquilæ,	1859, Sept. 1,	175·17	2			Stars yellow and lilac.
" "	1859, Sept. 6,			52.12	4	
γ Delphini,	1860, Sept. 1,	273 ·9	2			Stars yellow and bluish.
Σ 2523,	1860, Sept. 18,	149 ·4	5	6 ·3	4	A beautiful pair, the stars very much alike and whiter than the stars commonly called "white." I notice a little blue tinge.
"	1860, Sept. 22,	151 ·6	2			
β Cygni,	1859, Sept. 7,	56 ·1	2			The colors are beautifully contrasted; orange and light blue.
ζ Sagittæ,	1859, Sept. 9,	313 ·6	2			
	1859, Sept. 29,	314 ·1	2			
	1860, Aug. 29,	312 ·9	5	6 ·9	3	The colors are pale yellow and very faint lilac.
57 Aquilæ,	1859, Sept. 30,	174 ·9	3	38 ·8		Stars yellow with a greenish tinge; the smaller star has the more green.
" "	1860, Aug. 26,	171 ·6	4	37 ·2	4	
Σ 2613,	1860, Sept. 21,			4 ·6	2	
"	1860, Sept. 22,	349 ·9	4	5 ·6	3	The stars are much alike, but the smaller is the lighter in color.
"	1860, Sept. 26,	349 ·7	5	4 ·3	3	
"	1860, Sept. 30,					I noticed the colors only. They are much alike, but the northern one is ruddier.
Σ 2621,	1860, Sept. 26,	223 ·6	3			The stars are smaller than those of Σ 2613, and there is more difference in relative size. The smaller star has a ruddy tint.
	1860, Sept. 30,					The small star seems to be of a bluish white color.
226 P. XX. Antinoi,	1860, Sept. 3,	211 ·0	6	4 ·6	4	Very good night. Colors yellow and blue. Stars very nearly of a size.
	1860, Sept. 10,	208 ·9	4	3 ·5	3	
ζ Aquarii,	1859, Nov. 23,	342 ·6	5			
"	1859, Dec. 12,	341 ·6	1			
	1859, Dec. 15,	343 ·2	2			
	1860, Oct. 27,	342 ·6	4			Stars much alike in size. Northern one smaller and yellower in color.
	1860, Nov. 1,	339 ·9	5	2 ·2	3	Some fog. Stars blurry.
	1862, Oct. 18,	339 ·4	4			Night poor. The stars run together.
	1862, Oct. 20,	341 ·9	4			Colors alike pale yellow.
	1862, Nov. 14,	341 ·1	4			

ART. V.—*On the Flora of the Devonian Period in Northeastern America;* by J. W. DAWSON, LL.D., F.R.S., Principal of McGill University, Montreal.[1]

[CONCLUDED FROM VOL. XXXV, P. 319.]

IN the course of the preceding pages, I have endeavored to notice points of general geological and botanical interest as they occurred; and it will now be necessary only to mention a few leading results, as to the Devonian Flora, which may be deduced from the observations above recorded.

1. In its general character, the Devonian Flora resembles that of the Carboniferous Period, in the prevalence of Gymnosperms and Cryptogams; and, with few exceptions, the generic types of the two periods are the same. Of thirty-two genera to which the species described in this paper belong, only six can be regarded as peculiar to the Devonian Period. Some genera are, however, relatively much better represented in the Devonian than in the Carboniferous deposits, and several Carboniferous genera are wanting in the Devonian.

2. Some species which appear early in the Devonian Period continue to its close without entering the Carboniferous; and the great majority of the species, even of the Upper Devonian, do not reappear in the Carboniferous Period; but a few species extend from the Upper Devonian into the Lower Carboniferous, and thus establish a real passage from the earlier to the later Flora. The connexion thus established between the Upper Devonian and the Lower Carboniferous is much less intimate than that which subsists between the latter and the true Coal-measures. Another way of stating this is, that there is a constant gain in number of genera and species from the Lower to the Upper Devonian, but that at the close of the Devonian many species and some genera disappear. In the Lower Carboniferous the Flora is again poor, though retaining some of the Devonian species; and it goes on increasing up to the period of the Middle Coal-measures, and this by the addition of species quite distinct from those of the Devonian Period.

3. A large part of the difference between the Devonian and Carboniferous Floras is probably related to different geographical conditions. The wide swampy flats of the Coal Period do not seem to have existed in the Devonian era. The land was probably less extensive and more of an upland character. On the other hand, moreover, it is to be observed that, when in the Middle Devonian we find beds similar to the underclays of the Coal-measures, they are filled, not with *Stigmaria*, but with rhi-

[1] Copied from the *Quarterly Journal of the Geological Society,* Nov., 1862.

zomes of *Psilophyton;* and it is only in the Upper Devonian that we find such stations occupied, as in the Coal-measures, by *Sigillaria* and *Calamites.*

4. Though the area to which this paper relates is probably equal to any other in the world in the richness of its Devonian Flora, still it is apparent that the conditions were less favorable to the preservation of plants than those of the Coal Period. The facts that so large a proportion of the plants occur in marine beds, and that so many stipes of Ferns occur in deposits that have afforded no perfect fronds, show that our knowledge of the Devonian Flora is relatively far less complete than our knowledge of that of the Coal-formation.

5. The Devonian Flora was not of lower grade than that of the Coal Period. On the contrary, in the little that we know of it, we find more points of resemblance to the Floras of the Mesozoic Period, and of modern tropical and austral islands, than in that of the true Coal-formation. We may infer from this, in connexion with the preceding general statement, that, in the progress of discovery, very large and interesting additions will be made to our knowledge of this Flora, and that we may possibly also learn something of a land Fauna contemporaneous with it.

6. The *facies* of the Devonian Flora in America is very similar to that of the same period in Europe, yet the number of identical species does not seem to be so great as in the coal-fields of the two continents. This may be connected with the different geographical conditions in these two periods; but the facts are not yet sufficiently numerous to prove this.

7. The above general conclusions are not materially different from those arrived at by Gœppert, Unger, and Bronn, from a consideration of the Devonian Flora of Europe.

Art. VI.—*Action of Bromine and of Bromhydric Acid on the Acetate of Ethyl;* by J. M. Crafts.[1]

Mr. Wurtz proposed to me to seek to obtain from the action of bromine on the acetate of ethyl a product of substitution represented by the formula[2] $C_4H_7BrO_2$, in order to study its reaction with the oxyd of silver in the presence of water. The treatment with oxyd of silver and water, of products of substitution of bromine and iodine in organic radicals, serves to replace these elements, and consequently the equivalent of hydrogen, whose place they occupy, by the peroxyd of hydrogen,

[1] Communicated to this Journal by the Author.

[2] C=12, H=1, O=16. Doubled atomic weights of C and O.

HO, and is a reaction which of late has attracted the attention of chemists.

In the case of the body, $C_4H_7BrO_2$, which I hoped to obtain from the acetate of ethyl, it was a question of some interest to determine whether this reaction would be represented by the first or the second of the two following equations:

$$\text{I.}\quad \left.\begin{matrix}C_2H_3O\\C_2H_4Br\end{matrix}\right\}O+Ag_2O+H_2O=\underset{\text{Glycol.}}{\left.\begin{matrix}C_2H_4\\H_2\end{matrix}\right\}O_2}+\underset{\text{Acetate of Silver.}}{\left.\begin{matrix}C_2H_3O\\Ag\end{matrix}\right\}O}+AgBr;$$

$$\text{II.}\quad \left.\begin{matrix}C_2H_2BrO\\C_2H_5\end{matrix}\right\}O+Ag_2O+H_2O=\underset{\text{Glycocollate of Silver.}}{\left.\begin{matrix}C_2H_2O\\HAg\end{matrix}\right\}O_2}+\underset{\text{Alcohol.}}{\left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}O}+AgBr:$$

in other words, whether the bromine had been substituted for hydrogen in the radical ethyl or acetyl.

As chlorine attacks the acetate of ethyl with great energy, and gives a series of products of substitution, commencing, however, only with the one containing two equivalents of chlorine, $C_4H_6Cl_2O_2$, it seemed probable that bromine would act in a similar manner, but with less violence, and that the first product of substitution, the body $C_4H_7BrO_2$, might be obtained if two equivalents of bromine for one molecule of acetate of ethyl were employed. The reaction would be represented by the equation:

$$\underset{\text{Acetete of ethyl.}}{C_4H_8O_2}+2Br=C_4H_7BrO_2+HBr.$$

The reaction of bromine proved, however, to be entirely different from that of chlorine on the acetate of ethyl. No product of substitution was formed, and therefore the question proposed above could not be resolved; but the results which I obtained in making the experiment possess sufficient interest to induce me to publish them.

Bromine dissolves in the acetate of ethyl with disengagement of heat, and a diminution of the original volume takes place after the mixture of two liquids has become cool; but no bromhydric acid is disengaged, and the mixture, even after it has been exposed to the diffuse light in the laboratory for several weeks, when distilled, passes over mostly between 60° and 80° C., and can be separated, by washing with a dilute alkaline solution, into bromine and acetate of ethyl unchanged.

If, however, one molecule of acetate of ethyl and two equivalents of bromine are heated together in a glass tube sealed by drawing the end to a point and melting it with the blast lamp, the color of the bromine disappears almost immediately at a temperature of 150°, or after twelve to twenty hours at 106°. No bromhydric acid is disengaged on opening the tube, and the liquid contains only a small quantity in solution, which is given off in heating it. The liquid obtained from several operations in sealed tubes was distilled. About one-half distilled at a temperature near 45° C.; the mercury then mounted rapidly to 200°, while only a small quantity of an acid liquid passed over; the residue was allowed to cool in the retort.

The first portion, distilling near 45°, washed with a solution of potash and dried over solid hydrate of potash distilled at 38°·5–39°, and had all the properties of bromid of ethyl. An analysis gave:—

	Found.		Theory, C_2H_5Br.
C =	22·66	- - -	22·02
H =	4·55	- - -	4·59

The portion that distilled at 45° to 200° consisted mostly of acetic and bromacetic acids: when diluted with water it deposited only a small quantity of bromid of ethyl, together with a few drops of another bromated compound, whose point of ebullition was higher, but of which not enough could be obtained to enable me to determine its nature.

The liquid, boiling above 200°, which had been left in the retort, on cooling was transformed in greater part into a crystallized solid. The crystals, pressed between folds of filter-paper, and then heated to 180° in current of carbonic acid to free them from bromhydric acid, possessed the properties of bromacetic acid, $C_2H_3BrO_2$. Their solidifying point was about 46°. A determination of bromine gave:—

	Found.		Theory.
Br =	57·60	- - -	57·55

The portion of the product, boiling above 200°, which did not solidify in the retort, was without doubt a mixture of bromacetic and bibromacetic acids; it contained 67 p. c. of bromine, while bibromacetic acid contains 73·4. The quantity of this latter acid produced in the reaction was small.

It will be seen from these data that the acetate of ethyl, instead of giving a product of substitution with bromine, is decomposed by it into bromid of ethyl and bromacetic acid. The chief reaction is represented by the equation:—

Acetate of Ethyl.			Bromacetic acid.		Bromid of Ethyl.
$\left.\begin{matrix}C_2H_3O\\C_2H_5\end{matrix}\right\}O$	$+2Br$	$=$	$\left.\begin{matrix}C_2H_2O\\Br\end{matrix}\right\}O$	$+$	C_2H_5Br

The bibromacetic acid is evidently a secondary product, arising from the action of bromine on the bromacetic acid, and it only remains to account for the formation of the small quantity of acetic acid which was also observed. Thinking that the formation of one might have been dependant upon that of the other, that is, that the bromhydric acid set free by the action of bromine on the bromacetic acid might have reacted upon a portion of the acetate of ethyl to produce bromid of ethyl and acetic acid:

$$\underset{\text{Acetate of ethyl.}}{\left(\begin{matrix}C_2H_3O\\C_2H_5\end{matrix}\right\}O} + HBr = \underset{\text{Acetic acid.}}{\begin{matrix}C_2H_3O\\H\end{matrix}\Big\}O} + \underset{\text{Bromid of ethyl.}}{C_2H_5Br\Big)}$$

I was induced to try by direct experiment whether this latter reaction really takes place.

Acetate of ethyl absorbs, at the ordinary temperature, about one and a half times its weight of dry bromhydric acid gas, but disengages it again almost entirely at a temperature near its point of ebullition. If, however, the saturated solution is heated to 100° for half an hour in a sealed tube, complete decomposition is effected; the contents of the tube separate into two layers, the upper consisting of bromid of ethyl, and the lower of acetic acid, containing enough bromhydric acid in solution to make its density greater than that of the bromid of ethyl. Thus the acetate is actually decomposed by bromhydric acid in the manner suggested above.

There is also another reaction which might have played a part in the one first studied, and have contributed to the formation of acetic acid, namely, that between bromacetic acid and the acetate of ethyl. Perkin and Duppa[3] state that bromacetic acid displaces acetic acid from its combinations with bases; an analogous reaction might also take place between the former and the combinations of acetic acid with alcohol radicals; but the absence of bromacetate of ethyl from among my products, as well as the following experiment, which shows that this double decomposition is only partially effected at a higher temperature than that employed in the reaction of bromine upon the acetate of ethyl, prove that the reaction in question did *not* play any part in the formation of the acetic acid observed above.

Experiment.—One equivalent of bromacetic acid in crystals, together with an equivalent of acetate of ethyl, was heated in a sealed tube to 180° during three hours; at the end of this time the greater part of the crystals had disappeared, and, on distilling the contents of the tube, besides acetate of ethyl and bromacetic acid, a considerable quantity of an acid liquid distilling between 100° and 170° was obtained. This liquid was diluted with a large quantity of water, and the portion which did not dissolve was washed with a very dilute solution of caustic potash, and dried over chlorid of calcium; it began to distil at 80°, at which point a little acetate of ethyl passed over, the temperature then rose rapidly, and remained constant between the limits, 156° to 158°, while the greater part of the liquid distilled. An analysis of this latter portion gave:—

	Found.	Theory, $C_4H_7BrO_2$.
C =	29·35	28·74
H =	4·46	4·19

[3] *Quart. Journ. Chem. Soc.*, xi, 22.

The boiling point of bromacetate of ethyl is 159°, and its properties correspond with those of the liquid obtained above; the slight difference in boiling point and composition arises without doubt from traces of acetate which the liquid still contained.

The portion which dissolved in water was a strong acid; the solution, neutralized with carbonate of lead and evaporated over sulphuric acid, gave large and well formed crystals, whose form and angles, measured with a hand goniometer, corresponded to those described by Gerhardt as belonging to acetate of lead. The double decomposition which takes place here is represented by the equation:—

$$\underset{\text{Acetate of ethyl.}}{\left.\begin{matrix}C_2H_3O\\C_2H_5\end{matrix}\right\}O} + \underset{\text{Bromacetic acid.}}{\left.\begin{matrix}C_2H_2BrO\\H\end{matrix}\right\}O} = \underset{\text{Bromacetate of ethyl.}}{\left.\begin{matrix}C_2H_2BrO\\C_2H_5\end{matrix}\right\}O} + \underset{\text{Acetic acid.}}{\left.\begin{matrix}C_2H_3O\\H\end{matrix}\right\}O.}$$

The decomposition of acetate of ethyl by bromine is analogous to that which Mr. Gal[4] has observed with anhydrous acetic acid and chlorine:—

$$\underset{\text{Anhydrous acetic acid.}}{\left.\begin{matrix}C_2H_3O\\C_2H_3O\end{matrix}\right\}O} + 2Cl = \underset{\text{Chloracetic acid.}}{\left.\begin{matrix}C_2H_3O\\Cl\end{matrix}\right\}O} + \underset{\text{Chlorid of acetyl.}}{C_2H_3OCl}$$

and this reaction is another among the many which tend to demonstrate the close analogy existing between the composition of the ethers and that of the anhydrous acids.

Paris, March 3, 1863.

ART. VII.—*New Facts and Conclusions respecting the Fossil Footmarks of the Connecticut Valley;* by EDWARD HITCHCOCK.

I HAVE devoted a considerable time during a few years past to the preparation of a Descriptive Catalogue of the Fossil Footmarks in the large collection of Amherst College. Moreover, during the past winter (1862–3) I have made a large addition to the Cabinet by the purchase of Mr. Roswell Field's private collection. Unexpectedly, many new facts have been brought to light, not contained in my "*Ichnology of New England,*" published by the Government of Massachusetts in 1858, which have an important bearing on the fundamental principles of this science. Though I have been led to give up ten or a dozen of my old species of footmarks, I have described over 30 new ones, in a paper lately read by me before the American Academy of Arts and Sciences in Boston. But it is the results as to the principles of the science contained in that paper, which seem to me of most importance, and I venture, without asking leave of the Academy, to send you a few pages of the latter part of the paper for this Journal. The paper is largely illustrated by drawings

[4] *Comptes Rendus*, liv, 570.

and photographs, and, should the Society publish it, these I think will fully sustain the conclusions at which I have arrived.

The collection of Footmarks at the College, whose examination has led to the following unexpected conclusions, is now quite large. I have counted the number of individual tracks and found them to be over 20,000; but several thousands of them are the tracks of insects and small crustaceans. It was, however, some of the specimens in the collection purchased the past winter, by the generous contributions of the friends of science, that first opened my eyes to the facts detailed below.

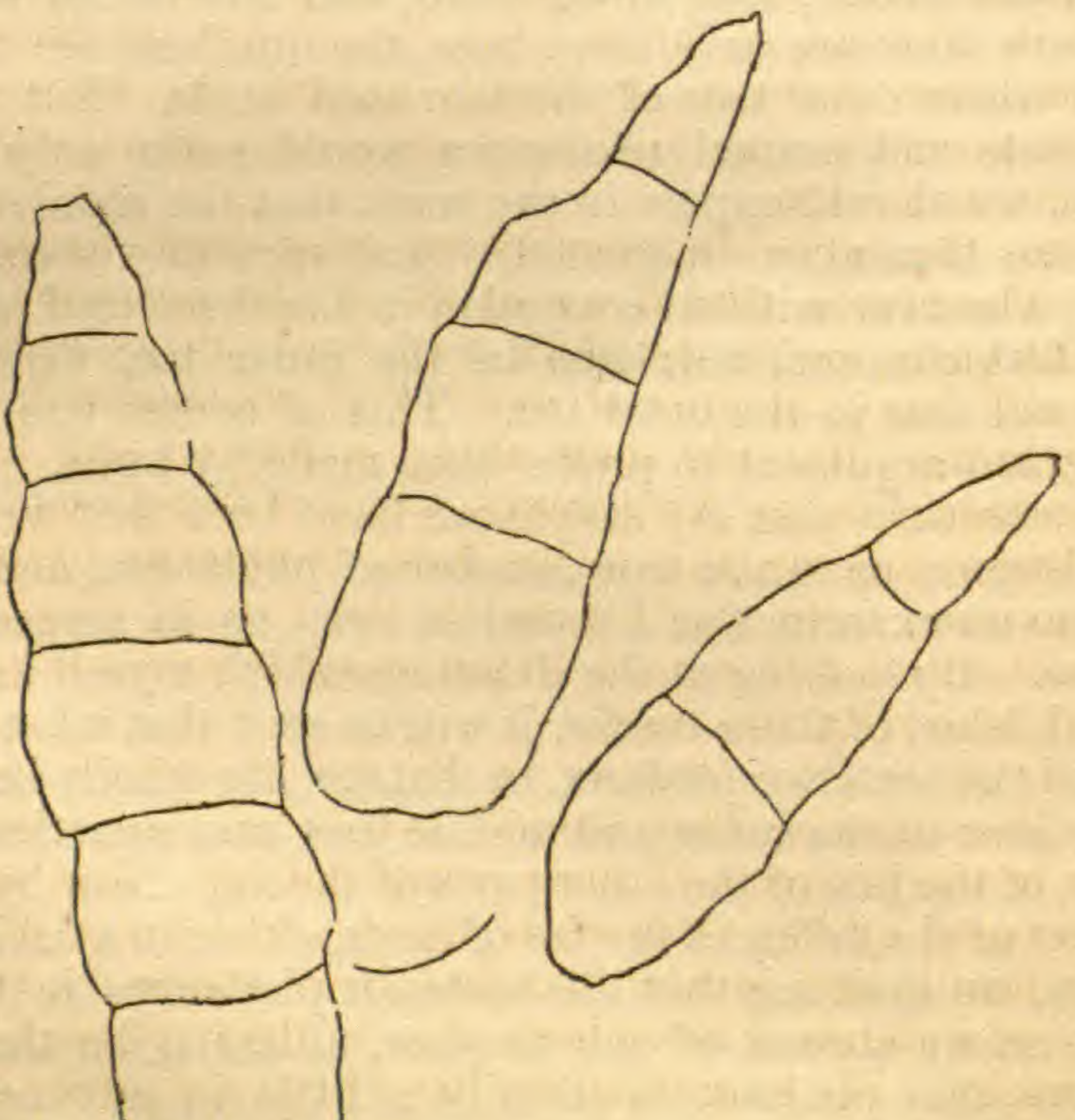
Supposed Mistake as to the number of Phalanges in some of the Lithichnozoa.—It is well known that the number of phalanges and their order, in the toes of living birds, enable the anatomist to distinguish them from other animals, with only a few exceptional cases. In four-toed birds it is two in the inner toe, three in the second, four in the third, and five in the outer toe, and where there are only three toes, the numbers are the same as in the three outer toes of the four-toed birds. But since the penultimate and ungual phalanges would make only one impression, we should expect in the track that the numbers would be one less than above indicated. And such they seemed to be to every observer without exception in the three-toed pachydactylous Lithichnozoa, viz: two in the inner toe, three in the middle and four in the outer toe. This of course was regarded as the grand argument to prove them made by birds.

For some time past my suspicions have been that we have all been mistaken as to the true number of phalanges, and when I went into an examination I found it even so in respect to the outer toe. By looking at the drawings which myself and others have published of these tracks, it will be seen that what we have supposed the posterior phalanx, in that toe, lies wholly behind the first phalanx of the inner and middle toes, and sometimes also a little out of the line of the other parts of the toe. Now by looking at the feet of the different species of birds, either in a cabinet or in drawings, we shall see that the posterior phalanges in the three toes lie nearly abreast of one another, unless it be the middle toe where this phalanx is usually a little in advance. This posterior impression, behind the outer toe, was not, therefore, made by a phalanx, but probably by a process of the tarso-metatarsal bone. We accordingly sometimes find a similar posterior impression behind the inner toe, and indeed a thin and smaller imprint of this sort shows itself sometimes, as on the sketch on page 782 of my Ichnology, and on figs. 21 and 22 of the present paper.

This fact, I confess, very much unsettled my conviction that any of the Lithichnozoa were birds. And they were still farther shaken by the facts I have already detailed respecting that most anomalous animal, the Anomœpus. The trifid tracks of its hind feet had been mistaken by us all for those of birds. Indeed, the number of phalanges in the toes were found without much doubt to correspond with those of living birds, and also with those Lithichnozoa which I had regarded as birds. But the Anomœpus had been proved without question to be four-footed: Are we not forced to the conclusion that all the Lithichnozoa with similar trifid feet must be quadrupeds?

Another development as to the Phalanges.—Probably I should ere long have come to this conclusion, had not another discovery awaited me. Among the new specimens purchased was one very beautiful row of thick-toed trifid tracks, such as we had been in the habit of supposing made by birds; but I have little doubt that they were those of an Anomœpus, though no marks of fore

1.



feet or tail are seen. I have named it as a new species (the *A. curvatus*) of that genus, though differing but little from the *A. intermedius* already described. On looking at these tracks I was surprised to find in the outer toe, *four very distinct phalangeal impressions besides the posterior imprint*, which I now look upon as made by a heel bone not a phalanx. The sketch annexed gives an exact outline of one of these tracks, and on examining the remarkable slab of *Anomœpus intermedius* already described in this paper, I found on that, also, evidence that in some cases the outer toe had four phalangeal impressions beside the heel bone. So far as the Anomœpus is concerned, then, I feel sure that we have in its phalangeal impressions the normal number and order in the feet of living birds. I was at once led to inquire whether the same thing might not be true in respect to those thick toed Lithichnozoa which I have regarded as birds. I have found proof enough to satisfy myself that it is so, and that the reason the fact has been overlooked is that the penultimate and ultimate phalanges (omitting the ungual) rarely made separate impressions. But occasionally I can see a faint line of demarcation between them. But I had frequently noticed that the length of the ultimate phalangeal impression on the outer toe (as a reference to the outlines of these tracks in the *Ichnology* will show) was as long as, and sometimes longer than, those which preceded it, whereas, so far as I have examined the osteology of birds' feet, they decrease in length toward the extremity. I think that generally two phalanges have been mistaken for one, in this part of the toe.

If these are probable conclusions they lead to important results. The first is, that if we strike off the posterior impression of the outer toe in the thick-toed bird tracks referred to, we shall still have the normal number of phalanges in the feet of living birds. But the same thing is proved still more decidedly in regard to the Anomœpus, which is four-footed. Hence the conclusion follows, that in the fossil footmarks birds cannot be distinguished from quadrupeds by the number of phalanges. This law of correlation among living animals would seem not to have been true with the fossil.

How far do the Protuberances on the feet of animals correspond with the Phalanges?—This subject could not but engage my attention in the progress of these investigations. But not finding it discussed by any anatomical author, and being prevented by feeble health and winter weather from access to any large collection of animals, I have been able to arrive at only unsatisfactory results. My examinations have been confined chiefly to the feet of birds, and the following facts have been obtained. The most important question under consideration is this:—Is it the phalanges or the articulations of the toes that make the deepest impression on mud or other plastic material trod upon? This will be determined by finding under which of these parts the protuberances are the most prominent. If under the phalanges, the number in the toe will be one less than if under the articulations; that is, if we count, as one of them, the articulation with the tarsal or tarso-metatarsal bone.

The protuberances on the foot of the turkey, both wild and tame, correspond neither with the phalanges nor the articulations, but are more numerous than either. The same is true of the domestic hen. There is a general resemblance, however, in this respect, between different individuals of these genera.

In the *Botanus lentiginosus*, the protuberances seem to correspond with the articulations, or joints.

In the Coot, the wings along the toes expand most in the middle of the phalanges.

In the Crow, the correspondence seems to be essentially with the articulations, judging from some tracks of this bird on clay in the cabinet. But the Struthionidæ have feet more nearly resembling the tracks under consideration. And in the *Rhea Americana* or South American Ostrich, although these protuberances are tolerably distinct on the middle toe, yet the inner and outer toes do not show them. A large heel shows itself behind the middle toe.

(Casts of the feet of the above bird were exhibited to the Academy).

These few examples show that there is a great diversity among living birds in the matter under consideration. Sometimes the protuberances correspond with the articulations, sometimes to the phalanges, and sometimes to neither. But I have never found feet that would make such distinct and marked tracks, and with always the same number of rounded impressions, as did the thick toed Lithichnozoa; and I am still inclined to believe that such was the structure of their feet that their tracks would show the number of phalanges rather than of the articulations. It could not be the latter, if the views I have presented in this paper as to the posterior imprint in the outer toe be correct; for that impression is entirely behind the phalanges on all the toes. I could wish, however, that I had time, strength, and opportunity, to pursue this subject farther among existing species of animals.

But, though my researches have been unsatisfactory on the particular point above mooted, they seem to me to have settled another of much interest. I do find protuberances on the feet of birds, especially the tridactyle species, behind the phalanges, such as might well have left those impressions on the tracks which we have mistaken for the posterior phalanx. We are thus relieved from the necessity of supposing anything peculiar in the processes of the tarso-metatarsal bone in the fossil animals.

The Feathered Fossil of Solenhofen.—The recent discovery of a remarkable animal, called by some Griphosaurus, and by others Archæopteryx, in the famous lithographic quarries of Solenhofen in Bavaria, throws some light, I think, upon the thick toed Lithichnozoa, while these reflect some light upon the feathered fossil. For it had feathers, yet some of the ablest zoologists pronounced it a reptile. Others, however, as Prof. Owen of London and Prof. Dana of New Haven, believe it to have a predominence of ornithic characters, so as to make it a bird. Some important parts of the skeleton are wanting, as the head, neck, dorsal vertebræ, and sacrum, and the ribs are detached and scattered about. The forearm consists of radius and ulna, a metacarpal bone, and a few detached small fingers; also two small slender bones with sharp claws like those on the hind foot, which may have been used for clinging, or as weapons of offense.

The lower right limb consists of a femur, tibia, and tarso-metatarsus, to which one hind toe and three foretoes are articulated, the phalanges being one, two, three, and four, though the last number is a little doubtful, on account of the position of the outer toe. The toes are all armed with sharp claws.

The tail is six inches in length and consists of twenty vertebræ, of narrow elongated form, diminishing in size to the last. The feathers of the tail are attached in pairs to each vertebra throughout its entire length.

Now between these characters and those of some of our Lithichnozoa there are some remarkable analogies or resemblances, so far as I can judge, and which I would now indicate —at least such as have arrested my attention, with some of the inferences that follow. It is perhaps unexpected that they ally the Archæopteryx rather to the quadrupedal Anomœpus than the biped tridactyles in my *Ichnology*.

1. In both we have on the hind foot three front toes articulated to a stout tarso-metatarsal, and not as in all animals except birds, to a tarsus of several bones. This resemblance applies also to the biped, thick-toed, tridactyle Lithichnozoa, as well as to the Anomœpus, for they must all have had tarso-metatarsals below the tibia and fibula, though no impression among the tracks indicate any such bone. But we have the most decisive evidence that these animals had only three toes, and where in existing nature do we find that number articulated with anything but a tarso-metatarsal, except a few cases in the Ruminantia and Solipedia.

2. They both had the same number of phalanges in the three front toes, though a little doubt remains as to the outer toe of the latter. The same number of phalanges existed in biped Lithichnozoa so far as we can judge by their tracks.

3. The posterior extremities of both, as far as the tarsal joint, correspond exactly with those of living birds: hence the tracks of the hind feet of the Anomœpus, as well as those of the trifid Lithichnozoa under consideration, are pronounced at once, on first seeing them, to have been made by birds; and it is only when we occasionally see where the Anomœpus brought its fore-

feet to the ground, that we suspect it could have been four footed.

4. Precisely how much correspondence there may be in the anterior extremities of the two animals we cannot decide. The Archæopteryx is thought to have had but one metacarpal bone, and the fingers are so scattered that their number is not given, but two are described as slender with long claws. The most perfect track of the fore-foot of the Anomœpus has five toes, the two hindmost showing two phalanges, the third, four, the fourth, three, and the farthest two. The four last toes at least show small claws. The fingers are arranged so as to be fan-shaped, all pointing more or less outward, resembling an expanded wing. But they seem to be genuine fingers, and there is no appearance of feathers on any of the tracks, on the hind or fore-feet. The figure annexed shows an outline of the most perfect track of the fore-foot yet found.

2.

This certainly looks more like the fore-foot of a lizard, and still more like that of some mammals, than the forearm of a bird, and it is difficult to conceive how it could have been used as an organ of flight, though possibly it might have been employed for prehension. But on the other hand we have conclusive evidence that it was not used for walking, except perhaps occasionally, and imperfectly. The right and left anterior feet that made the tracks were placed almost invariably nearly abreast of each other, as if the animal were resting, and not in alternation as in walking. But of more than forty steps of *Anomœpus intermedius*, shown on the remarkable slab described in this paper, the fore-feet show themselves only twice, and that when the animal rested. Indeed, we may safely assume that the principal object of the fore-feet was not locomotion, and the same remark is applicable to other species, even the gigantic Otozoum. What other purpose in the economy of these animals could have been subserved by such a structure, except perhaps prehension, I will not attempt to decide. Yet the fact has awakened an inquiry whether such a structure may not have existed in an animal whose predominant characteristics were those of a bird.

5. But there was a tail, and how shall we reconcile that fact with an ornithic character? It might have been impossible before the discovery of the fossil at Solenhofen. But that animal had a tail six inches long with twenty vertebræ, and yet the most eminent zoologists regard it as a bird. The characters of the tail in the Anomœpus are very peculiar, yet there are some curious resemblances between its markings on stone and the tail of the Archæopteryx. The traces of the tail of the Anomœpus

have three distinct phases. The largest species left a heart-shaped indentation, which was repeated every few inches. Would not such impressions be just what we might expect if this animal had such a short blunt tail as the Archæopteryx? And does it not suggest one of the uses of such a tail, viz: to furnish the animal with a sort of third hind-foot to help sustain it while it might use its fore-feet perhaps for seizing upon objects above and around it.

The tail of the *Anomœpus intermedius*, although rarely leaving an impression, did sometimes drag along and make a narrow continuous trail. This would indicate greater length and perhaps tenuity. But how much of attenuation and elongation might be consistent with an ornithic type we have no means of knowing. Prof. Dana speaks of a posterior elongation of the body as "connected profoundly with inferiority of grade in the different types of animal life," and says, that "it is the very one of all abnormal features to be looked for in the early birds."

Upon the whole the singular markings of the tail upon stone, with the exception perhaps of *A. intermedius*, do really suggest a curious coincidence between the the caudal extremity of this genus and that of the Archæopteryx.

Just as I had reached this point in my conclusions, a curious development awaited me. In examining some new specimens, a singular trail showed itself upon one which I had never before noticed; or if I had seen it, I had not connected it with the tracks, but considered it among those inexplicable markings due perhaps to water and wind, which so frequently puzzle the student of ichnology. But in this case there is a series of some six or seven rather flat and broad grooves, each one or two tenths of an inch wide, and the whole forming a trail more than an inch wide, running across the entire specimen, passing over one very distinct three-toed narrow-toed track, which is half an inch deep, and the grooves show themselves on opposite sides of the footmark, certainly two thirds of its depth, appearing as if some flipper-like appendage had dragged behind the animal, capable of easily conforming itself to the irregularities of the surface. The fact, that the marks follow the depression of the track, shows that they were made subsequent to the track, and suggests at once the idea of a broad and singular tail. What a pity it is that there is only one track upon the specimen: but so far as I can judge, the trail runs in the direction in which the animal was moving.

In these conclusions I should have acquiesced with considerable confidence, had I not found, on examining our new specimens, as well as others in the cabinet, that we had quite a number with similar markings, and that the trails in these do not always follow the line of tracks, but are sometimes on one side of it and sometimes on the other; now and then on both sides, and then crossing the line of tracks, so as to seem to have no connection with them. In general, however, it seems as if some flipper-like appendage to the posterior part of the animal had been thrown out on one side and the other, making sweeps occasionally, so as to leave curved trails. The species of track with which they are most usually associated is the *Anisopus gracilior*, described in the first part of this paper. In this species both hind and fore-feet are almost always shown on the stone, and it is quite obvious that the flipper-like impressions have no connection with the feet. They seem also rather large for the tail of so small an animal, whose feet are all less than an inch. I have hence been sometimes inclined to believe that the trails were made by some animals swimming along near the bottom, and occasionally striking and grooving it with its flippers or fins. But my more mature conviction is, that they are connected with the tracks. But it needs a series of expensive drawings to make the facts fully understood without specimens.

6. But to return to the Anomœpus; which characters shall we now regard as predominating in its structure and movements, those of the bird, or those of the lizard, or of the mammal? It is difficult to avoid the conclusion that the ornithic characters are the most numerous and striking. It may, after all, have been a bird, of so low a grade, however, that, even with its skeleton before him, the anatomist would hesitate where to place it, as in the case of the Archæopteryx.

7. This conclusion, to which the facts and reasoning have conducted me, not without remaining doubts, would, not long since, have appeared very absurd. But, if it could be admitted, see what a relief it would give to difficulties. If the Anomœpus were a lizard, or marsupial, we must give up that firmly established law of correlation which enables us to distinguish different classes of animals by the number and order of phalanges; but if it were a bird, that law can still be reckoned upon among the fossil as well as living animals. If a bird, we can see also how it was that it generally walked upon two feet, although it had another pair, to be used perhaps for several purposes, but rarely for locomotion.

8. If we can presume that the Anomœpus was a bird, it lends strong confirmation to another still more important conclusion, which is, that all the fourteen species of thick-toed bipeds, which I have described in the *Ichnology*, and in this paper, were birds. In this case, if we can retain the law as to the phalanges, all the characters of the animal, as made known by their tracks, belong to birds, with little variation from the existing bird type. They were bipeds unquestionably. Since they are the most abundant of the tracks, I have now seen thousands of them, and had fore feet existed I am sure they would occasionally have left some

trace of them, as is the case with every other species of Lithichnozoa. They had but three toes: at least, if a fourth existed in any case, it must have been articulated so high as not to reach the ground. These three toes are articulated to a tarso-metatarsus, as is the case with nearly all tridactyle animals. They had the same number of phalanges as the birds.

The impressions left by the cushion beneath those processes of the tarso-metatarsus which form the heel correspond to those which living birds would make, and, so far as I have examined, not to those of any other class of animals, though my examinations on this point have been few. The claws and papillæ agree essentially with those of birds. Finally, the great length of stride in some cases, and the position of the tracks nearly on a right line, indicate long legs of wading birds, and not any other kind of animal.

Most of these arguments are good for the ornithic origin of these tracks, whatever opinion we may entertain as to the Anomœpus. The only difference is, that, if we regard it as a reptile, the argument from the number of phalanges must be given up; if as a bird, that strong evidence is retained. But even without this, I cannot hesitate to reckon the biped thick-toed Lithichnozoa as birds; for I see no characters in their tracks that ally them to any other animals. I must consider them not only as birds, but as forming a quite perfect type of birds for sandstone days. The analogies taught us by palæontology (see Prof. Dana's appended letter) would lead us to expect also in the same period a lower group of birds, and these may have been the Archæopteryx and perhaps the Anomœpus, with some other genera of Lithichnozoa which I might name.

How then could I avoid the conclusion that these animals were birds? Doubtless with some peculiarities of structure, bringing them into the "*comprehensive types*" of Dana, but still decidedly birds. When I began this paper, and ascertained that we had probably made a mistake as to the number of phalanges, I felt as if this opinion, which I have always maintained, was becoming doubtful. But new examinations brought new facts to light, and the history of the Solenhofen fossil added others, until it appears to me we may now with more confidence than ever maintain the *avian*[1] character of these animals. It is certainly gratifying even to seem to touch soundings, after having been so much tossed on the sea of difficulty, and I cannot but hope that subsequent researches will show that we have not cast anchor merely in quicksand.

Prof. Dana's Views.—Having occasion while engaged in the investigations detailed in the preceding paper to write a letter on business to Prof. J. D. Dana, I mentioned some of the results to

[1] A new and much needed word used by Professor Richard Owen in a recent letter, and which I venture to introduce into print.

which my mind was coming. His reply contained too much good reasoning and important suggestions to be lost, and I venture, without his leave, but trusting he will excuse me, to annex his remarks to this paper.

"New Haven, Feb. 7th, 1863.

My Dear Sir:—Your new results from recent researches among the tracks of the Connecticut valley are of great interest, and I should be glad to put your conclusions, when you are ready with them, in the Journal. I am satisfied that we cannot infer the form and character of the earliest birds from those of the present day. The early type is evidently one of the mixed types ('comprehensive,' as I have called them) which diverged widely from the normal type—just in fact as the Ganoids diverge from the Ctenoids and Cycloids—the Marsupial from ordinary Mammals—and Amphibians (or Batrachians) from true Reptiles. You know that in the class of Mammals the Marsupials are *semi-oviparous*, or intermediate between the true viviparous mammals and the oviparous birds and reptiles. So again the Amphibians are intermediate between true reptiles and fishes, having gills when young like fishes, etc. Now by the recent discovery of the feathered fossil of Solenhofen, we have a corresponding inferior division of birds intermediate between birds and reptiles. Thus each class has its great typical group and its inferior abnormal group, related to the class next below. This being so, wide divergencies of form in the abnormal group are to be looked for.

There is another principle bearing on this subject—*the remarkable harmony among the types of any era through the past ages.* Thus the coal plants are made up mainly (1) of the highest Cryptogams, that is, the Acrogens, of which the Fern is the typical group, (2) the lowest Phænogams, the Conifers, and (3) intermediate (or comprehensive) types in each class, Lepidodendra of the Lycopodium tribe, a type coniferous in habit, and Sigillariæ of the Phænogams, also intermediate between Conifers (in the Gymnosperms) and the Lepidodendra. By such an assemblage, the flora was rendered remarkably harmonious. Had the progress of life consisted in an advance of Cryptogams to Mosses, along with the introduction of Conifers, it would have been far otherwise.

Again, (1) the Reptilian fishes (Ganoids), (2) and (3) fish-like higher Reptilians (Marsh's Eosaurus) made up a harmonious assemblage of *animal* life in the Carboniferous age. Again, the semi-oviparous Mammals (Marsupials) along with oviparous Reptiles, &c., were in harmony with one another; and if true non-marsupial Insectivores appeared also, it was still in harmony; for the Marsupials were mainly Insectivores: moreover the former were prophetic of the higher development of the Mammalian class. Now, if, along with the semi-oviparous Mammals and swimming, crawling, and flying Reptiles, there were Reptilian Birds, waders and others, the harmony would be only the more complete. The presence of the same number of phalanges in birds and reptiles would be not at all improbable—certainly no basis for an argument against the birds."

In another business letter of Feb. 14th, I find the following: "The strongest argument for the ornithic character of the feathered fossils are, (1) *that the animal had feathers:* for the idea that they were not true feathers is a mere supposition without any facts to sustain it: (2.) That the *expanse of the wing was made by feathers on a short arm*, and not as in the Pterodactyl by an expanse of the skin supported by an elongated finger. The structure of the foot in the Pterodactyl also shows that the animal had no close relation to the Birds. The world will have finally to settle down to the belief that there were Reptilian Birds in ancient times, as well as Ichthyoid Reptiles and Oöticoid Mammals. This is my strong persuasion."

Amherst, Mass., May, 1863.

Correction for part of edition.—On the preceding page, in the last paragraph, after (2), the word Amphibians should be inserted.

ART. VIII.—*On Hydrastine;* by F. MAHLA, Ph.D., Chicago, Ill.

HYDRASTINE was detected by Durand, in Philadelphia, as early as 1851, who noticed its alkaline nature, but did not succeed in preparing it in a pure state.

It is contained in *Hydrastis Canadensis*, in which it is associated with berberine. Mr. J. D. Perrins, of Worcester in England, first separated it from this plant in pure form, and described some of its properties, but did not institute an elementary analysis. At thet ime of Mr. Perrins' investigations, I had, prompted by the remarks of Prof. Procter of Philadelphia, also prepared pure hydrastine, and intended to analyze it; seeing, however, that Mr. Perrins had promised to study its composition, I did not continue my investigations. After the lapse of more than a year, I do not any longer hesitate to finish my researches, and take the liberty to publish herewith the results.

Hydrastine may be obtained by adding aqua-ammonia in slight excess to the liquid from which the berberine has been previously separated by an addition of chlorhydric acid. The precipitate, obtained under these circumstances, is collected on a calico filter, freed by expression from water, and mixed with strong alcohol, in which it easily dissolves by application of heat. On cooling, the hydrastine crystallizes readily, and may be purified from adhering coloring matter by redissolving and recrystallizing it several times with alcohol.

Hydrastine crystallizes in forms which belong to the right prismatic system. They are combinations of the longitudinal with the vertical prism, in which the planes of the first mentioned form are prevailing. It is perfectly white, and its crystals exhibit great brilliancy. Hydrastine in the pure state is tasteless; its salts, however, have a bitter, heating, acrid taste. It melts like a resin, when heated to 135° C.; it decomposes at a higher temperature with emission of yellowish vapors, the odor of which resembles that of carbolic acid. When heated on platinum foil, it readily takes fire and burns with a smoky flame.

Hydrastine is insoluble in water; it dissolves, however, in alcohol and in ether. It is not affected by a dilute solution of caustic potassa, even if boiled with it for a prolonged period. Concentrated nitric acid does not at first act on it, but dissolves it afterwards with a red color.

Hydrastine dissolves in cold concentrated sulphuric acid, and imparts to it a yellowish tint; this mixture when slightly warmed, assumes a red color; bichromate of potassa produces with it a dark brown coloration, which is distinct, however, from the strychnia reaction, in as far as it does not show any blue or violet shades.

It dissolves readily in diluted hydrochloric acid; ammonia and caustic potassa produce in this solution white precipitates, which are insoluble in an excess of the reagent; ferrocyanid and iodid of potassium generate also white deposits; iodine dissolved in a solution of iodid of potassium produces a cinnamon brown precipitate, which, when heated with the liquid in which it is suspended, contracts readily to a resinous mass.

Bichlorid of platinum precipitates the solution of muriate of hydrastine with a yellowish red, chromate of potassa with a yellow, color. This latter precipitate dissolves when heated with the liquid, in which it is suspended, but separates again on cooling; before dissolving, it assumes, in the liquid, the appearance of a melted resin.

Terchlorid of gold produces in the solution of muriate of hydrastine a reddish-yellow precipitate, which also contracts in the liquid on application of heat, and looks like melted resin; it however finally dissolves. Sesquichlorid of iron produces no change.

In order to subject the hydrastine to an elementary analysis, it was desiccated at a temperature of 100° C. until it ceased to diminish in weight.

The analysis itself yielded the following results:

I. 0·5013 hydrastine, burned with oxyd of copper, gave
1·2260 carbonic acid, and
0·2712 water.

II. 0·5085 hydrastine, burned with oxyd of copper, gave
1·2377 carbonic acid, and
0·2608 water.

III. 0·4469 hydrastine, burned with soda-lime for nitrogen-determination, gave
0·2727 ammonio-chlorid of platinum.

IV. 0·5904 hydrastine, burned with soda-lime, gave
0·3542 ammonio-chlorid of platinum.

These results lead to the following percentage composition:

	I.	II.	III.	IV.
C =	66·696	66·379		
H =	6·010	5·698		
N =			3·832	3·767

In order to determine the formula of the alkaloid, I selected the platinum double salt, prepared by precipitating the hydrochlorate of hydrastine with chlorid of platinum, and the hydrochlorate itself.

The chloroplatinate of hydrastine is an amorphous reddish-yellow powder, which is slightly soluble in water, better, however, in alcohol. When its alcoholic solution is boiled for some time, the platinum separates in the form of a black powder. The liquid, in which this deposit forms, has a remarkable blue fluorescence.

Chloroplatinate of hydrastine fuses, when heated to little above 100° C.; it decomposes readily when heated higher.

1·0079 chloroplatinate of hydrastine, desiccated at 100° C., gave on careful ignition
0·1630 platinum.

This corresponds to 16·17 p. c. platinum.

The chlorhydrate of hydrastine is obtained by dissolving pure hydrastine in diluted chlorhydric acid. The solution obtained is evaporated over the water-bath to dryness. It then forms a gum-like white substance, which can be readily powdered. It is easily soluble in water and alcohol. Its aqueous solution has a strong blue fluorescence. It is uncrystallizable.

In order to determine the proportion of chlorhydric acid, which is in combination with the hydrastine, I dried the powdered salt for a long time at a temperature of 100° C., and then precipitated its aqueous solution with nitrate of silver:

0·7258 hydrochlorate of hydrastine gave
0·2419 chlorid of silver.

This amount corresponds to 8·46 p. c. of chlorhydric acid.

From these results, I calculate $C_{44}H_{24}NO_{12}$ as the formula and equivalent for hydrastine. Indeed, if we figure the percentage composition of a body with this formula, and compare it with the obtained data, we find that they agree well:

Hydrastine = $C_{44}H_{24}NO_{12}$ in 100 parts:

		Found.		Calculated.
Carbon,	=	66·696	66·379	66·333
Hydrogen,	=	6·010	5·700	6·030
Nitrogen,	=	3·832	3·767	3·517
Oxygen,	=	23·462	24·154	24·118

The chloroplatinate of hydrastine, $C_{44}H_{24}NO_{12}$,HCl+$PtCl_2$, requires in 100 parts 16·32 p. c. platinum; I found 16·17 p. c.

The chlorhydrate of hydrastine = $C_{44}H_{24}NO_{12}$, HCl, contains, according to its formula, 8·34 p. c. chlorhydric acid, while I found 8·46 p. c.

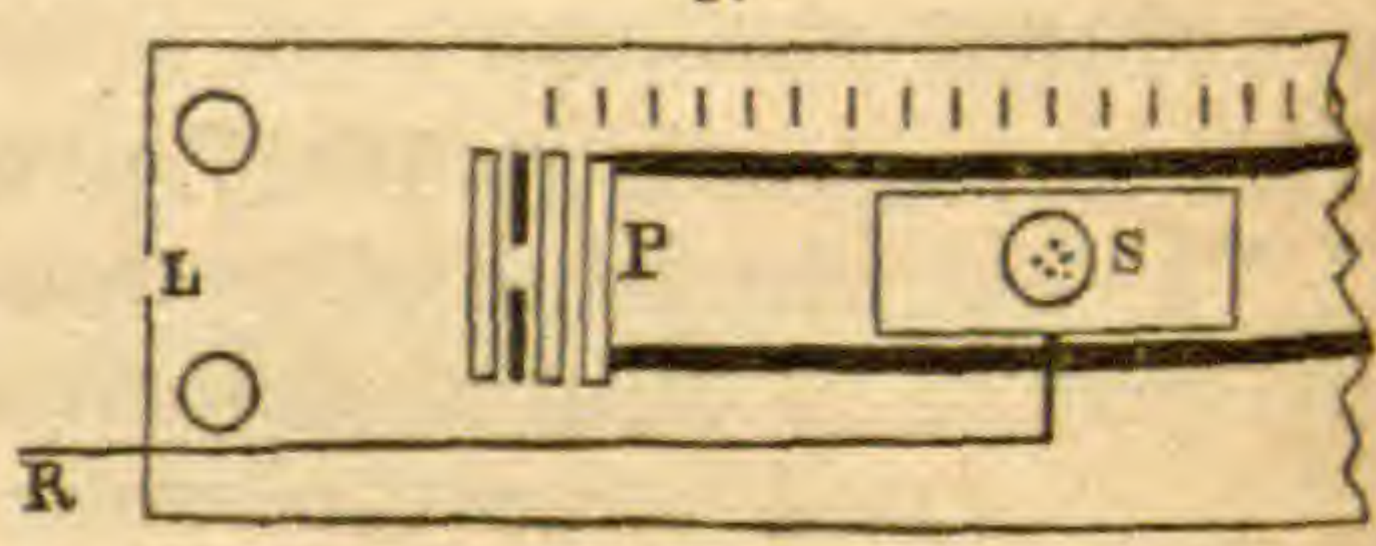
Chicago, Ill., March 31, 1863.

Art. IX.—*Description of a Photometer;* by Prof. O. N. Rood.

Dove, a short time ago, proposed a photometer, consisting essentially of a compound microscope provided with a microcopic photograph, the latter being used as a "screen," and illuminated from both sides, in such a way that when the compensation had been reached, the photograph was rendered invisible.[1] In the construction of the instrument at present described, I have availed myself of this general mode of comparison, discarding however the microscope and photograph, and supplying their place in a more simple, and, for many purposes, in an at least equally efficient manner.

The instrument consists of a board from 50 to 100 inches in length and 12 inches wide; a slide at S supports a small lamp or standard candle; the slide can be moved towards or away from the screen P by a light rod R; the screen, reckoning from L, is composed of four parts, disposed thus: 1st, a glass plate is coated with a collodion sensitized with iodid of potassium and of the variety called adherent; this is immersed in a solution of nitrate of silver, just as though it was to be used for the reception of the camera image; it is then exposed to broad daylight for two minutes, and developed by sulphate of iron, washed and dried. By this operation a dense and absolutely opaque deposit of silver is produced in the substance of the collodion. The collodion film is then removed from one spot ½ an inch square; this can be done neatly and completely by the use of a needle, great care being taken to leave the edges of the little square sharp and clean. The observations are made by means of this spot, and it of course is to be located in the axis of the instrument. On account of the uniformity of the appearance of this plate by

1.



[1] *Pogg. Annalen*, Band cxiv, page 145.

reflected light, and the sudden, sharp transition from the thin opaque edges of the film to the clear glass, the screen is, I suppose, better adapted for use than any which has yet been proposed. The collodion side of the glass plate is turned towards L.

2d. Next in order comes a piece of blackened card board of the same size with plate No. 1: this is provided with an aperture corresponding with, and somewhat larger than, the exposed portion of the collodion plate.

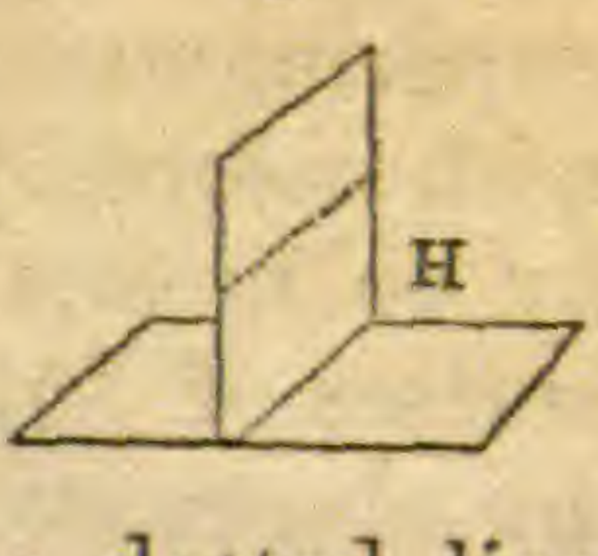
3d. Then follow two thin finely ground glass plates of the same size with No. 1, the ground surfaces facing towards L. A single plate of ground glass can be used, but with two plates the illumination of the square spot is much more uniform and even; in other words, by the use of two plates all idea of the ground glass *texture* is removed, and nothing is seen but a square patch of light.

Two small lamps or standard candles are placed about as indicated at L. The construction of the lamps is the same with those used by Potter, (*Physical Optics*, page 112,) in his photometric experiments, consisting, viz: of shallow cups filled with oil, which support little metallic bridges, formed of thin plates of metal perforated with four holes, through two of which small wicks are drawn, the others supplying air. When properly arranged these lamps will give a pretty uniform light for thirty minutes together; but though by no means furnishing a truly constant illumination, yet the variation is almost always gradual and steady, a point of great importance, for this being the case, it is easy to make the observations in such a manner as nearly to exclude errors from this source, as will presently be shown.

The movable lamp is provided with a vernier, and a scale divided into inches and tenths extends from that ground glass surface next to the collodion plate along the entire length of the board. At L is a shade to protect the eyes while observing; this has an aperture one inch square placed in the axis of the instrument. The photometer, when arranged as above described, is peculiarly adapted to measure the amount of light transmitted by plates of colorless or colored substances, as well as the amount of light reflected from polished or unpolished plane surfaces at various angles of incidence. For these uses it was expressly contrived, but at the same time it is plain that by modification it can be adapted to other purposes.

Mode of using the instrument.—The three lamps, after being lighted, are allowed to burn for twenty or thirty minutes, till their light has become steady, then the centre of the flame of the slide lamp is made to coincide with the vernier by using the arrangement seen in fig. 2; this is constructed of wood and all its angles are right angles. The vernier being placed at H, the eye is brought opposite the dotted line

2.



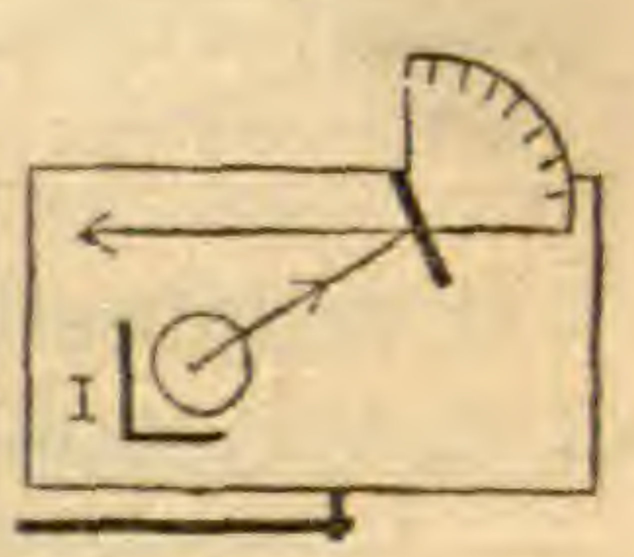
in such a way that the surface of the upright is reduced apparently to a line, when the lamp is moved on the slide till this line bisects the flame. The slide is then placed about as distant from the screen as may be desired and, compensation approximately effected, by moving the pair of lamps at L away from or towards the collodion surface. If these are brought too near the screen, the square spot will appear dark on a light ground; if too far off, light on a dark ground. The final compensation is made by moving the slide lamp S; this takes place when the spot becomes invisible. In the first experiments, instead of a *pair*, a single lamp was used, and owing to this the spot never became entirely invisible; one half was always faintly lighter than the adjacent ground, the other half darker; this was owing to the slight difference in the distances of the sides of the square from the lamp, and may be adduced as a proof of the delicacy of the photometer. The observer then reads off on the scale the distance of the slide lamp from the ground glass surface next to the collodion plate, the white page of the note book reflecting sufficient light for the purpose. Supposing it is desired, for example, to measure the amount of light transmitted by a plate of polished glass at a perpendicular incidence, the plate is placed at P, when it will be found necessary to bring the slide lamp nearer in order to effect compensation. The amount of light transmitted is then at once calculated by the law that "the intensities which measure the values of the illuminating powers are *directly* as the squares of the distances from the screen."

My mode of work, in all cases, has been to alternate the observations, reading off first the distance of the free flame; 2d, that found with the plate of glass under experiment; 3d, free flame; 4th, plate of glass, &c.; and as the variations in the brightness of the lamps are generally slow and steady, by this mode, the error from this source is very greatly reduced.

Dove considers this form of photometer the only one which is capable of comparing the intensity of two masses of light having different colors; the degree of accuracy attainable in this direction can be judged of by the experiments detailed below. As the square spot will in such cases be differently colored from the ground, it always remains plainly visible, but there is a moment when the edges of this spot, to a great extent, disappear, and it seems to blend softly into the white ground. This I assume to take place when the illuminations are exactly balanced, and have accordingly employed it as the test in my experiments on colored media. Though the determinations with colored light are on the whole pretty satisfactory, still they by no means reach the accuracy easily attained when the two masses of light are similar in tint. With regard to the following observations, it may be remarked, that those made with colored light show fully the degree of accuracy that can be attained in this direction with the photometer, the experiments, as far as they extend, being made with much care, though in such a way that each reading is entirely independent of the preceding, and has exercised no influence on that which follows.

To measure the amount of light reflected from a polished surface, I have used a plan not unlike that employed by Potter;[2] the slide, in the place of the lamp, carries the polished plate in an upright position and supported in such a way that it can be rotated on its axis and adjusted properly. Its axis corresponds with the centre of a divided quadrant by which the incidence of the ray can be measured. I is a shade to prevent the light from reaching the screen. The vernier is made to correspond with the perpendicular axis of the mirror, and of course the distance of the lamp's flame from the mirror's axis is always to be added to the reading obtained. By removing the shade I, and placing it so as to protect the mirror from light, the direct light of the lamp falls on the screen. As the lamp stands then a little obliquely, the slight error thus introduced must be allowed for, or, what is better, the lamp and mirror may be placed on either side of, and at equal distances from, the axis of the photometer.

3.



Prof. Silliman, Jr., pointed out to me some weeks ago, that small errors might be introduced by reflection from the walls of the room. This is guarded against by the use of several blackened shades properly disposed about the instrument, or by experimenting in a room with blackened walls, as is usual in determining the photometric power of illuminating gas.

Determination of the amount of light transmitted by a plate of colorless, polished crown glass $\frac{1}{12}$th of an inch thick:

Flame free.	With plate.
20·7 inches.	19·95 inches.
20·85 "	19·9 "
20·8 "	19·9 "

Amount of light transmitted, 91·09 per cent.

Determination of the amount of light transmitted by a plate of colorless glass, finely ground on one side, $\frac{1}{12}$th inch thick:

Flame free.	With ground glass.
20·2 inches.	13·1 inches.
20·35 "	12·8 "
	13·1 "
	13·05 "

Amount of light transmitted, 41·13 per cent.

[2] Physical Optics, p. 112.

Determination of the amount of light transmitted by one plate of polished orange colored glass, $\frac{1}{10}$th inch thick:

No. 1.		No. 2.		No. 3.	
Flame free.	Orange glass.	Flame free.	Orange glass.	Flame free.	Orange glass.
23·05	15·50	22·10	15·50	22·2	15·40
22·60	15·80	22·20	15·75	22·2	15·65
22·60	15·40	22·40	15·40	21·15	15·10
22·64	15·30	22·60	15·60	21·8	15·00
22·15	15·65	21·90	15·50	21·2	14·3
22·35	15·50				
Amount of light transmitted,	47·45	48·74		48·42 per cent.	

Determination of the amount of light transmitted by a plate of deeply colored red glass, $\frac{1}{10}$th of an inch thick:

Flame free.	Red glass.	Flame free.	Red glass.	Flame free.	Red glass.
21·5	7·3	21·10	6·7	20·9	6·5
21·	6·3	21·15	6·6	20·2	6·7
22·	7·2	21·40	6·9		7·0
22·	6·7		6·8		6·2
	6·7		6·6		
Light transmitted, 10·03		10·05		10·36 per cent.	

Peace Dale, R. I., May 7th, 1863.

Art. X.—*On Aerolitics, and the fall of Stones at Butsura, India, May*, 1861; by N. S. Maskelyne.[1]

The branch of science that treats of Meteorites has acquired sufficient importance to justify our giving it a special name, and I therefore propose for it the denomination with which this article is headed. Many reasons conspire to render this study of "aerolitics" one of increasing interest, and to make it highly desirable that collections of meteorites should exist to illustrate it, as complete as possible, not only in the numbers of the different falls they represent, but also as regards the modes in which the specimens are prepared for exhibition. These remarkable bodies will always command a general interest, from the fact that in them we see matter foreign in its origin and history to our own world, and handle, in them, the only tangible substances that belong to the space beyond our atmosphere. But the special interest attaching to a collection of them arises from the fact that, while they exhibit features of marked similarity, they withal, both

[1] Extracted from an article entitled *Mineralogical Notes*, by Professor Maskelyne and Doctor Victor von Lang, (of the British Museum) in the L. E. & D. Phil. Mag., No. 165.

as regards their constituent minerals, and the manner in which those minerals are mixed with each other, possess almost every one of them a very distinct individuality. Moreover, every day that the collection of specimens representing the older meteoric falls is deferred, adds to the difficulty of forming a complete series of them. It was on these accounts that the small but valuable collection that three years ago existed in the British Museum, has since that period been very largely increased. Towards the furthering of this object, most valuable assistance has been rendered by Governors of Colonies and Indian Presidencies, who have exerted their authority with a liberality that has been in one case, indeed, rivalled by the patriotism of a valuable and learned body, the Asiatic Society of Calcutta. The result of this, and of the considerable acquisitions made by purchase, has been that the aerolitic collection, which is an appanage to the Mineral Department in the British Museum, has now risen in point of material into the foremost place among such collections in the world.[2]

To accumulate so great a material is, however, but one step towards the end which should be held in view in the formation of a scientific collection. The next step consists in making that material available for the use of science, partly by a proper preparation and exhibition of the specimens, partly by a complete description of them. I propose in this and subsequent papers to contribute something towards the last of these objects.

Yet, when one approaches the subject with a view to undertake investigations in it, one cannot help feeling some disappointment, as well at the incompleteness of the chemical results that have been hitherto obtained, as at the unsatisfactory position of our knowledge concerning the origin and the sources of meteorites. Aerolitical science has to deal with the circumstances that attend the fall of a meteorite, no less than with its mechanical condition and its chemical composition; and from the data thus acquired it has to arrive at conclusions regarding the origin, the motion, and the cosmical relations of the foreign matter that thus wanders as it were into the atmosphere of our earth.

The general literature of the subject is becoming very considerable. Besides the tables and reasearches published by Mr.

[2] Every great collection has its own characteristic merits. If I may speak of that in the British Museum as the richest in material, taking the mass of the specimens as well as their numbers into consideration, it is with cordial pleasure that I express the highest admiration and respect for what I will not call a rival collection at Vienna. That collection is a classical one. Its specimens have been gradually collected, well described, and admirably exhibited. That aerolitics exists at all as a scientific subject is probably due to the existence of, and the care bestowed on, that collection. In the cause of science it is to be hoped that persons in authority in Vienna may not feel any jealousy of the rising collection in London, but may be ready to exchange, to the mutual advanatge of both collections, duplicate specimens of aerolites not common to the two.

Greg in our own country, and besides many papers of Baron Reichenbach in Poggendorff's *Annalen*, Hofrath Haidinger has, by his active pen and energetic mind, contributed, in Austria, perhaps more valuable notices on the fall of meteorites than all other living authors; and Dr. J. Lawrence Smith, as well as Prof. Silliman, by their accurate collection of facts and by their own investigations as chemists, have done much for the subject in America, where also the vigilant activity of Prof. Shepard has been conspicuous in collecting and distributing the specimens themselves.

The more special and exact literature, that, namely, which details the work done on meteorites in the laboratory, carries the names of the best inorganic analysts of this century, including those of Rose, Wöhler, and Rammelsberg. But if the progress thus far made in either the general or the special parts of the subject is not very large, it is at any rate enough to convince us that we see with tolerable clearness the questions to which we have to seek answers, and what are the cardinal points of interest raised by the presence of a meteorite on our globe, and by the circumstances attending its advent to it.

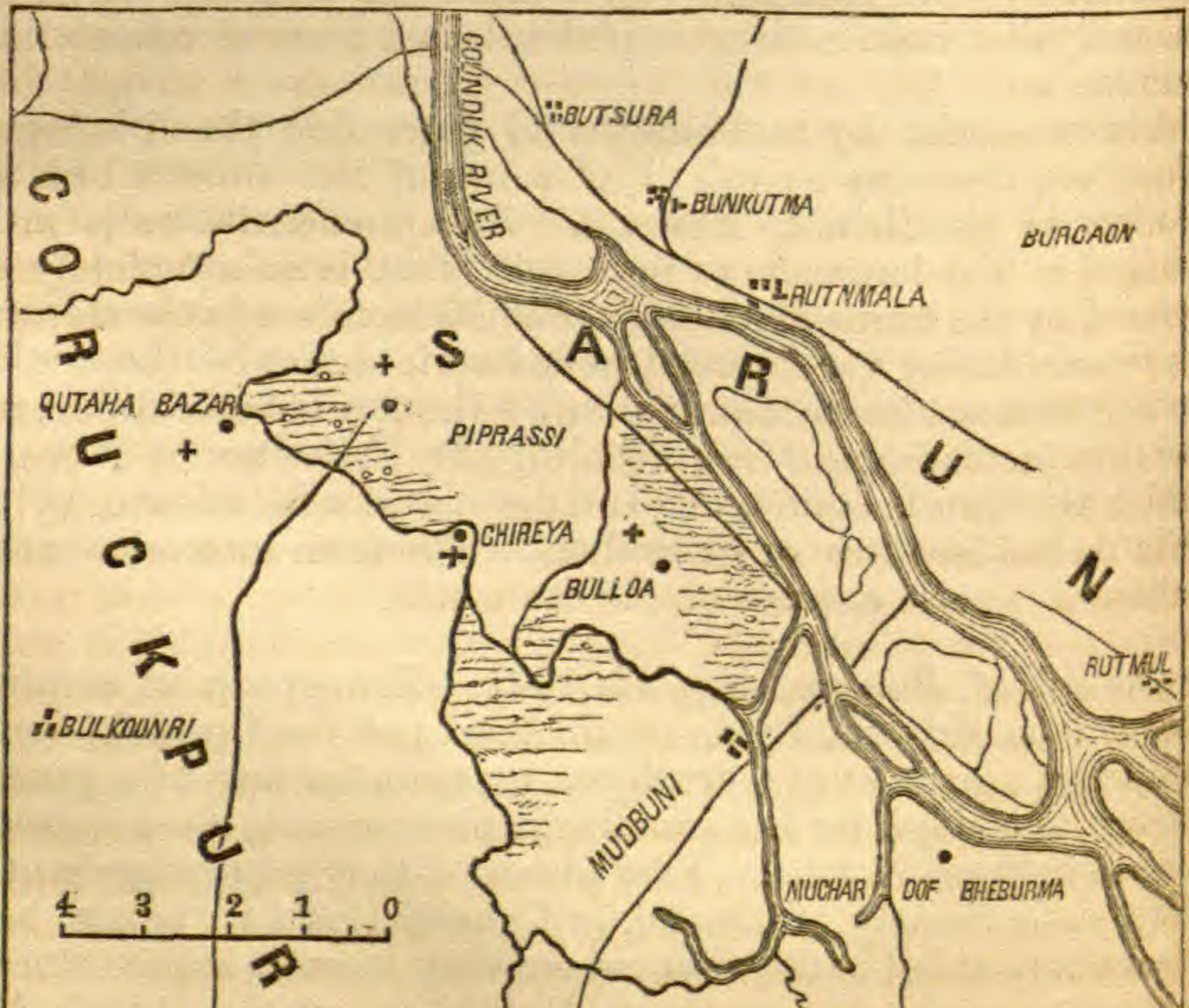
The chemical methods adopted for the analysis of a meteorite are probably unsatisfactory to every chemist who has employed them. The separation of the olivinoid and soluble felspathic from the insoluble felspathic, augitic, and other constituents, by the action of an acid, is necessarily incomplete; and the assignment of even empirical formulæ for the augitoid and felspathic ingredients is no less unsatisfactory. Yet in many meteorites it seems very difficult to conceive any better direct mode of operation. The intimate manner in which the different minerals are sometimes mingled, in what I may call a microscopic breccia, building the structure of the minute spherules in some of those belonging to the large group to which G. Rose has given the happy name of chondritic meteorites, and the excessive subdivision of the nickel-iron which in infinitesimal spangles is disseminated alike through homogeneous spherules and through those which present this brecciated character,—these facts, which the microscope alone reveals, seem to bar the chemist from any complete mechanical separation of the ingredients of many meteorites, whether by the agency of a magnet, or by that of the selection by the eye and hand, of distinct homogeneous particles.[3] Still there are cases in which analysis is possible; in some mete-

[3] Probably it would be found practicable to determine the iron indirectly by the estimation of the hydrogen developed by the treatment of the aerolite with acids, under conditions convenient for collecting that gas. The sulphuretted hydrogen might be estimated at the same time; and even if it were all calculated as emanating from meteoric pyrites, the ultimate error in the analysis would be less than by a method in which the entire separation of the metallic iron is generally impossible, and the estimation of ferrous oxyd therefore as often too high.

orites, as in that of Parnallee, the minerals are tolerably isolated from each other; and the fact, that the chemist, in dealing with such meteorites as those of Chantonnay, Stannern, Luotolax, Bokkevelde, and Bishopville, is enabled to place each of them as the characteristic member of a group, may furnish ground for the hope that approximate methods may be found for at least determining the nature of the minerals contained in any given meteorite. One such method appears to be furnished by the microscope. A thin transparent section of an aerolite exhibits, under a low power, in a tolerably characteristic way the minerals of which the aerolite is composed. By comparing these minerals as thus seen, and as observed *en masse* in the specimen, with the minerals that predominate in certain well-investigated and, so to say, standard types of meteorite, one soon learns to discriminate between them, and to predicate of any given individual aerolite, with what others it presents mineralogical analogies, though the assignment to each of these minerals of its precise place as a mineral species is in some cases very difficult. Occasionally, however, as in the coarser-grained varieties, one is enabled to discriminate and to separate by mechanical selection for chemical analysis certain mineral ingredients in a state of considerable purity.

I have sought by these means to determine the lithological character, if I may so call it, of some of the undescribed meteorites in the British Museum. As a nomenclature is much wanted in our language to represent what is so completely expressed by the terms Meteorstein and Meteoreisen in the German, I propose calling the former (the meteoric stone) by the original term Aerolite, the meteoric iron by the term Aerosiderite, and the intermediate varieties (including the Pallasites of Rose), in which the iron is continuous and associated with silicate, by the term Aerosiderolites or Siderolites. The term meteorite would remain a generic expression for the whole.

The fall of Butsura, May 12, 1861.—The group of aerolites that fell on May 12, 1861, on the banks of the Gunduk, forty-two miles northeast of Goruckpur, presents features of a general interest that claim for it a prominent place among those to be described in these Notices. Five pieces of that group were sent to the Asiatic Society of Bengal, at Calcutta; and they have been thence forwarded to London, where they were exhibited during the period of the International Exhibition, at the British Museum. They have since been cut, in directions agreed upon by Mr. Oldham on the part of the Asiatic Society, and in accordance with a liberal and patriotic resolution of the Society to share with the National Museum in London their valuable acquisitions in Indian aerolites. These five stones fell at four distinct places, southwest of the main stream of the Gunduk, near the village of Mudbuni and on the opposite side of the river to Butsura, which, as being the nearest place indicated on the Royal Atlas of Johnston, is perhaps the best to give its name to the fall (see Map). The four spots where the aerolite fell are marked with a cross on the map, and form the angles of an irregular four-sided figure, one side of which runs nearly northwest and southeast, taking a direction parallel to the general course of this part of the Gunduk. The northern angle is very near to, and rather to the north of, a little place called Piprassi; the southeastern angle is a little to the northwest of one called Bulloah. These points are some three miles apart. Two very small fragments, weighing about 5 and 7 ounces respectively, fell at the latter locality. A thin slab-like piece fell at the former. It weighed about 11 pounds. Of the other angles, one is formed by a spot called the Qutahar Bazar (described in one account as in the Thannah of Nimboah); this is the northwesterly angle. The southern angle is at a spot called Chireya. The stones that fell at these two points respectively weighed 13 lbs. and $8\frac{3}{4}$ lbs.



These points, like the former, are some three miles apart; and whereas Chireya and Piprassi are only two miles, the northern point, Bulloah and Qutahar Bazar are some three miles distant from each other.

For the narrative of the circumstances accompanying the fall of these aerolites, I am indebted partly to Mr. Atkinson, the Secretary of the Asiatic Society, partly to my friend Dr. Oldham, the Director of the Indian Geological Survey.

The fall of the Qutahar Bazar and Chireya specimens was heralded by a report from out of a cloudless sky with a sound like that of ordnance, succeeded by several successive peals of seeming thunder. An appearance as of smoke was seen above the ground where they fell. One stone penetrated the soil for a cubit (=18 inches); the other did so to half that depth.

The two small fragments from Bulloah were accompanied by phenomena well substantiated by a near eye-witness. A native was taking his cattle to the water, when he was startled by three very loud reports, and saw in the air on high "a light" (a luminous body), which fell to the ground within 200 yards of him. Here too the sky was serene, and the weather fiercely hot, but there was a very small cloud, out of which this witness stated the report and the luminous body to have come. "First," he adds, "there was the loud report, and about the same time I saw the light like a flame; then the stone fell, and in falling made a great noise, and after it fell the sand was taken up high into the air." He went to the spot whence the sand had been raised from the ground, and found there five pieces of stone. They were very hot, and so was the sand all round, which was thrown up to the height of a foot. Unfortunately only two of these five fragments were preserved. Dr. Oldham further mentions that the incandescent fragments in falling are stated to have scintillated like iron when at a white heat.

The Piprassi stone was seen to fall by a witness quite independent of the other, but unfortunately from a much greater distance. In the midst of the calm hot day, while sitting in a field on the east side of the village of Piprassi, with many of the villagers, he states that they were startled by three loud reports succeeded by a rumbling sound which gradually died away. Their attention was immediately arrested by a cloud of smoke, which rose from the ground at about 1000 yards from them. They saw nothing like a falling body, but they heard a whistling sound as of a bullet, but much louder. They went to the spot and found the stone, round which the gravel had been thrown up for some 2 feet. Fortunately the stone was not carried away, for nobody touched it for two days. It was Mahadeo!

Two hours after the fall, the serenity of the weather was interrupted by a storm accompanied by a little rain.

The reports of the explosion were heard at a distance of sixty miles from the locality.

Dr. Oldham, on sending these most interesting aerolites to England, accompanied them by remarkable observations of his own. The two little Bulloah fragments fit exactly together, and both fit on to the Piprassi stone. The Chireya stone in like manner fits with sufficient precision to that which fell at Qutahar. He surmised also that a careful adjustment would succeed in uniting all five fragments into a whole; and he indicated as a guide to this adjustment, a remarkable vein of iron which ran through the Piprassi and the Qutahar stones. I have since tried every possible means of effecting this; and though it is not practicable to find continuous surfaces of contact on the Piprassi and Qutahar stones, I have been enabled to determine the precise position they must have occupied relatively to each other, and have modelled and constructed an intermediate piece which, allowing contact of the stones at one part, builds the whole of the fragments into one large shell-like piece, obviously itself a fragment of some far larger mass. But this presents also another point of great interest. The Bulloah and the Piprassi stones, at the contact surfaces by which they fit together, exhibit no crust, though in other respects coated with it. The Chireya and Qutahar fragments, on the other hand, present a crust hardly, if at all, distinguishable from that covering the rest of their mass, on the very parts that form the faces of junction, and at which they fit with unquestionable precision. These surfaces indeed are smooth, and the edges very much rounded off, while those of the Bulloah and Piprassi stones fit together with the exactitude of adjustment with which the portions of a broken piece of oolite might be reunited.

Before attempting to draw conclusions from these facts, I will describe the general characters of the several fragments, in order that all the data offered by this aerolitic fall may be given in consecutive order.

The two that have been preserved out of the five stones that fell at Bulloah are small fragments, fitting on, as before mentioned, to one of the long edges of the Piprassi stone. Probably the whole five formed a long bar-like piece fitting on to that edge, and these two would, in that case, constitute the half of it.

The Bulloah stones are rounded along their summits and sides, and are there coated with a crust of a sooty black, and of dense texture. On the surface of contact they and the Piprassi are not crusted. The material of which the interior of the Bulloah stones is composed proves, when examined by a lens, to contain a profusion of protruding points of metallic iron. It presents a yellowish-brown ground-mass. It is mottled with irregular dark stains, which surround the metallic iron. This iron, associated with a considerable amount of meteoric pyrites, is present in this aerolite to a very high percentage. It is very evenly distributed in small, isolated, irregularly formed and sometimes crystalline-looking particles, not aggregated into a sponge, as in the siderolites, but, as in the beautiful aerolite of Akbarpúr, the grains of metal seem linked by a ferruginous or iron-stained mineral, which may possibly indicate the vestiges of a sponge-like structure of the iron at some earlier period in its history, when perhaps the silicates were less basic than at present, and less of the iron oxydized.

Besides these ingredients, there are several very irregularly-distributed spherules of a mineral of the greenish-brown color and translucency, as well as the lustre, of dirty bees-wax. It is somewhat transparent in thin sections, and presents the characters of olivine.

A minute amount of iron pyrites occurs besides the meteoric pyrites; and a little of a very dark-colored mineral is also present, generally with a lustrous fracture, and perhaps occasionally somewhat crystalline.

In a section under the microscope with a power of one-inch focus, this aerolite does not prove to be a very remarkable one.

The mass of it seems to consist of olivine. This is associated with a gray mineral, and also with one that is of an opake white. This gray mineral in some cases seems to constitute entire nodules of the aerolite, and sometimes seems mingled in the sort of brecciated mass, containing olivine crystals, that forms other nodules in it. It presents the appearance, in the former case, either of a dark mottled surface spangled with dark points (consisting sometimes of iron, and in some cases curiously distributed, as if spurted through the mass from a centre), or of a mineral presenting very regular and minute parallel cleavage-planes with dark gray bars running along them, often rayed out like a fan, and with cross-cleavages usually oblique, but at angles which vary with the inclination of the section to the axis of the crystal.

There is also another mineral, transparent and presenting cleavages nearly perpendicular to each other, which appears to be distinct from the foregoing.

What these minerals thus associated in small proportion with the olivine may be—whether they are solely augitic, or whether also the long feldspathic-looking bars are really fragments of some feldspar—is at present difficult to say with certainty. But in a subsequent article in these Notes I purpose giving all the data I possess for assigning to these and other meteoric minerals their true mineralogical character.

The Butsura fall, therefore, seems, like other aerolites rich in iron, to approach in character to a siderolite in that the silicates it contains consist, as I believe, for the most part of olivine. This olivine is generally very transparent, and comparatively colorless; but near the iron particles, and forming a continuous fringe to them, its granules become of a ferruginous color, and are at times, especially in parts of the Qutahar and Chireya stones, red, like fragments of garnet or zircon.

The meteoric pyrites is present in a ratio of about one-half the apparent quantity to that of the iron. It is generally in little independent particles of the same average size as those of the iron; and it sometimes is continuous with the iron in the same particle, like the copper and silver of Lake Superior.

The Bulloah stone exhibits less of the ferruginous olivine than the others around the iron, and may perhaps contain more of the barred and grey mineral or minerals. The result is a paler hue on it. Its crust, on the other hand, is thicker and coal-black, that on the other stones having a browner cast.

But the specific gravity of the aerolite seems pretty constant in its different parts, namely about 3·60.

The next stone in order to the fragments that fell at Bulloah is the thin slab-like piece that fell at Piprassi. One of the faces of this piece is convex, while the other side presents a somewhat hollowed form. It is nearly rectangular in its general outline. The inner, as well as the outer side, presents some large but shallow hollows or "pittings." This piece, as before observed, does not fit on directly to the great mass that fell at Qutahar Bazar; but, that it formed a closely contiguous part to it on the original aerolite, there can be no doubt. In fact, the general contour of the stone, the correspondence of the outline and character of the shallow hollows on both, and, finally, the existence in them of the remarkable vein of nickel-iron before alluded to, and which runs persistently in one plane through each of them from the top to the bottom,—these all serve as guides to the restoration of the original form of the aerolite, so far as these two parts of it are concerned, and are the grounds of justification for the restoration of this part of the meteorite which I have attempted, by moulding the small intermediate piece, to unite the two stones. The Qutahar stone, which becomes thus adapted to the Piprassi piece, is a fine mass of an irregular wedge shape. The inner side is fitted with large shallow depressions, and presents a rather concave surface. The outer side is flat and smooth. The base on which it stands, and which is the result of the wedge-like form, is also smooth, rounded at the edges, and presenting hollows and irregularities on one half of its surface, while to the side of this base, on the inner or just below the concave part of the stone, the irregular piece that fell at Chireya adapts itself. This last fragment is somewhat pitted and deeply grooved on its upper side, and rounded everywhere else. Indeed, notwithstanding the precision with which it fits to the Qutahar stone, the faces and edges at the parts of contact are rounded off so as almost to obliterate the original form of the stones at this part. The contour presented by the reconstructed mass, so far as the reuniting of these scattered fragments enables one to build it up, is that of a shell or the thick outer rind of one side of a considerable aerolite.

The lithological character of the Piprassi, Qutahar, and Chireya stones is very similar to that of the Bulloah pieces. But there are differences between them worthy of being noticed. Thus the crust on them is not very different from the dense black crust that coats those of Bulloah; it is, however, less characteristic and less thick. They are all dull, as the crusts on highly olivinous meteorites generally are, as contrasted with the shining enamels on the feldspathic-augitic kinds. It exhibits crystalline metallic-looking points, as well of iron as of meteoric pyrites and, at very rare intervals, of iron pyrites, that are disseminated among small globular projections of a pitch-black color. It is these black projections, on the other hand, that constitute the whole mass of the Bulloah crust. But in the three larger masses the crust assumes a dirty blackish-brown hue.

The facts above recorded appear to me to throw some light upon several interesting questions.

We may hazard a pretty safe conjecture as to the direction of the Butsura fall, by observing that the lighter stones fell to the S.E. of the heavier ones; the Bulloah three miles S.E. of Piprassi; the Chireya a similar distance E.S.E. of Qutahar. If we suppose that the disruption of all the stones was simultaneous, we might further assume that they fell with a diverging flight; for the Qutahar Bazar and Piprassi points are considerably further asunder than those of the Bulloah and Chireya falls. In fact, a line passing from the E.S.E. to W.N.W. would represent the direction of the flight of the aerolite; and if we are to judge by the different divergences of the stones, that flight would not have been at a great inclination to the horizon.

Had it been quite horizontal, the point of the divergence would have been, on this view, about seven miles E.S.E. of the central point of the fall, and two miles N.W. of the Mudbuni. As, however, it would seem to have fallen from a considerable elevation, it may have been much further off, though the point of disruption would have been somewhat nearly vertical over the position thus indicated.

But this fall is remarkable for the evidence it affords of the incrustation of an aerolite subsequently to its disruption, as well as of the probability of successive disruptions, of which one, at least, was not followed by incrustation. In the great Parnallee aerolite, and still more in that which fell at Bustee, we have cases, of which indeed every collection must exhibit some more or less evident examples, showing crusts on different parts of an aerolite that seem not to have been contemporaneous —where, in fact, the crust on one part has not the thickness and homogeneity that characterizes that on another part. The following, in the British Museum collection, are cases in point: Stannern, Bokkeveldt, Benares, l'Aigle, and Mezö-Madaras. These facts are among those we have to explain. On the present occasion they were accompanied, according to every witness, by reports in the air, and by a subsequent roll of thunder. In two cases the distinct reports were three in number. There was a cloud in the sky, out of which the aerolite seemed to descend; while at Bulloah the stone or stones were seen to fall as a luminous body, which at some part of its path appeared to scintillate in the air. The shell-like form, too, of the united fragments, in suggesting the idea of an internal cone or mass from which the external pieces have been severed, recalls to mind the suggestion of Mr. Benjamin Marsh, that the bursting of the meteorite is the result of the expansion produced by heat. If we couple with Mr. Marsh's suggestion the remarkable explanation by Hofrath Haidinger of the intense coldness declared to have been exhibited by the Dhurmsala stones after their having fallen quite hot, I believe that suggestion will prove a very fertile one. The coldness of the cosmical space must be shared by bodies wandering therein without atmosphere.

Such a body entering with planetary velocity the terrestrial atmosphere, is arrested in its course with an abruptness of which it is difficult to get a clear conception as is that velocity itself. The intense heat instantaneously developed on the surface of the mass will assuredly be sufficient to melt that surface down into an enamel, before it could have time to penetrate to even a sensible depth into the body. If, as is probable, this fused and white-hot enamel flies off from the mass as it proceeds with the scream of a huge projectile through the air, its place will be continually taken by a fresh and continuously flowing stream of the same incandescent material. That material, too, is combustible. The metallic iron in many an aerolite ranges above 20 per cent, and is associated with sulphur as pyrites, and sometimes in other forms. Here at least is cause for much, if not a sufficient cause for the whole, of the spectacle exhibited by the blaze of a meteor. That the air itself is also heated to whiteness, and as such becomes visible, as Haidinger suggests, is highly probable, and would add still more to the brilliancy of the light.

But while the enormous velocity of the body is thus instantaneously arrested and converted into heat, the effect of that heat will not be exhibited in the molten spray of enamel alone. The heated surface will gradually, but by no means slowly, impart its heat to the interior; and notwithstanding the non-conducting character of the stony ingredients of an aerolite, the outer portions (a sort of shell around it) will rapidly rise in temperature.

The coldness of the interior would only gradually be overcome, and, long before it would be so, the expansion of the external parts would tend to tear them away from a contracted and far more than ice-cold core within. The limits and the form of that core, the conditions under which disruption would ensue (indeed, whether it would ensue at all, as it would not if the mass were absolutely homogeneous), would depend on the structure of the mass, its directions or planes of weaker aggregation, or perhaps the unequal distribution in it of matter of various degrees of conductibility. But when the disruption comes, it must come with explosion.

The process may be repeated, or it may take place at different intervals on the different sides of the meteorite. The earlier explosions may take place at points in its path where there is still velocity enough to produce a fresh enameling,—sometimes in a copious flow, at others only enough to barely glaze the exposed surface of the stone again; the later ones may occur when the velocity is more nearly spent, and the friction is no longer competent to generate the glaze.

The cloud in the air, out of which the meteorite has been seen to come in so many authenticated instances, would be satisfactorily explained by the dust of the enamel after its separation from the aerolite in its course, and the combustion of its iron, sulphur, &c.; perhaps, also, small fragments are splintered and fly off by the same principle as the larger ones, and, partially burning, becomes dust too.

Following in the track of the body, this dust would soon, however, linger behind it and hang in the air like a vapor-cloud, as is often seen to be the case in the wake alike of a meteor and of a meteorite.

Finally, if the reports represent the successive concussions of the air produced by the disruption of the aerolite (and reaching the ear generally in the inverse order of their occurrence in time), we must attribute the "thunder" that is so often described as succeeding the reports, to the echo of the reports themselves.

That a noise, the true extent of which is likely to be exaggerated, should be heard over so large a range of country as sixty linear miles, is perhaps not so surprising when we consider the distance to which a small cannon may be heard, even over a surface of country teeming with obstacles and air-currents calculated to impede the passage of the sound; whereas from a height of two or three miles in a still, clear air, the spread of even a comparatively small sound over an area with a radius of thirty or forty miles seems nothing astonishing. To me, at least, who have heard the roar of a train between Shrivenham and Swindon, as I stood, on a still night, in the station at Cirencester, a distance of certainly nearly twenty miles, such a wide promulgation of a sound in the air is no more difficult to understand than it is to credit the assertion of our aeronauts, who a few months back heard a musical instrument played on the earth when their balloon was some three miles above the ground. That this propagation of the sound of a cannon or a train is not due to the conduction of the earth, is proved by the fact that it is only in certain states of the atmosphere, independent of wind, that it occurs.

The cause to which I have assigned the disruption of a meteorite, and the reports which accompany it, may also furnish an explanation of the great size of some aerosiderites and siderolites, as compared with that of the largest stones. The more rapid conducting power of the metal, as well as its greater power of resisting a divellent force, would—perhaps after a first disruption—tend to prevent the repeated breaking up of the mass; and this may be the case in many instances, notwithstanding the fact that, in others, meteorites of this kind have fallen in associated and perhaps dissevered masses or even in showers.

ART. XI.—*The Sun and Stars photometrically compared;* by ALVAN CLARK.

IF we place a lens of known focal distance, one foot for instance, between the eye and a star of the first magnitude, or one of any considerable brightness, with conveniences for guiding its movement in distance, to any point where it may be needed, and find the star just visible, or reduced to a sixth magnitude, when the lens, if a convex, is eleven feet from the eye, it becomes clear that, since the star has undergone a reduction of ten diameters, it would be visible, if removed in space to ten times its present distance. This, however, is on the supposition that no absorbing or extinguishing medium exists in space.

If a concave lens be employed, the measure must be commenced at the lens itself, but if convex, at the focal point; or once the focal distance must be subtracted from the measure, and the number of focal distances remaining corresponds to the number of reductions under which the object is viewed.

Castor is visible, when reduced,	10·3	times
Pollux,	11	"
Procyon,	12	"
Sirius,	20	"
The full Moon,	3,000	"
The Sun,	1,200,000	"

I have actually seen the sun under such a reduction; attended by circumstances which have led me to believe that it is about the limit at which the naked human eye could ever perceive this great luminary.

I have an under-ground, dark chamber, 230 feet in length, one end terminating in the cellar of my work-shop, and the other communicating with the surface of the ground by a vertical opening, one foot square, and five feet deep. In a moveable partition, between this opening and the end of the chamber, a lens of such focal distance as I choose can be inserted. A twentieth of an inch focus I have employed, of the best finish possible; its flat side cemented to one face of a prism with Canada balsam.

No light whatever can enter the dark chamber, except through this little lens. A common, plane, silvered, glass mirror, placed above-ground, over the vertical opening, receives the direct rays from the sun, and sends them down into the prism of total reflexion, by which they are directed through the little lens into the chamber.

An observer, in the cellar, 230 feet distant, sees the sun reduced 55,200 times; and its light, in amount, varies but little from that of Sirius.

Upon a little car, moveable in either direction, by cords and a pulley, is mounted another lens, with a focal distance of six inches. The eye of the observer is brought into a line with the lenses, or so near it, that he sees the light through the six inch lens; then, by the cord, he sends the car into the chamber, to the greatest distance at which he can see the light, like that from a star of the sixth or seventh magnitude.

At noon, March 19th, with a perfectly clear sky, I found the sun visible through the six inch lens, when it was removed to the distance of 12 feet from the eye. The distance between the lenses being 218 feet, the reduction by the small lens, if viewed from the point occupied by the six inch lens, would be 52,320 times; and that again by the six inch, distant from the eye 12 feet, or 24 times its focal distance, is reduced 23 times; making the total reduction 1,203,360 times.

It becomes now an important matter to ascertain as nearly as possible the proportion of light lost, by and through the media above described; the looking-glass, the prism, and two lenses; though joining the little lens with balsam to the prism, it may be regarded as one piece.

I have only investigated by experiments with artificial lights; but I find, when the mirror is placed at the angle which the sun requires at the date above given, the difference in the distance at which a direct light, and the same light reflected, is brought to a *minimum visibile*, does not exceed one-eighth part of the entire distance, and could not reach one-seventh, when the prism and lenses were interposed.

Again; the image of the highly illuminated atmosphere, for some degrees about the sun, is admitted with the sun's direct light, through the little lens, to the dark chamber; and the light, thus augmented, is observed in contrast with a darkness greater than that of a clear nocturnal sky. The entire loss by reflecting and absorbing is manifestly so small, and the light of the sky in the immediate vicinity of the sun, so great, that I can readily believe the waste, in effect, is fully made up; especially when considering the absolute blackness of the ground, upon which the light, in the dark chamber, is projected; and I can find no reason to doubt that the sun would appear as a star of the sixth magnitude, or be only just visible to the unassisted human eye, even setting aside the idea of an extinguishing medium, if removed 1,200,000 times his present distance; and at 100,000 times his present distance, he would only rank as a pretty bright star, of the first magnitude; though his parallax would be double that imputed to any star in the whole heavens. If his intrinsic splendor generally proves to be less than that of those stars whose distances have been measured, we need not infer that it is less than the average of existing stars; for, in case of a diversity among them, bearing any proportion to that among organic bodies, on the face of the earth, or the planets of our system, where the numbers are so comparatively small, the *visible* stars, would, of course, exceed, upon the average, our sun; for, by the laws of perspective, the small ones would be lost to our view, at distances from which the brighter individuals would appear as conspicuous objects.

Such would be the case with telescopic magnitudes, as well as with those visible to the naked eye.

The number of stars visible, by aid of the more powerful telescopes, is far less, in proportion to the power of the instruments, than those visible to the unassisted eye, or with smaller telescopes.

This fact has given rise to the doctrine of an extinguishing medium in space; which is accepted by the most able astronomers as the truth, and has been the foundation of much ingenious reasoning.

Plausible or probable, as this appears, I see no difficulty in understanding that an exceedingly great diversity in the intrinsic brightness of the stellar orbs, promiscuously scattered through space, *might* result in the same appearances as those on which this doctrine is founded. For, at the smaller distances, we should see the whole, both great and small, when using only moderate powers; but in the regions bounding the remotest reach of the great telescopes, though the great and the small might be there, it would be only the great that we should see; and those only as the most minute specks of light that can be imagined.

The vast number of smaller, or more moderate lights, like our sun, which may remain concealed among those of extraordinary splendor, yet so remote as only just sensibly to impress our vision when aided to the utmost that human skill can do, will be better understood when we consider the ratio in which an increase of radius increases the cubic contents of a sphere.

Upon the outer limits of such a sphere as would embrace the great mass of telescopic stars, a moderate depth, extended round the whole, would afford an immense amount of room for stars of all imaginable sizes. I desire to be particularly understood, that it is in those very remote regions, or beyond where any telescope, now in use, can possibly show stars of the average, or smaller sizes, that we may look for the modification introduced, by such supposed diversities, into the investigation of this doctrine of an absorbing medium.

Were all the stars in existence of one pattern, one uniform brightness, scattered broadcast through all space, I think the great telescopes would count up more nearly the numbers belonging theoretically to their powers than they now do.

However, with these suggestions, I leave this interesting branch of my subject for the present.

The ratio, in which the light from a celestial object diminishes with an increase of distance, needs no explaining; and I will close by briefly giving, in tabular form, my own results, with those published by Mr. Bond of the Harvard College Observatory, and by Dr. Wollaston, in vol. cxix of the *Philosophical Transactions of the Royal Society of London*, of comparisons between the bright star α Lyræ and the sun.

To bring the magnitude of our sun to an equality with that of this star, his distance would require to be increased, according to

Wollaston, nearly - - - -	425,000 times.
Bond, " - - - - -	155,000 "
Clark, - - - - -	102,000 "

The light received from these luminaries differs, according to

Wollaston, as - - - -	180,000,000,000 to 1
Bond, " - - - -	24,000,000,000 "
Clark, " - - - -	10,400,000,000 "

I have alluded to the light in the atmosphere about the sun, as giving an increase to his photometrical force; though I am aware that such must be the case with a star; and it must bear the same proportion to the star's light, that it bears to the sun's light.

The difference, in effect, is here; we have several thousand stars playing into our atmosphere at once; but only one sun.

If the distances imputed to several of the stars, from parallax, can be true, I am sure, those having the taste, talent, and leisure, necessary for following up photometrical researches with efficiency, cannot fail to find our glorious luminary a very small star; and to the human understanding, thus enlightened, more than ever, must the heavens declare the glory of God.

P. S.—Since the above had left my hands for the press, I prepared a close covering for the vertical opening to my dark chamber, with a circular perforation, subtending at the prism an angle of 32′; and substituted for the little lens one having a focal distance of one-eighth of an inch. By this arrangement, with the mirror placed above, and an eye-hole by its side, the sun light would be directed upon the prism, or just beside it, at pleasure.

I assumed that when the pencil was made to fall entirely outside of the lens, I was viewing a portion of the sky just equal in form and area to the sun itself, close by its side.

Allowing the direct light to pass centrally over the prism, I found the image visible for more than one minute after the last direct ray from the sun had left the line of the lens, although reduced nearly 22,000 times.

After proceeding thus far, it appeared to me, that could the sun be reduced to a *minimum visibile*, *without* reflexions, and the lenses so arranged that both eyes could be employed in observing, the results would be more satisfactory.

By removing the object-glass, eight inches in diameter, from the tube of my equatorial, and placing a lens in the eye-tube, one-twentieth of an inch in focal distance, and turning the eye end toward the sun, with the eyes 100 inches from the lens, I obtained such a view as the sun would present if removed 2,000 times his present distance. To accomplish the further necessary reduction, I applied an extension sliding tube carrying a lens one-thirty-fourth of an inch focus.

To my surprise the sun was visible when the distance between the lenses was such as to give a reducing power of two millions. But, upon examination, it appeared that light was copiously reflected upon the lens nearest to the eye, from the inner surface of the bright brass extension tube. After diaphragming and properly darkening this tube, on some occasions of very clear skies, a power of nearly 1,600,000 was required to send the sun entirely out of sight. Were not the use of both eyes, the avoidance of reflexion by the looking-glass and prism, and the increased altitude of the sun sufficient to explain the difference between this and the dark chamber observations, I might add, the angle of aperture of the lens nearest the sun is 80° and used without a screen, while only a much smaller proportion of the bright sky was sent down by the mirror into the prism. Seated comfortably upon the observing chair, with the driving clock acting, the face in the mouth of the tube nearly to the ears, and a folded blanket drawn over the head, so as to exclude all light, I usually gaze listlessly into total darkness for about two minutes, that the pupils may expand, and the eye become prepared for its work. I then slowly remove or raise the blanket upon one side for the admission of light, barely sufficient to show the direction of the lens. A curious effect is here witnessed. Though no view of the sun had previously been obtained, and the eyes had wandered perhaps many degrees, they are suddenly arrested by the appearance of a good plump sixth magnitude star.

On closely drawing the blanket again, it fades to the faintest perceptible point, or disappears altogether, and the eyes wander from its place in spite of every effort to keep them upon it. But when the slightest ray is again allowed to enter past the head, it is reflected from the lens, and combining with the light transmitted from the sun becomes conspicuous as before.

This I have found to be the case when neither the transmitted nor the reflected light alone could stimulate the visual organs to a recognition of its presence.

Since noticing this fact, I invariably close every avenue to the admission of light about the head, preparatory to the final effort, and if the sun is not seen within five minutes, I give it up, reduce the distance between the lenses about one-twentieth, and try again. My eye gains about its maximum power for such purposes in three minutes. I am on my guard against the consequences of allowing insufficient time. When a view is obtained estimated equal to a faint sixth magnitude, the distance between the lenses is measured, and the reducing power computed. I do not, it will be seen, trust to any comparisons with artificial lights as standards, but make a *minimum visibile* the standard in all cases; which leaves the eyes and attention free to pursue one object at a time. A movable brass plate, with a perforation one-tenth of an inch in diameter, is placed nine inches beyond the lens next the sun, for admitting his direct rays, with only a very narrow annulus of surrounding skylight.

The following results were obtained April 28.

6^h 30^m A. M.	1,055,360	sky admitted.
6 40	783,100	sky screened off.
12 noon,	1,308,000	sky screened off.
12 10 P. M.	1,574,400	sky admitted.

These morning observations were made for the benefit of a friend, who wished them for a special purpose.

The numbers in the noon observation are very near the maximum exhibited in many extreme efforts, made when the skies were remarkably clear.

It makes an enormous difference, it will be noticed, whether the sun is observed with the atmospheric light screened off or not; but, to give the sun and the star equal conditions, the eye should in both cases be shielded against the light from surrounding regions. If we could condense into a compass of less than the fourth of a second all the sky-light within 40° of a faint star, and add it to the star, it would give a manifold increase to its brightness. This is what occurs in observing the sun without the screen; though the atmospheric light in question is, by day, from the sun alone, but by night, from the host of stars of all magnitudes; which would make the conditions monstrously unequal; and this indicates the importance of clearing it away in both cases as effectually as possible. The method by which it is cleared from the sun is already explained; but for dealing with a star, I remove all the lenses from the finder tube to my equatorial, and place in each end of it, a disk of brass, with a central perforation, one-fifth of an inch in diameter, while another similar disk is placed upon an arm extending beyond the object end of the main tube, ten feet from the eye, upon a direct line with those in the finder. When a star occupies a given point in the field of the telescope, it is also in a line with the centres of these holes. But the plan is faulty; inasmuch as it is going back to the use of one eye; and since the holes must equal in diameter its expanded pupil, they admit sufficient light from the blank sky to be seen, if a sharp effort is made; though when that from a star of the seventh magnitude is brought in with it, an augmentation is quite apparent, and a bright sixth magnitude, like 32 Bootis, is instantly and constantly seen with the single eye, through the screen, with two pieces of plate glass interposed. Mitchell has estimated, from Bouguer's observations, that the sun would equal a star of the sixth magnitude, if he were removed to ten millions of times his present distance, which is nine times as much as could possibly be required, so far as I can understand by these experiments, to give him companionship with the star 32 Bootis.

Cambridgeport, May 26, 1863.

ART. XII.—*On Glucinum and its Compounds;* by CHARLES A. JOY, Professor of Chemistry in Columbia College, New York.

THE distinguished crystallographer, Haüy, having discovered a perfect identity in crystalline form, hardness, and specific gravity between the minerals beryl and emerald, requested Vauquelin to subject the former to a careful analysis. Beryl had previously been analyzed by Bindheim in 1790, with the following result:

$SiO_3=64$, $Al_2O_3=27$, $CaO=8$, and $Fe_2O_3=2$. Total 101.

Vauquelin[1] fused 100 parts of finely pulverized beryl with 300 parts of caustic potassa, and separated the silica (69 p. c.) in the usual way. He then precipitated the earths by carbonate of potassa, and digested the precipitate in caustic potassa, by which a portion, amounting to nine per cent, was again thrown down.

This property of re-precipitation from caustic potassa, in one portion of the beryl, attracted his attention, and the fact that he could not obtain an alum with it, when its sulphate was mixed with the sulphate of potassa, lead to the final discovery of glucina.

Vauquelin did not give a name to the new earth, but left it to his colleagues to propose one. In consequence of its forming salts of a sweetish taste, they called it *glucina*, from γλυκύς, sweet, γλυκύ, sweet wine, γλυκαίνω, to render sweet. The German chemists, however, have preferred the name *berylla*, from the mineral in which it was first found.

Since the days of Vauquelin, a number of minerals containing glucina have been added to the list. The following catalogue of these minerals, together with the literature of the subject, is believed to be tolerably complete.

1. *Alexandrite*—same as chrysoberyl.
2. *Alvite.*—Dana, Sup. iii. 5. Nyt. Mag. f. Nat., xiii, D. Forbes and T. Dahll.
3. *Aqua-marine.*—Vauquelin, Ann. de Chim. et Phys., [1], xxvi, 155. Hermann, Ann. de Chim. et Phys., [2], xix, 361. Don Pedros, magnificent specimen of, British As. f. Adv. Sci., i, 86. Du Menil, Schwgg. J., xxxiv, 454. Dana, Min., 178. Rammelsberg, Hdb. d. Min. chem., 553. Hausmann, Min., 603, 887.
4. *Beryl.*—Plin. Hist. Nat., xxxvii, 5, s. 20. Hard., ii, 776. Irenæus contra hæreses Ed. Ren. Massuet., 1710, Lib. i, proem. § 2, p. 2. Theophrast. de lapid., §§ 44, 45, 46. Klaproth, Beiträge. i, 9, iii, 215. Werner, 40, 41. Haüy, Traité, ii, 504. Gren., J. d. Phys., ii, 421, 1795. Graf v. Veltheim, Sammlung einige Aufsätze histor. antiquar. Miner. u. Ahnl. Inhalts., 1800, ii, 134. Carl Ritter, Erdkunde, i, Africa, 2 A., 673–677. Beckmann's Beitr. z. Gesch. d. Erfind., iii, 297. Wilken, Gesch. d. Kreuzzüge. Beil. 8. Du Menil, Schwgg. J., xxxiv, 454; id. xxxix, 487. Fusion of beryl, Schwgg. J., xviii, 237; id., xix, 320. Apatite mistaken for beryl (απαταω, to deceive). Gilbert, J. d. Phys., xvi. 126 and 250. Hausmann, Handb. d. Min., ii, 603. Von Leonhard, Handb., 391. Beud. Traité, ii, 41. Phillips, Min., Brooke and Miller, p. 336. Ausserord, Beilage zur Augsb. Allgem.

[1] Ann. de Chim., [1], xxvi, 155, Feb. 15, 1798.

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5. *Chrysoberyl.*—Haüy, Traité, ii, 303. An. de Mus. d'Hist. Nat., 1811, xviii. Gilbert, Ann., xli, 53, from Haddam, sent to Haüy by Mr. Bruce of New York, 1810. Hausmann, Min., ii, 430. Henry Seybert, Trans. Am. Phil. Soc., March 5, 1824, p. 116. Schwgg. J., xlii, 228. Thomson's Outlines, i, 400. Awdejew, Pogg. Ann., lvi, 118. Ann. Chem. Pharm., xliv, 270. Plinius, xxxvii. Hard. ii, 776. Klaproth, Beit., i, 97. G. Rose, Pogg. Ann., xlviii, 570. G. Rose, Reise nach dem Ural, ii, 379. Pott, K. K. Gesl. f. d. gesam. Min. St. Petersburg, i, 116. Werner, 5. V. Leonhard, Handb., 539. Beud., Traité, ii, 145. Mohs, Phys., 342, fig. 37. Damour, Ann. Ch. Phys., [3], vii, 173. Pogg. Ann., lix, 120. Dana's Min., 122. Moore's Anc. Min., 151. Jameson's Min., i, 202. Arfvedson, Vet. Acad. Hand., 1822. Schwgg. J., xxxviii, 4. Bergemann, Dissertat. Göttingen, 1826. Artificial, Caron and Deville, Ann. Chem. Pharm., cviii, 57. Rose, ib., lxiv, 287. Ebelmen, ib., lxviii, 265; ib., lxxx, 207. Am. J. Sci., [1], ii, 240; ib., iv, 37. Haidinger, Pogg. Ann., lxxvii, 228. Schwgg. J., l, 329; ib., li, 251. Cleaveland, Min., 204.

6. *Cymophane*—same as chrysoberyl.

7. *Davidsonite*—same as beryl, Phil. Mag. [4], xii, 386. Ann. Chem. Pharm., xix, 154.

8. *Emerald.*—Haüy, identity of emerald and beryl, Ann. Chim. Phys., xxvi, 156. Vauquelin, Ann. Chim. Phys., xxvi, 264. Am. J. Sci., [1], ii, 354. Klaproth, Beitr., iii, 215. Chemical Essays, London, 1801, 176. Moberg, Acta. Soc. Sci. Fenn., ii, 81. Ville, Comp. Rend., xli, 698. Lewy, ib., xlv, 877. Ann. Chim. Phys., [3], liii, 51. Rép. Chim. Appl., 1858–59, 27. Du Péron, Ann. Chim. Phys., xxiii, 68; ib., xiv, 64. Dumas, Séances de l'Acad., 1855. De Senarmont, ib., 1859. Dana's Min., 178. Cleaveland's Min., 274. Hausmann's Min., 603.

9. *Euclase.*—Vauquelin, J. des Mines, No. 28, 258. Haüy, Traité, ii, 528. Werner, 39. Beud, Traité, ii, 32. Mohs' Phys., 351. Weiss, Verhand d. Gesells. Natf. F., Berlin, 1820, 110. Weiss, Abhd. k. Akad. d. W., Berlin, 1841, 249. Berzelius, Schwgg. J., xxvii, 73. Schabus, Monograph. Wien Ak. d. W., 1852. Pogg. Ann., lxxxviii, 608. Kokscharow, Pogg. Ann., ciii, 348. Bulletin de l'Ac. de St. P., xvi, 284. Material zu Miner Russ., iii, 131. Hausmann, Handb. d. Min., 601. Von Leonhard, Handb., 395. Levy, Edin. Phil. J., xiv, 129. Mallet, Phil. Mag., [4], v, 127. J. f. p. Chem., lviii, 447. Damour, Comp. Rend., xl, 944. J. f. p. Chem., lxvi, 154. Dana, Am. J. Sci., xvi, 96. Ann. Chim. Phys., xi, 216; ib., xii, 26. Dr. Brewster, Edinb. J., ix, 217. Gilbert's J., lxix, 1. Schwgg. J., xxxiii, 106. Zinken, Schwgg. J., xxvi, 372; xxvii, 73. Rammelsberg, Hdb. d. Min. Chem., 570. Cleaveland's Min., 278.

10. *Gadolinite.*—Berzelius, Schwgg. J., iv, 51; ib., xxiii, 194; ib., xxvi. Afhandl. i. Fys., v, 54. Thomson, Steel and Richardson, Phil. Mag., vii, 430. J. f. p. Chem., viii, 44. Schwgg. J., viii, 450. Thomson's Outl., i, 410. Ekeberg, Ann. Chim. Phys., xliii, 228. Gilbert's Ann., xiv, 247. Scheerer, J. d. Chem., iii, 187, 1794. Mém. de l'Akad. de Stock., 1797. Dana's Min., 211; 4th Sup., 119; 6th Sup., 351. J. f. p. Chem., lxxiv, 271. Von Kobell, Schwgg. J., 1834. Pogg. Ann., ciii, 314; ib., lix, 101. Berlin, Berz. Jahr. B., xvii, 220. Schwgg. J., xiv, 33; ib., xvi, 404; ib., xxi, 261. Connel, Edinb. n. Phil. J., June, 1836. Ann. Chem. Pharm., xlviii, 224. Pogg. Ann., li, 414; ib., lvi, 479. Klaproth, Beit., iii, 52; ib., v, 173. Ann. Chim. Phys., xxxvi, 143; ib., xxxvii, 87; ib., xliii, 278; ib., xlix, 124; ib., lxii, 208; ib., [2], ii, 411; ib., [2], iii, 26. Brcht. d. k. Akad. d. W., Berlin, 1801, p. 16. Hausmann, Min., 542, 1590. Geyer, Crell. Ann. d. Chem., 1788. Descloizeaux and Damour, Ann. Ch. Phys., [3], lix, 357. Cleaveland's Min., 205. Orthite and Gadolinite, Pogg. Ann., lix, 103. Berz. Jahrb., xxiv, 318; ib., xxv, 365.

11. *Goshenite*—same as beryl.

12. *Helvine.*—Vogel, Schwgg. J., xxix, 314. C. Gmelin, Pogg. Ann., iii, 53. Rammelsberg, Pogg. Ann., xciii, 453. Breithaupt, Gilbert's Ann., lxiv, 42. Dana's Min., 194; 1st Sup., 9. Hausmann's Min., 870. Werner, 23. Triesleben's Beiträge, i. Haüy, Traité, ii, 168. V. Leonh. Hab., 462. Beud., Traité, ii, 168. Mohs' Phys., 397, fig. 206.

13. *Leucophane.*

14. *Melinophane.*—Th. Scheerer, J. f. p. Chem., lv, 449. Greg, Phil. Mag., [4], x, 510, 1855. Descloizeaux, Ann. Chim. Phys., [3], xl, 76. Rammelsberg, J. f. p. Chem., lxviii, 245. Pogg. Ann., xcviii, 257. Berzelius's Jahrb., xxi, 168. Dana's Min., 182; 2d Sup., 13; 4th Sup., 121. A. Erdmann, Vet. Acad. Hand., 1840, 191. Awdejew, Ann. Chem. Pharm., xliv, 270. Phillips's Min., 856. Hausmann's Min., 888. Rammelsberg, Handb. Min. Chem., 763.

15. *Phenacite.*—Hausmann, Handb. d. Min., 538, 1590. Nordenskiöld, Pogg. Ann., xxxi, 57. K. v. Ac. H., 1823, 1860. Beirich, Pogg. Ann., xxxiv, 519; ib., xli, 323. Ann. Chem. Pharm., xvi, 251. Mohs' Phys., 353. R. Hermann, Bullet. Soc. Imp. Mos., 1844, iv, 877. Hartwall, Ann. Chem. Pharm., xiv, 82. Berz. Jahrb., xiii, 157. Pogg. Ann., xxxi, 57; ib., cxxxii, 120. Ann. Chem. Pharm., xvi, 251. Phil. Mag., [4], iii, 378. Dana's Min., 189. Am. J. Sci., [2], xvii, 78. G. Rose, Pogg. Ann., lxix, 143. Rammelsberg, Hdb. Min. Chem., 553. Awdejew, Ann. Chem. Pharm., xliv, 270. Deville, artificial, Ann. Ch. Pharm., cxx, 178.

16. *Smaragd*—same as beryl.

17. *Tyrite.*—Phil. Mag., [4], xiii, 91. Dana's 4th Sup., 129.

As my object was to procure a supply of glucina, a re-examination of the above minerals was not deemed necessary. One hundred pounds of fragments of beryl were obtained from Acworth, New Hampshire, and ground in a gold-quartz mill, and decomposed according to the following methods.

Methods for the decomposition of beryl.

1. By passing chlorine gas over a calcined mixture of lamp-black, oil, and beryl.

2. By treating beryl with concentrated hydrofluoric acid and sulphuric acid.

3. By digesting seven parts of beryl and thirteen parts of fluor spar in eighteen parts of concentrated sulphuric acid.

4. By fusing beryl with three parts of fluorid of potassium, and digesting in sulphuric acid.

5. By fusing beryl with fluorid of ammonium, and digesting in sulphuric acid.

6. By digesting fifty parts of beryl in thirty parts of sulphuric acid, and fusing with one hundred parts of ferrocyanid of potassium and seventy parts of chlorid of sodium.

7. By fusing beryl with fluor spar.

8. By fusing beryl with half its weight of caustic lime.

9. By fusing beryl with litharge.

10. By fusing beryl with binoxyd of manganese.

11. By fusing beryl with three parts of carbonate of potassa and two parts of carbonate of soda.

12. By fusing beryl with two parts of carbonate of potassa.

1. *By chlorine gas.*—Finely pulverized beryl was intimately mixed with lamp-black and linseed oil, and calcined. Chlorine gas was passed over it at a red heat in a porcelain tube, and the more volatile chlorids of silicon and iron driven into the receiver. The chlorids of glucinum and aluminum were collected in the further end of the tube. The beryl was completely decomposed. An unsuccessful attempt was made to take advantage of the difference in the points of volatilization of the chlorids of iron, silicon, aluminum, and glucinum, in order to separate them.

2. *By hydrofluoric acid.*—This well-known method was tried for comparison, and, where the beryl was finely pulverized, was entirely successful. The glucina was separated from the alumina by carbonate of ammonia.

3. *By fluor spar.*—This method was proposed by Scheffer.[2] Seven parts of beryl, thirteen parts of fluor spar, and eighteen parts of concentrated sulphuric acid were gently heated in a leaden trough under constant stirring, for two hours, and then transferred to an iron vessel, and heated sufficiently to expel the fluorid of silicon and the excess of sulphuric acid. The decomposition is fully accomplished in this way, and the only objections to it are the presence of so much sulphate of lime in the solutions, and the difficulty in expelling the excess of sulphuric acid.

4. *By fluorid of potassium.*—One part of beryl was fused with three parts of fluorid of potassium, and digested in sulphuric acid. If this flux could be obtained in sufficient quantity, the method would be preferable to all others, as the mass fuses easily, the fluorid of silicon is driven off at a gentle heat, and the potash alum crystallizes readily, carrying down all of the alumina, thus at the same time accomplishing the decomposition of the silicate and the separation of the alumina and glucina.

5. *By fluorid of ammonium.*—Four parts of beryl were intimately mixed with nine parts of fluorid of ammonium, gently heated in a capacious platinum crucible, and fused at a low red heat. The mass was covered with an excess of sulphuric acid,

[2] Ann. Chem. Pharm., cix, 144.

and evaporated, care being taken to prevent the formation of nearly insoluble fluorid of aluminum by too great heat.

This method[3] for the analysis of silicates is one of the best ever proposed, and, as fluor spar is abundant, there is no reason why it should not be frequently applied.

6. *By ferrocyanid of potassium.*—The method of Corbelli,[4] for obtaining aluminum from its compounds, was applied to beryl. Fifty grammes of beryl and thirty grammes of sulphuric acid were digested for two hours, and the heat raised to 500° C. After cooling, one hundred parts of dry ferrocyanid of potassium and seventy parts of chlorid of sodium were added, and the mass exposed to the highest heat of an anthracite fire. The result was a button of iron, but no glucinum. The beryl was only slightly decomposed. A trial with an alumina salt also yielded a button of iron, but no aluminum.

7. *By fusing with fluor spar.*—One hundred and eight parts of beryl and one hundred and sixty parts of fluor spar fused very readily, but required close attention to prevent the mass from running through the iron crucible. The complication in the separation of the earths, occasioned by the introduction of so much lime, was found to be inconvenient in this method.

8. *By caustic lime.*—Two parts of beryl and one part of caustic lime were fused in a hessian crucible. The mass melted readily, but, as in the previous example, required care to prevent it from running through the crucible. This method[5] is rapid, requires only a low heat and cheap material, and would be highly advantageous, if suitable crucibles could be provided. I modified it somewhat, as follows: by fusing one part of lime, two parts of beryl, and three and a half parts of gypsum. (Two and one and a half parts of gypsum were tried.) This readly decomposed the beryl, forming a beautiful glass, and did not run through the crucible; but so much lime in the solution was an objection.

9. *By litharge.*—One hundred grammes of beryl were intimately mixed with three hundred grammes of litharge in an iron crucible, and gradually heated to fusion. The mass was then stirred with an iron spatula, and poured upon a marble slab. The cold slag had a dirty yellow color, and was soft and easy to pulverize. It was reduced to a fine powder, moistened with water, digested in an excess of nitric acid, and evaporated to dryness. The silica was separated, and the filtrate was left until nitrate of lead had crystallized out. The balance of the lead was separated by sulphuric acid, and the requisite amount of sulphate of ammonia then added to form an alum with the sulphate of alumina. This method with litharge proved to be

[3] Rose, Pogg. Ann., cviii, 19.
[4] Rep. pat. inv., Oct. 1858, 300.
[5] Debray, Ann. Ch. Phys., [3], xliv, 5.

admirable, and is highly recommended for the decomposition of other silicates. The yield of glucina was not quite equal to that obtained for the fusion with carbonate of potassa.

10. *Binoxyd of manganese.*—Two parts of finely pulverized beryl were intimately mixed with three parts of binoxyd of manganese, and exposed to the highest heat of an anthracite coal furnace for two hours. The mass fused completely, affording a dark glass resembling obsidian, hard and difficult to pulverize. It was digested in concentrated sulphuric acid, and the silica separated as usual. The beryl was fully decomposed. My object was to see whether the binoxyd of manganese would decompose silicates in this way, and, further, to try if an alum with the *protoxyd* of manganese ($MnO, SO_3+Al_2O_3, 3SO_3+24HO$) could be formed. No such alum was obtained, thus confirming previous experiments in this direction.

11. *Carbonates of soda and potash.*—By fusing beryl with a mixture of two parts of carbonate of soda and three parts of carbonate of potassa, the mineral is easily decomposed, and, where the earths are separated by carbonate of ammonia or chlorid of ammonium, the method is very convenient. It has no advantages, however, over the succeeding and last method.

12. *By carbonate of potassa.*—One part of finely pulverized beryl and two parts of carbonate of potassa fused very readily in a platinum crucible. It will be seen, under the head of the separation of alumina and glucina, that this method was preferred to any other.

All of the above methods, and numerous others which it is not necessary to recapitulate, were subjected to repeated trials in my laboratory, and a large supply of glucina obtained for use in the further prosecution of this investigation.

Methods for the separation of glucina and alumina.

1. Chlorid of ammonium.
2. Carbonate of ammonia.
3. Caustic potassa.
4. Sulphurous acid.
5. Carbonate of baryta.
6. Hyposulphite of soda.
7. Decomposition of nitrates.
8. Acetate of soda.
9. Fusing with caustic potassa.
10. Formate of ammonia.
11. Decomposition of sulphates.
12. Potash alum.

1. *By chlorid of ammonium.*—The oxyds of iron, alumina, and glucina were precipitated by ammonia, and the precipitate digested in a concentrated solution of chlorid of ammonium, with constant replacement of the evaporated water. The iron and alumina, being insoluble in sal-ammoniac, are collected upon a filter, and the glucina precipitated from the filtrate by sulphid of ammonium.

This method, proposed by Berzelius,[6] was regarded by Wee-

[6] H. Rose, Handb. d. Analyt. Chem., ii, 61.

ren[7] as preferable to any other. My observations confirm the accuracy of the results to be obtained, if all of the precautions are observed, but the time required for the digestion of the mixture and the care to keep it at a proper concentration render it more tedious than other methods, without a corresponding increase in the yield of glucina.

2. *By carbonate of ammonia.*—The filtrate from the silica was dropped, with constant stirring, into a warm concentrated solution of carbonate of ammonia in excess, which precipitated the alumina and dissolved the glucina. The solution was left for some days in a corked flask, and occasionally well shaken. After separating the alumina by filtration, the glucina was obtained by distilling off the carbonate of ammonia and collecting on a filter. The carbonate of ammonia was thus saved for future operations. A serious objection to this method is the fact that considerable alumina is always dissolved in the presence of glucina, although alone it is not affected by carbonate of ammonia. I instituted a series of experiments in order to ascertain the degree of concentration of the carbonate of ammonia, and the length of time most favorable for the solution of the glucina.

One gramme of pure glucina was treated with carbonate of ammonia, under the same circumstances of temperature and concentration, for three, seven, eleven, and sixteen days. After ten days, with carbonate of ammonia of 1·080 specific gravity and 15° C., the maximum amount was dissolved. If the solution be kept longer than ten days, a precipitate of carbonate of glucina will begin to form, and at the expiration of sixteen days, fifteen per cent less of the original amount will go into solution. It was found advisable to separate the glucina after the expiration of a week. I observed that it was preferable to precipitate the two earths in the first instance with carbonate of ammonia, as the glucina was then more soluble than if first thrown down by ammonia and afterwards digested in the carbonate. Carbonate of potassa and carbonate of soda dissolve the precipitate of glucina, but with greater difficulty.

3. *By caustic potassa.*—This method was first proposed by C. G. Gmelin.[8] The solution of alumina and glucina in chlorhydric acid is neutralized by a cold solution of caustic potassa until the precipitate disappears; it is then largely diluted with water, and boiled in a platinum capsule for a quarter of an hour. The glucina will be precipitated, and must be carefully washed in hot water to free it of all traces of potassa.

By diluting the potassa with ten parts of water, and dissolving the glucina in chlorhydric acid, and re-precipitating by ammonia, I obtained very pure glucina, but always with loss of mate-

[7] Pogg. Ann., xcii, 91.

[8] Handwörtb. d. Chem., 2te Auf., ii, 1018. Pogg. Ann., xcii, 97.

rial, as a portion of the glucina remained in solution with the alumina, and where iron was present, I found that considerable quantities were dissolved by the potassa, notwithstanding every precaution was observed.

4. *By sulphurous acid.*—This method of Berthier,[9] founded upon the difficult solubility of the basic sulphite of alumina, did not succeed, as variable quantities of the sulphite of glucina were always thrown down with the alumina salt. The sulphite of ammonia was substituted for the sulphurous acid gas, but the result was the same. My observations confirmed the experience of Weeren[10] and Böttinger.[11]

5. *By carbonate of baryta.*—According to H. Rose,[12] carbonate of baryta does not precipitate glucina from cold solutions, while alumina under the same circumstances is precipitated. I found, however, in confirmation of the observations of Weeren,[13] that both glucina and alumina were precipitated.

6. *By hyposulphite of soda.*—Chancel's[14] method for the separation of alumina and iron was applied to the separation of alumina and glucina. Weighed portions of alumina and glucina were dissolved in chlorhydric acid, nearly neutralized by carbonate of soda, largely diluted, and to the cold liquid a slight excess of hyposulphite of soda added, and the whole boiled until fumes of sulphurous acid were no longer observed. It was found that the glucina was precipitated along with the alumina, and the method proved unavailing.

7. *By the decomposition of the nitrates of alumina and glucina.*—According to Deville,[15] if the nitrate of alumina be heated for some time to 200° and 250° C., all the nitric acid will be expelled, and a residue of granular alumina, insoluble in water, will be left in the capsule. Alumina can in this way be separated from BaO, SrO, CaO, KO, NaO, and MgO.

Weighed portions of the nitrates of alumina and glucina were heated together to 200° C., and afterwards treated with water. It was found that the nitrate of glucina was decomposed the same as the nitrate of alumina—affording no method for the separation of the earths.

8. *By acetate of soda.*—Alumina is precipitated from a boiling solution of acetate of soda, the same as the oxyd of iron, a method employed for the separation of alumina and iron from other bases. I found that the behavior of glucina was the same as alumina, and consequently this method was also unavailing.

9. *By fusing with caustic potassa.*—Weeren[16] says, if glucina

[9] Rose, Handb. d. Analyt. Chem., ii, 60. Berz. Jahrb., xiii, 148.
[10] Pogg. Ann., xcii, 99. [11] Ann. Chem. Pharm., li, 397.
[12] Rose, Handb. d. Analyt. Chem., ii, 57, 61.
[13] Pogg. Ann., xcii, 104.
[14] Comp. Rend., xlvi, 987. Ann. Chem. Pharm., cviii, 237.
[15] Ann. Chim. Phys., [3], xxxviii, 9. [16] Pogg. Ann., xcii, 106.

and caustic potassa be fused together, water dissolves out nothing, but if alumina be fused in the same way, it will be rendered soluble in water. This suggested a method for the separation of the two earths. Upon trial it was found that the glucina was equally soluble in water after fusion with caustic potassa.

10. *By formate of ammonia.*—Formate of ammonia precipitates iron, alumina, and glucina, and none of them are soluble in excess. This method was therefore not available.

11. *By the decomposition of sulphates.*—If sulphate of glucina be heated to redness, sulphuric acid and sulphur are driven off, and the pure oxyd remains. The sulphate of manganese is not decomposed by heat in this way. This method may be of application in the analysis of helvine, but can not be used to separate alumina from glucina, as the sulphate of alumina behaves, when heated, in the same manner as the sulphate of glucina.

12. *By the formation of potash-alum.*—One part of finely pulverized beryl was intimately mixed with two parts of carbonate of potassa, and fused in a capacious platinum crucible at an ordinary red heat. After cooling, concentrated sulphuric acid was poured over the mass, care being taken to prevent loss by effervescence, and the whole constantly stirred until it assumed a gelatinous condition. The excess of sulphuric acid was then expelled, and the silica determined as usual. The filtrate from the silica, containing the sulphates of alumina, glucina, iron and potassa, was concentrated by evaporation, and allowed to stand twenty-four hours, and sometimes longer, according to the amount of beryl taken, until a crop of alum crystals had formed. These were collected and washed, and the liquid evaporated until a second crop was gathered. The filtrate from the alum crystals was concentrated and poured into a hot saturated solution of carbonate of ammonia, and allowed to stand several days with frequent agitation. The insoluble portion was collected on a filter and digested a second time in carbonate of ammonia and afterwards in caustic potassa. The portion insoluble in caustic potassa was collected and weighed, and gave the percentage of iron. The filtrate from the iron was examined for alumina and yielded only traces, showing that all of the alumina was separated as alum. The glucina and alumina were determined as oxyds. This method was subjected to repeated trials, and was found to give better results than any other. The average composition of the beryl from Acworth, New Hampshire, determined in this way on a large scale, proved to be as follows: silica 68·84, glucina 13·40, alumina 16·47, sesquioxyd of iron 1·70. Total 100·41.

The consideration of the salts of glucina is reserved for a future communication.

Columbia College, New York, May, 1863.

ART. XIII.—*Remarks on the Luminosity of Meteors as affected by Latent Heat;* by BENJAMIN V. MARSH.

IN the January number of this Journal, in the notice of Mr. Quetelet's work, "*Sur la Physique du Globe,*" objection is made to certain "novel ideas on the constitution of the atmosphere," —apparently adopted by the author mainly for the purpose of explaining the fact that shooting stars and meteorites always disappear before reaching the earth. The notice says, "we observe these meteors at elevations of 140 to 160 miles; they increase in brightness as they approach the earth; they disappear entirely as they approach the lower part of the atmosphere, as if they entered a medium which had not the elements necessary for their continued brilliancy."

The aim of the present paper is to show that the well-established fact thus stated may be fully and satisfactorily explained by other means.

Mr. Birks, in his chapter on the "*Igneous condition of matter,*" says,[1] "There will thus, according to the present theory of the laws of matter, be more truth than has latterly been recognized in the old arrangement of the four elements, which placed a fourth region of fire above the solid, liquid, and gaseous constituents of our globe. In fact, above the region where the air, though greatly rarefied, is still elastic, there must be a still higher stratum where elasticity has wholly ceased, and where the particles of matter, being very widely separated, condense around them the largest amount of ether. All sensible heat, in the collision or oscillation of neighboring atoms of matter, will thus have disappeared; but latent heat, in the quantity of condensed ether or repulsive force ready to be developed on the renewed approach of the atoms, will have reached its maximum, and may be capable of producing the most splendid igneous phenomena, like the northern lights, or tropical thunder storms."

On reading the above, I was so struck with its peculiar adaptation to the explanation of meteoric phenomena, that I was induced to inquire, without any regard to the theoretical views of the author—what, according to the accepted laws of heat, must be the condition of the upper regions of the atmosphere in reference to latent heat?

"If a unit of weight of any gas, allowed to expand freely without change of pressure, is heated from the freezing point one degree, the amount of heat thus absorbed, measured in fractions of the unit, is called 'the specific heat under constant pressure.' If the same gas is heated one degree when so confined

[1] On Matter and Ether, or The Secret Laws of Physical Change; by Thomas Rawson Birks, M.A. Cambridge (England), 1862.

that its volume can not be increased, the amount of heat required to produce the change of temperature is called 'the specific heat under a constant volume.'"—*Silliman's Physics*, p. 459.

The specific heat of air under constant pressure (that of water being unity) has been found to be 0·2377; specific heat of air under constant volume has been found to be 0·1678; difference 0·0699.

"Comparing these results in the case of air, we see that, when air is heated in a situation where it is free to expand, only about $\frac{5}{7}$ of the heat applied is expended in producing elevation of temperature—as in heating a room—while about $\frac{2}{7}$ of the heat is expended in producing expansion of the air, to be given out again as the room cools."—*Silliman's Physics*, p. 451.

Again: "It is a perfectly well known fact that a certain amount of heat is rendered latent in producing the expansion of a given mass of gas, and that, on condensing the gas to its original volume, the same amount of heat is set free."—*Cooke's Chemical Physics*, p. 480.

The absorption of a certain amount of heat, and the rendering of it latent, appears to be admitted as a necessary accompaniment of the act of expansion, as such, and essential to its accomplishment—whether the expansion be produced by the removal of pressure,[2] or by the application of heat, or by both combined. The amount absorbed must therefore depend solely upon the extent of the expansion—and air of any given density must always contain the same amount of latent heat, no matter what may be its past history or its present condition as to temperature or pressure.

Now it has been ascertained by Regnault and others, that when air is heated in a situation where it is free to expand without change of pressure, equal additions of heat make equal additions of volume—and that this holds good at all temperatures and at all pressures—also that the rate of expansion is such that air at the freezing point expands $\frac{1}{491}$ part of its bulk for every added degree of heat on Fahrenheit's scale. That is

491 cubic inches of air at 32° become
492 " " " 33 "
493 " " " 34 " &c.

[2] A striking instance of the effect of the removal of pressure is afforded on a vast scale at the fountain of Hiero, at the mines of Chemnitz in Hungary. "A part of the machinery for working these mines is a perpendicular column of water 260 feet high, which presses on a quantity of air enclosed in a tight reservoir. The air is consequently condensed to an enormous degree by this height of water, which is equal to 8 or 9 atmospheres; and when a pipe communicating with the reservoir of condensed air is suddenly opened, it rushes out with extreme velocity, instantly expands, and in so doing absorbs so much caloric as to precipitate the moisture it contains in a shower of very white compact snow, or rather hail, which may be readily gathered in a hat held in the blast."—*Silliman's Chemistry*, 1830, vol. i, p. 121.

Whence it appears that

1 vol. of air at 32°, by having its temperature raised 491° becomes 2 vols.
1 " " " " " " " 982 " 3 "
1 " " " " " " " 1473 " 4 "

&c.,—the increase being 1 volume for each 491°.

But it has already been shown that, of the heat employed in this process, about $\frac{2}{7}$ (more exactly $\frac{699}{2377}$) is absorbed by the air and rendered latent. Hence, of each 491° expended as above, $\frac{699}{2377}$, or about 144°, are rendered latent. It therefore follows that

Vol. of air.				Latent heat.
1	at 32° by having its temp. raised 491°	becomes 2 vols.	and contains	144°
1	" " " " " 982	" 3 "	"	288
1	" " " " " 1473	" 4 "	"	432

and so on indefinitely.

Now, inasmuch as it is known that at the height of 3·43 miles the volume of a given weight of air is twice what it is at the earth's surface, and that as we ascend the number of volumes is doubled for each addition of 3·43 miles to the height, the above considerations enable us to calculate the amount of latent heat in any given weight or bulk of air at any given height within the limits of the atmosphere.

Height in miles. — n being = number terms of this series. a being =3·43. — na.	Number of volumes corresponding to 1 volume at the surface of the earth. — 2^n.	Number of degrees of latent heat. — $144\,(2^n-1)$.	Latent heat in 1 volume to nearest degree. — $144\left(\frac{2^n-1}{2^n}\right)$	Number of grains of air in cylinder 1 mile long and 1 foot in diameter. — Weight at surface of the earth =2342847 grains =334·69 pounds avoirdupois.
3·43	2	144	72	1171424
6·86	4	432	108	585712
10·29	8	1008	126	292856
13·72	16	2160	135	146428
17·15	32	4464	139	73214
20·58	64	9072	142	36607
24·01	128	18288	143	18303
27·44	256	36720	143	9152
30·87	512	73584	144	4576
34·30	1024	147312	144	2288
37·73	2048	294768	144	1144
41·16	4096	589680	144	572
44·59	8192	1179504	144	286
48·02	16384	2359152	144	143
51·45	32768	4718448	144	72
54·88	65536	9437040	144	36
58·31	131072	18874224	144	18
61·74	262144	37748592	144	9
65·17	524288	75497328	144	4
68·60	1048576	150994800	144	2
102·90	1073741824	154618822512	144	$\frac{1}{512}$
137.20	1099511627776	158329674399600	144	$\frac{1}{524288}$
171·50	1125899906842624	162129586585337712	144	$\frac{1}{536870912}$
205·80	1152921504606846976	166020696663385964400	144	$\frac{1}{549755813888}$

The above table shows results thus obtained, together with some other facts bearing upon the subject.

The most important as well as the most striking fact shown by this table, is that the quantity of latent heat in a given bulk of air is sensibly constant for all heights exceeding 30 miles. Below that point it decreases rapidly as we descend, being at 3½ miles only one-half of what it is at 30 miles.

For convenience of illustration, let us assume as our unit of measure a cylinder 1 mile long and 1 foot in diameter—this being the space traversed by a globular meteor 1 foot in diameter in going 1 mile. Such a cylinder will contain, at the surface of the earth, 335 pounds (2342847 grains) of air.

At the height of 3·43 miles it will contain 167 pounds, which, when condensed to the density of air at the surface, will evolve enough heat to raise the temperature of the original weight—say 335 pounds—72°.

At 34·30 miles it will contain ⅛ pound of air, which, condensed as before, will evolve heat sufficient to raise 335 pounds 144°.

At 68·60 miles—the weight of air is only 2 grains but its condensation will raise 335 pounds 144°—and *generally, the same bulk of air is capable of effecting the same result at any greater height, even to the extreme limits of the atmosphere.*

Now let us suppose a meteoric stone one foot in diameter (weighing say two hundred pounds) to enter the atmosphere with a velocity of ten miles per second.

In every mile that it travels, it meets with and condenses before it a bulk of air equal to our assumed unit of measure, which, compressed to the density of air at the surface of the earth, will give out heat enough to raise 335 pounds of air 144°. In one second it passes through ten units, and the heat evolved will raise 335 pounds 1440°, or the weight of the stone—two hundred pounds—2412°, being more than sufficient to bring the whole mass to an incandescent state.

But this heat is developed, *not* in the stone weighing 200 lbs., but in a body of air, the total weight of which is at most only a few grains. The intensity of the heat in this small mass must therefore be proportionally greater. The table shows that at the height of only 55 miles the heat is sufficient to raise the temperature of the whole mass of air encountered, more than nine million degrees; and at greater heights the intensity will increase in a geometric ratio, so that at 137 miles only it becomes one hundred and fifty-eight millions of millions.

It thus appears that we have the means of accounting for a brilliancy of any imaginable intensity—*the greatest splendor being, not in the meteor, but in the air which surrounds it.*

Those particles of air which are in *immediate contact* with the stone (and those only) will of course part with a considerable portion of their heat, which, as time is not afforded for it to penetrate the mass, must be expended in burning off or vaporizing the surface layer of the stone. The greatest elevation of temperature must evidently take place in the remaining portions of the air, which, retaining nearly the whole of the heat developed in them, will reach a state of the most brilliant incandescence, the splendor of which will be vastly increased by the presence of the stony particles thrown off from the meteor.

For convenience I have assumed that the air is in all cases compressed to the density of that at the surface of the earth. Whilst this is doubtless sufficiently correct for the purpose of illustrating the nature of the phenomena, it can of course lay no claim to accuracy. The density attained must vary with the height, velocity, &c. Again, the sudden elevation of temperature must generate an enormous elasticity which will tend to drive the incandescent particles outward from the axis of the meteor's path and thus limit the degree of condensation.

In this way the most intensely brilliant particles must be directly in front of the meteor, streaming outward in all directions. As these are left behind by the meteor in its flight they must form a cylinder of fire; but the expansion which immediately ensues, promptly cools it off, and as this cooling process must begin at the surface of the cylinder and can only reach the axis at some distance in the rear of the meteor, it will evidently convert the cylinder into a luminous cone moving base-foremost —far the greatest brilliancy being in the base itself. This is precisely the form actually observed in the great daylight meteors of 1859 and 1860 and in some others of the same class.

The conclusion to which these considerations lead, is that the upper regions of the atmosphere, even to its utmost limit, are grand reservoirs of latent heat[3] most admirably adapted for the

[3] Sir John Herschell in his "*Outlines of Astronomy*," p. 617, says, "The heat which they possess when fallen, the igneous phenomena which accompany them, their explosion on arriving within the denser regions of our atmosphere, &c., are all sufficiently accounted for on physical principles, by the condensation of the air before them in consequence of their enormous velocity, and by the relations of air in a highly attenuated state to heat," and he refers to the *Edinburgh Review*, Jan., 1848, p. 195. The passage in the Review is as follows:

"Arriving with planetary velocity at the confines of our atmosphere, where the air is many thousand, perhaps million times rarer than at the surface of the earth, such a body would carry before it the air on which it immediately impinged, compressing it to an enormous *relative* extent against its own surface, before the *absolute* compression could reach such a point as to determine its lateral escape. Now it has been shown by Poisson (*Ann. de Chim.*, xxiii, 341) that the latent heat of a given weight of air is greater, the lower the pressure under which it exists. A given quantity (by weight) of air, therefore, at those elevations contains more latent heat than the same quantity at the earth's surface. When condensed, therefore, it will give out *more* heat than would be elicited by the same extent of *relative* condensation from air of ordinary density, which we know to be capable of producing ignition, even under very moderate degrees of sudden compression. A source of sudden and transient heat of almost any conceivable intensity, is thus provided in immediate contact with the surface of the stone, which it would fuse and partially protection of the earth from collision with bodies approaching it with planetary velocity from without. The intruder is instantly surrounded with a fiery envelope heated to the greatest conceivable intensity, its surface is burned off or dissipated into vapor, the sudden expansion of the stratum immediately beneath the burning surface tears the body into fragments, each of which, retaining its planetary velocity, is instantly surrounded by a similar envelope, which produces like effects; and so on, until in most cases the whole is burnt up or vaporized.

Of the vast number of meteors seen, and which may fairly be presumed to embrace great variety of material, but very few are known to reach the earth, and these few are invariably found to be composed of the most incombustible substances—flinty stones or masses of iron. Such bodies may penetrate the whole depth of the atmosphere with only a partial loss of substance, whilst those of a more combustible nature may be totally destroyed during the flight of a few miles.

vaporize, while the sudden and violent expansion of the parts immediately beneath the fused film must necessarily cause decrepitation and disruption of fragments. In short, there is no part of the phenomenon which this explanation does not reach. Mere friction against the atmosphere, as suggested by Poisson, seems quite insufficient to produce incandescence."

Although no numerical results are here given, it might be supposed that this article anticipates those given above, but such does not appear to be the fact.

The conclusion to which the mathematical investigations of Poisson led him are thus announced (*Ann. de Chim.*, 1825, xxiii, 341). "Whence we conclude in general,

$$c = (0{\cdot}2669)\left(\frac{P}{p}\right)^{1-\frac{1}{k}}$$

and the value of c' can be deduced from that of c by dividing it by k. As this quantity k is greater than unity, we see that the *specific* heat of a gramme of air, and generally of any gas whatever, will increase when the elastic force, p, becomes less."

In the above, c represents the specific heat under constant pressure.
c' " " " volume.
p " the pressure.
P " " with barometer at 29·92 in.

and $k = \frac{c}{c'}$ and assumed $=1{\cdot}3750$.

Poisson treats only of "specific heat," and makes no mention of "latent heat" in any part of the article ('Sur la Chaleur des gaz et des vapeurs.')

But even overlooking this misquotation, and assuming, as the writer seems to have done, that, for the object in view, "specific heat" and "latent heat" might be treated as convertible terms, the ratio of increase with the increase of height is altogether too trifling to serve as the basis of any explanation of the phenomena in question. At the height of 41 miles, the specific heat, according to this formula, would be only ten times that at the surface of the earth; whereas the latent heat at that height as above stated is 589680 degrees.

But, furthermore, Poisson's result was a mere theoretical deduction, which has been proved to be altogether erroneous by the experiments of Regnault, who has shown that the *specific heat is the same for all pressures;* so that the explanation as it stands in the Review appears to be entirely without foundation.

If, as above maintained, the observed splendor is not due to the temperature of the meteoric bodies themselves, but to that of mere envelopes of air, brought to the most intense degree of incandescence by the development of their latent heat, it is evident, that, inasmuch as this heat is nearly constant for all considerable heights, the most splendid results must be developed in the rarest portions of the atmosphere, because there the mass of the air to be acted upon by this fixed amount of heat is least; and that as we descend, a point may be reached where the mass is so great that the intensity will fall short of that required to produce incandescence, and all luminosity must instantly cease. The meteor will then have "entered a medium which has not the elements necessary to its continued brilliancy."

The table shows that at the height of $10\frac{1}{4}$ miles, with the assumed degree of condensation, the intensity will not exceed one thousand degrees, even without making any allowance for the portion of heat which must always be absorbed by the meteor itself. Luminosity must therefore cease above this limit, and the meteor must perform the remainder of its journey to the earth as a dark body, unless the velocity be such as to produce a much greater condensation. The daylight meteor of November 15, 1859, owing to its amazing velocity, passed this limit, disappearing at the height of only six or eight miles without any perceptible diminution of velocity, but this is believed to be a rare instance.

Whilst the luminous track of those meteors which have their paths directed downward is always cut short before reaching the earth, there are instances of very extended flights, in meteors moving more nearly horizontally. That of July 20th, 1860, was seen to traverse the atmosphere more than a thousand miles, and finally disappeared in the distance over the Atlantic apparently without having become extinct. But this meteor had at no time an elevation less than forty miles, and therefore did not leave the medium which favored its continued brilliancy.

The preceding views may be thought to imply that all meteors should be seen the instant they enter the atmosphere, and consequently at a uniform elevation. But it must be remembered that the extremely cold surface of the meteor (having the temperature of interplanetary space, say 100° below zero) must first be heated; and the distance passed through before this is accomplished must depend upon the size and conducting power, but more especially upon the velocity of the body, and the results may therefore differ widely.

A meteoric body of great conducting power and very moderate velocity might upon first entering the atmosphere absorb so large a portion of the whole heat developed, as to prevent the development of any luminosity until a very considerable depth of air had been traversed. On the other hand, we have in the great daylight meteor of 1859 an example of the effects of the most extreme velocity—probably between fifty and a hundred miles per second. This body became visible at a probable height of near two hundred miles, and exhibited a brilliancy almost if not quite equal to that of the sun, being a conspicuous object to persons who were more than two hundred miles from the nearest point in its path, and maintained its luminosity until within a few miles of the earth.

Philadelphia, May 23, 1863.

ART. XIV.—*Proceedings of Learned Societies.—Foreign.*

I. ROYAL INSTITUTION OF GREAT BRITAIN.—Friday, Jan. 23, 1863.

1. *On Radiation through the Earth's Atmosphere;* by JOHN TYNDALL, Esq., F.R.S., Professor of Nat. Phil., Roy. Inst.—Nobody ever obtained the idea of a line from Euclid's definition that it is length without breadth. The idea is obtained from a real physical line drawn by a pen or pencil, and therefore possessing width; the idea being afterwards brought, by a process of abstraction, more nearly into accordance with the conditions of the definition. So also with regard to physical phenomena; we must help ourselves to a conception of the invisible by means of proper images derived from the visible, afterwards purifying our conceptions to the needful extent. Definiteness of conceptions, even though at some expense to delicacy, is of the greatest utility in dealing with physical phenomena. Indeed it may be questioned whether a mind trained in physical research can at all enjoy peace, without having made clear to itself some possible way of conceiving of those operations which lie beyond the boundaries of sense, and in which sensible phenomena originate.

When we speak of radiation through the atmosphere, we ought to be able to affix definite physical ideas, to both the term atmosphere and the term radiation. It is well known that our atmosphere is mainly composed of the two elements, oxygen and nitrogen. These elementary atoms may be figured as small spheres scattered thickly in the space which immediately surrounds the earth. They constitute about 99½ per cent of the atmosphere. Mixed with these atoms we have others of a totally different character; we have the molecules, or atomic groups, of carbonic acid, of ammonia, and of aqueous vapor. In these substances diverse atoms have coalesced to form little systems of atoms. The molecule of aqueous vapor, for example, consists of two atoms of hydrogen united to one of oxygen; and they mingle as little triads among the monads of oxygen and nitrogen, which constitute the great mass of the atmosphere.

These atoms and molecules are separate; but in what sense? They are separate from each other in the sense in which the individual fishes of a shoal are separate. The shoal of fish is embraced by a common medium, which connects the different members of the shoal, and renders intercommunication between them possible. A medium also embraces our atoms; within our atmosphere exists a second, and a finer, atmosphere, in which the atoms of oxygen and nitrogen hang like suspended grains. This finer atmosphere unites not only atom with atom, but star with star; and the light of all suns, and of all stars, is in reality a kind of music propagated through this interstellar air. This image must be clearly seized, and then we have to advance a step. We must not only figure our atoms suspended in this medium, but we must figure them vibrating in it. In this motion of the atoms consists what we call their heat. "What is heat in us," as Locke has perfectly expressed it, "is in the body heated nothing but motion." Well, we must figure this motion communicated to the medium in which the atoms swing, and sent in ripples through it with inconceivable velocity to the bounds of space. Motion in this form, unconnected with ordinary matter, but speeding through the interstellar medium, receives the name of Radiant Heat; and, if competent to excite the nerves of vision, we call it Light.

Aqueous vapor was defined to be an invisible gas. Vapor was permitted to issue horizontally with considerable force from a tube connected with a small boiler. The track of the cloud of condensed steam was vividly illuminated by the electric light. What was seen, however, was not vapor, but vapor condensed to water. Beyond the visible end of the jet the cloud resolved itself into true vapor. A lamp was placed under the jet at various points; the cloud was cut sharply off at that point, and when the flame was placed near the efflux orifice the cloud entirely disappeared. The heat of the lamp completely prevented precipitation. This same vapor was condensed and congealed on the surface of a vessel containing a freezing mixture, from which it was scraped in quantities sufficient to form a small snowball. The beam of the electric lamp, moreover, was sent through a large receiver placed on an air-pump. A single stroke of the pump caused the precipitation of the aqueous vapor within, which became beautifully illuminated by the beam; while, upon a screen behind, a richly-colored halo, due to diffraction by the little cloud within the receiver, flashed forth.

The waves of heat speed from our earth through our atmosphere towards space. These waves dash in their passage against the atoms of oxygen and nitrogen, and against the molecules of aqueous vapor. Thinly scattered as these latter are, we might naturally think meanly of them as barriers to the waves of heat. We might imagine that the wide spaces between the vapor molecules would be an open door for the passage of the undulations; and that if those waves were at all intercepted, it would be by the substances which form 99½ per cent of the whole atmosphere. Three or four years ago, however, it was found by the speaker that this small modicum of aqueous vapor intercepted fifteen times the quantity of heat stopped by the whole of the air in which it was diffused. It was afterwards found that the dry air then experimented with was not perfectly pure, and that the purer the air became the more it approached the character of a vacuum, and the greater, by comparison, became the action of the aqueous vapor. The vapor was found to act with 30, 40, 50, 60, 70 times the energy of the air in which it was diffused; and no doubt was entertained that the aqueous vapor of the air which filled the Royal Institution theatre, during the delivery of the discourse, absorbed 90 or 100 times the quantity of radiant heat which was absorbed by the main body of the air of the room.

Looking at the single atoms, for every 200 of oxygen and nitrogen there is about 1 of aqueous vapor. This 1, then, is 80 times more powerful than the 200; and hence, comparing a single atom of oxygen or nitrogen with a single atom of aqueous vapor, we may infer that the action of the latter is 16,000 times that of the former. This was a very astonishing result, and it naturally excited opposition, based on the philosophic reluctance to accept a result so grave in consequences before testing it to the uttermost. From such opposition, a discovery, if it be worth the name, emerges with its fibre strengthened; as the human character gathers force from the healthy antagonisms of active life. It was urged, that the result was on the face of it improbable; that there were, moreover, many ways of accounting for it, without ascribing so enormous a comparative action to aqueous vapor. For example, the cylinder which contained the air, in which these experiments were made, was stopped at its ends by plates of rocksalt, on account of their transparency to radiant heat. Rocksalt is hygroscopic; it attracts the moisture of the atmosphere. Thus, a layer of brine readily forms on the surface of a plate of rocksalt; and it is well known that brine is very impervous to the rays of heat. Illuminating a polished plate of salt by the electric lamp, and casting, by means of a lens, a magnified image of the plate upon a screen, the speaker breathed through a tube for a moment on the salt; brilliant colors of thin plates (soap-bubble colors) flashed forth immediately upon the screen—these being caused by the film of moisture which overspread the salt. Such a film, it was contended, is formed when undried air is sent into the cylinder; it was, therefore, the absorption of a layer of brine which was measured, instead of the absoption of aqueous vapor.

This objection was met in two ways. First, by showing that the plates of salt when subjected to the strictest examination show no trace of a film of moisture. Secondly, by abolishing the plates of salt altogether, and obtaining the same results in a cylinder open at both ends.

It was next surmised, that the effect was due to the impurity of the London air, and the suspended carbon particles were pointed to as the cause of the opacity to radiant heat. This objection was met by bringing air from Hyde Park, Hampstead Heath, Primrose Hill, Epsom Downs, a field near Newport in the Isle of Wight, St. Catharine's Down, and the sea-beach near Black Gang Chine. The aqueous vapor of the air from these localities intercepted at least seventy times the amount of radiant heat absorbed by the air in which the vapor was diffused. Experiments made with smoky air proved that the suspended smoke of the atmosphere of West London, even when an east wind pours over it the smoke of the city, exerts only a fraction of the destructive powers exercised by the transparent and impalpable aqueous vapor diffused in the air.

The cylinder which contained the air through which the calorific rays passed was polished within, and the rays which struck the interior surface were reflected from it to the thermo-electric pile which measured the radiation. The following objection was raised:—You permit moist air to enter your cylinder; a portion of this moisture is condensed as a liquid film upon the interior surface of your tube; its reflective power is thereby diminished; less heat therefore reaches the pile, and you incorrectly ascribe to the absorption of aqueous vapor an effect which is really due to diminished reflection of the interior surface of your cylinder.

But why should the aqueous vapor so condense? The tube within is warmer than the air without, and against its inner surface the rays of heat are impinging. There can be no tendency to condensation under such circumstances. Further, let five inches of undried air be sent into the tube—that is, one-sixth of the amount which it can contain. These five inches produce their proportionate absorption. The driest day, on the driest portion of the earth's surface, would make no approach to the dryness of our cylinder when it contains only five inches of air. Make it 10, 15, 20, 25, 30 inches: you obtain an absorption exactly proportional to the quantity of vapor present. It is next to a physical impossibility that this could be the case if the effect were due to condensation. But lest a doubt should linger in the mind, not only were the plates of rocksalt abolished, but the cylinder itself was dispensed with. Humid air was displaced by dry, and dry air by humid in the free atmosphere; the absorption of the aqueous vapor was here manifest, as in all the other cases.

No doubt, therefore, can exist of the extraordinary opacity of this substance to the rays of obscure heat; and particularly such rays as are emitted by the earth after it has been warmed by the sun. It is perfectly certain that more than ten per cent of the terrestrial radiation from the soil of England is stopped within ten feet of the surface of the soil. This one fact is sufficient to show the immense influence which this newly-discovered property of aqueous vapors must exert on the phenomena of meteorology.

This aqueous vapor is a blanket more necessary to the vegetable life of England than clothing is to man. Remove for a single summer-night the aqueous vapor from the air which overspreads this country, and you would assuredly destroy every plant capable of being destroyed by a freezing temperature. The warmth of our fields and gardens would pour itself unrequited into space, and the sun would rise upon an island held fast in the iron grip of frost. The aqueous vapor constitutes a local dam, by which the temperature at the earth's surface is deepened: the dam, however, finally overflows, and we give to space all that we receive from the sun.

The sun raises the vapors of the equatorial ocean; they rise, but for a time a vapor screen spreads above and around them. But the higher they rise, the more they come into the presence of pure space, and when, by their levity, they have penetrated the vapor screen, which lies close to the earth's surface, what must occur?

It has been said that, compared atom for atom, the absorption of an atom of aqueous vapor is 16,000 times that of air. Now the power to absorb and the power to radiate are perfectly reciprocal and proportional. The atom of aqueous vapor will therefore radiate with 16,000 times the energy of an atom of air. Imagine then this powerful radiant in the presence of space, and with no screen above it to check its radiation. Into space it pours its heat, chills itself, condenses, and the tropical torrents are the consequence. The expansion of the air, no doubt, also refrigerates it; but in accounting for those deluges, the chilling of the vapor by its own radiation must play a most important part. The rain quits the ocean as vapor; it returns to it as water. How are the vast stores of heat set free by the change from the vaporous to the liquid condition disposed of? Doubtless in great part they are wasted by radiation into space. Similar remarks apply to the cumuli of our latitudes. The warmed air, charged with vapor, rises in columns, so as to penetrate the vapor screen which hugs the earth; in the presence of space, the head of each pillar wastes it heat by radiation, condenses to a cumulus, which constitutes the visible capital of an invisible column of saturated air.

Numberless other meteorological phenomena receive their solution, by reference to the radiant and absorbent properties of aqueous vapor. It is the absence of this screen, and the consequent copious waste of heat, that causes mountains to be so much chilled when the sun is withdrawn. Its absence in Central Asia renders the winter there almost unendurable; in Sahara the dryness of the air is sometimes such, that though during the day "the soil is fire and the wind is flame," the chill at night is painful to bear. In Australia, also, the thermometric range is enormous, on account of the absence of this qualifying agent. A clear day, and a dry day, moreover, are very different things. The atmosphere may possess great visual clearness, while it is charged with aqueous vapor, and on such occasions great chilling cannot occur by terrestrial radiation. Sir John Leslie and others have been perplexed by the varying indications of their instruments on days equally bright—but all these anomalies are completely accounted for by reference to this newly-discovered property of transparent aqueous vapor. Its presence would check the earth's loss; its absence, without sensibly altering the transparency of the air, would open wide a door for the escape of the earth's heat into infinitude.

II. Proceedings of the Royal Society, Vol. xii, No. 51.

2. *On the Photographic Transparency of various Bodies, and on the Photographic Effects of Metallic and other Spectra obtained by means of the Electric Spark;* by Prof. W. Allen Miller.—In this paper the author pursues an inquiry the commencement of which was communicated to the Chemical Section of the British Association last year. Owing to the employment of a prism of bisulphid of carbon, he was then led to believe that the photographic effects of the electric spectra produced by the different metals were in a great degree similar, if not identical. Subsequent investigations have, however, shown him that the absorbent effects of the bisulphid upon the chemical rays are so great, that the conclusions then drawn from observations made by this refracting medium require very considerable modification. Notwithstanding the great length of the chemical spectra obtained by the aid of the bisulphid, not more than *one-sixth* or *one-seventh* of the true extent of the spectrum produced by the electric spark between various metals is procured, as may be shown

by comparing the spectrum with one of the same metal furnished by the use of a lens and prism of rock-crystal.

Rock-crystal, however, possesses but a comparatively small refractive and dispersive power, whilst it almost always affords some trace of double refraction in one portion or other of the spectrum procured by its means.

In searching for some singly refracting medium which should possess sufficient refractive and dispersive power to enable it to be used advantageously in the construction of lenses and prisms suitable for this inquiry, the author was led to examine the photographic absorption of a variety of colorless substances which appeared perfectly transparent to the luminous rays. The experiments detailed in the first portion of the present paper refer to this absorbent action of various media upon the chemical rays of the spectrum; whilst the second portion of the paper is devoted to a description of the electric spectra of some of the more important elementary bodies, and the effect of varying the gaseous media in which the sparks producing these spectra are made to originate.

(1.) *The Photographic Transparency of Bodies.*—In the experiments upon the absorbent action of the different media, the source of light employed was the electric spark obtained between two metallic wires (generally of fine silver), connected with the terminals of the secondary wires of a ten-inch induction-coil. The light, after passing through a narrow vertical slit, either before or after traversing a stratum of the material the chemical transparency or *diactinic* quality of which was to be tested, was allowed to fall upon a quartz prism placed at the angle of minimum deviation for the mean of the refracted rays. Immediately behind this was a lens of rock-crystal, and behind this, at a suitable distance, the spectrum was received upon a collodion-film coated with iodid of silver; this supported in the frame of a camera, and after an exposure, generally lasting for five minutes, the image was developed by means of pyrogallic acid, and fixed with cyanid of potassium.

The general results of these experiments were as follows:—

1. Colorless bodies, which are equally transparent to the visible rays, vary greatly in permeability to the chemical rays.

2. Bodies which are photographically transparent, in the solid form, preserve their transparency in the liquid and in the gaseous states.

3. Colorless transparent solids, which exert a considerable photographic absorption, preserve their absorptive action with greater or less intensity both in the liquid and the gaseous states.

Whether the compound is liquefied by heat or dissolved in water, these conclusions respecting liquids are equally true. The perfect permeability of water to the chemical rays, conjoined with the circumstance that in no instance does the process of solution seem to interfere with the special action upon the incident rays of the substance dissolved, renders it practicable to submit to this test a great number of bodies which it would otherwise be impossible to subject to this species of experiment on account of the extreme difficulty of obtaining them in crystals of sufficient size and limpidity.

Glass vessels cannot be employed to contain the liquids during the trial. Flint-glass, crown, hard white Bohemian, plate-glass, window-sheet, and Faraday's optical glass, all, even in thin layers, shorten the spectrum by from three-fifths to four-fifths or even more of its length. Mica produces a similar effect. Indeed, the only substance which the author found could be employed with advantage is rock-crystal cut into thin slices and polished. The value of this material in researches upon the more refrangible end of the spectrum was pointed out by Prof. Stokes and Ed. Becquerel several years ago. In order to hold the liquids for experiment, a small trough was prepared by cutting a notch in a thick plate of plate-glass, the sides being completed by means of thin plates of quartz, which were pressed against the ground surfaces of the plate-glass by the aid of elastic bands of caoutchouc; a stratum of liquid of 0·75 inch in depth was thus obtained for each experiment.

The substances which, after atmospheric air and certain other gases, are most perfectly diactinic, are rock-crystal, ice, as well as pure water, and white fluor-spar. Rock-salt is scarcely inferior to them, if at all. Then follow various sulphates, including those of baryta, and the hydrated sulphates of lime and magnesia, as well as those of the alkalies. The carbonates of the alkalies and alkaline earths, as also the phosphates, arseniates, and borates, are likewise tolerably transparent, though saturated solutions of phosphoric and arsenic acids exerted considerable absorbent power; so also did those of the alkalies, potash, and soda, possibly from the presence of a trace of some foreign coloring matter, as those liquids had an extremely faint greenish tinge.

The soluble fluorids, as well as the chlorids and bromids of the metals of the alkalies and alkaline earths, are freely diactinic, but the iodids are much less so, and exhibit certain peculiarities. All the organic acids and their salts which were tried by the author exerted a marked absorbent action upon the more refrangible rays. Amongst those subjected to experiment were the oxalates, tartrates, acetates, and citrates, those mentioned first in order having the greatest absorbent action. It is, however, much more difficult to obtain organic compounds in a state of purity sufficient to furnish trustworthy results, than is the case with the salts of the inorganic acids. The author, therefore, expresses himself with more reserve upon some of these organic bodies, particularly the acetates, than in other cases. The different varieties of sugar are freely diactinic.

Amongst the salts of inorganic acids, the nitrates are the most remarkable for their power of arresting the chemical rays. A solution of each of these salts, in all the instances tried, cut off all the more refrangible rays, and reduced the spectrum to less than a sixth of its ordinary length. The chlorates, however, do not participate in this absorptive power to nearly the same extent.

Although the sulphates, as a class, are largely diactinic, the sulphites are much less so; and the hyposulphites cut off about three-fourths of the length of the spectrum, leaving only the less refrangible portion.

Of eighteen different liquids tried by the author, two only can be regarded as tolerably diactinic, viz: water, which is eminently so, and absolute alcohol, which, however, exhibits a considerable falling off. The liquids which follow are mentioned in the order of their chemical transparency, those most transparent being mentioned first:—Dutch liquid, chloroform, ether; then benzole and distilled glycerin, which differ but little; then fusel oil, wood-spirit, and oxalic ether, which are also nearly alike; acetic acid, oil of turpentine, glycol, carbolic acid, liquid paraffin, boiling at 360° F., and bisulphid of carbon. Finally, terchlorid and oxychlorid of phosphorus, although perfectly colorless and limpid, arrest all the chemical rays.

The experiments upon aeriform bodies yielded important results; they show but little coincidence with those of Tyndall on the absorptive power of the gases for radiant heat. These experiments were made by interposing in the track of the ray between the vertical slit and the quartz prism, a brass tube two feet long, closed at each end air-tight by means of a plate of quartz. Each gas or vapor in succession was introduced into the tube, and the results compared with those produced by causing the rays to traverse the tube when filled with atmospheric air.

Amongst the colorless gases, oxygen, hydrogen, nitrogen, carbonic acid, and carbonic oxyd exhibit no absorptive power.

Olefiant gas, protoxyd of nitrogen, cyanogen, and chlorhydric acid exert a slight but perceptible absorbent effect. But in the case of coal-gas the absorptive action is extremely marked, the more refrangible half of the spectrum being cut off by it abruptly. The absorption exerted by sulphurous acid is still more powerful and as sharply defined; sulphuretted hydrogen and the vapor of bisulphid of carbon exhibit a still more decided absorbent action; the effect of the terchlorid and oxychlorid of phosphorus is not less marked. This absorbent action of these different compounds of sulphur and phosphorus is very striking.

Coal-gas appears to owe its remarkable power of arresting the chemical rays to the presence of the vapor of benzol and other heavy hydrocarbons; since the vapor of benzol at 65°, diffused to saturation through a column of atmospheric air two feet long, exerts a still more powerful absorbent effect than coal-gas.

On the other hand, the effect of a similar arrangement, in which the vapor of ether, of chloroform, and of oil of turpentine was substituted for that of benzol, gave effects which, though perceptible, were much less marked. An arbitrary scale is laid down, by which a comparative estimate of the absorptive power of each compound, whether solid, liquid, or gaseous, may be effected with tolerable accuracy.

With a view of facilitating the production of a spectrum on a flat field, at a uniform distance at all points from the prism, the author instituted a series of experiments, in which a small metallic speculum was substituted for the lens of rock-crystal; but the loss of chemical power in the reflected rays was so considerable, and this loss occurred so unequally at different points, that the method was abandoned. The results of the photographic action of light reflected at an angle of 45° from the polished surface of several of the principal metals is given. The reflection from gold, although not very intense, was found to be more uniform in quality than that from any other metal that was tried. Burnished lead also gave very good results. The reflection from silver is singularly deficient in some portions of the less refrangible rays, although in most other parts the reflection is tolerably perfect, except for rays of extremely high refrangibility.

3. *The Electric Spectra of the Metals.*—The author proceeds then to detail his experiments upon the spectra obtained by causing the sparks of the secondary current from the induction-coil to pass between electrodes composed of various elementary substances, and he gives photographs of the impressions obtained from collodion negatives of a considerable number of different elementary bodies. The spectra were procured by arranging a quartz-train in the manner already described. Among the elements so examined are the following:—

Platinum,	Arsenic,	Copper,
Palladium,	Tellurium,	Aluminum,
Gold,	Tungsten,	Cadmium,
Silver,	Molybdenum,	Zinc,
Mercury,	Chromium,	Magnesium,
Lead,	Manganese,	Sodium,
Tin,	Iron,	Potassium,
Bismuth,	Cobalt,	{ Graphite, and
Antimony,	Nickel,	{ Gas-coke.

The commencement of each spectrum in its less refrangible portion is similar in nearly all cases; and, as it is this portion only which is transmissible through bisulphid of carbon, this circumstance explains the similarity of all the spectra procured by the author from different metals in his earlier experiments, already laid before the British Association. In the more refrangible parts of the spectrum, great and characteristic differences between the results obtained with the different metals are at once manifest. In some cases, as in those of copper and nickel, the action is greatly prolonged in the more refrangible extremity, whilst the intense and highly characteristic spectrum of magnesium is much shorter.

In many cases, metals which are allied in chemical properties exhibit a certain similarity in their spectra. This occurs, for example, with the magnetic metals, iron, cobalt, and nickel, and with the group embracing bismuth, antimony, and arsenic. The more volatile metals exhibit generally the most strongly marked lines. Cadmium, for instance, gives two intense groups. Zinc, two very strong lines near the less refrangible extremity, three near the middle, and four nearly equidistant lines towards the termination of the more refrangible portion; whilst in the spectrum of magnesium the chemical action is almost suddenly terminated near the middle by a triple group of very broad and strong lines.

It will be observed, on examining the photographs of these spectra of the various metals, that the impressions, particularly in the more refrangible portions, consist of a double row of dots, running parallel with the length of the spectrum, and forming the terminations of lines rather than lines themselves, as though the intense ignition of the detached particles of metal, necessary to furnish rays capable of exciting chemical action, had ceased before the transfer of these particles to the opposite electrode had been completed.

If each electrode be composed of a different metal, the spectrum of each metal is impressed separately upon the plate, as is evident on examining the photographs.

When alloys are employed as electrodes, the spectrum exhibited is that due to both the metals; but if the metals made use of are approximatively pure, the spectrum is hardly to be distinguished from that of the pure metal. In the case when alloys are used as electrodes, it is not always the more volatile metal which impresses its spectrum most strongly. A specimen of brass, for example, containing 38 per cent of zinc, gave a spectrum which could not be distinguished from that of pure copper, though an alloy of three parts of gold and one of silver gave a spectrum in which the lines due to silver predominated.

The author then proceeds to describe a number of experiments upon the transmission of sparks between electrodes of different metals in a current of several different gases. The apparatus employed consisted of a glass tube; into the side an aperture was drilled, which could be closed by a plate of quartz; the ends of the tubes were closed by ground brass plates, each supporting a pair of brass forceps, into which the electrodes were fitted; through the axis of the tube a current of each gas was transmitted at the ordinary atmospheric pressure.

Among the gases thus tried were hydrogen, protoxyd of nitrogen, carbonic acid, carbonic oxyd, olefiant gas, marsh-gas, cyanogen, sulphuretted hydrogen, sulphurous acid, nitrogen, and oxygen. The spectrum obtained from the same metal varied considerably in these different media. In hydrogen, the intensity of the spectrum was greatly reduced, and the more refrangible rays were wanting, but no new rays made their appearance. In carbonic acid, carbonic oxyd, olefiant gas, marsh-gas, and cyanogen, the special lines due to the metal were produced, but in each a series of identical lines appeared, and these new lines were referable to the carbon contained in each of these gases. Each gas exhibits special lines which are continued across the spectrum, and are never interrupted like those of the metals.

The author observed that many of these gases, such as protoxyd of nitrogen, chlorhydric and sulphurous acid, presented a considerable obstacle to the passage of the sparks from the induction-coil.

4. *On the Long Spectrum of Electric Light;* by Professor GEORGE G. STOKES.—The author's researches on fluorescence had led him to perceive that glass was opaque for the more refrangible invisible rays of the solar spectrum, and that electric light contained rays of still higher refrangibility, which were quite intercepted by glass, but that quartz transmitted these rays freely. Accordingly he was led to procure prisms and a lens of quartz, which, when applied to the examination of the voltaic arc, or of the discharge of a Leyden jar, by forming a pure spectrum and receiving it on a highly fluorescent substance, revealed the existence of rays forming a spectrum no less than six or eight times as long as the visible spectrum. This long spectrum, as formed by the voltaic arc with copper electrodes, was exhibited at a lecture given at the Royal Institution in 1853; but the author, for reasons he mentioned, did not then further pursue the subject. * *

* * Among the metals examined, the author had found aluminum the richest in invisible rays of extreme refrangibility; and accordingly aluminum electrodes were employed when the deportment of such rays had to be specially examined. As the bright aluminum lines of high refrangibility do not appear to have been taken by photography, a

drawing of the aluminum spectrum is given, with zinc and cadmium for comparison.

The author has also described and figured the mode of absorption of the invisible rays by solutions of various alkaloids and glucosides. Bodies of these classes, he finds, are usually intensely opaque, acting on the invisible spectrum with an intensity comparable to that with which coloring matters act on the visible. This intensity of action causes the effect of minute impurities to disappear, and thereby increases the value of the characters observed. It very often happens that, at some part or other of the long spectrum, a band of absorption, or maximum of opacity, occurs; and the position of this band affords a highly distinctive character of the substance which produced it.

Among natural crystals, besides the previously known yellow uranite, the author found that in adularia, and feldspar generally, a strong fluorescence is produced under the action of the rays of high refrangibility, referable not to impurities, but to the essential constituents of the crystal. A particular variety of fluor-spar shows also an interesting feature, though in this case referable to an impurity, exhibiting a well-marked reddish fluorescence under the exclusive influence of rays of the very highest refrangibility. This property renders such a crystal a useful instrument of research.

With some metals broad, slightly convex electrodes were found to have a great advantage over wires, exhibiting the invisible lines far more strongly, while with some metals the difference was not great.

The blue negative light formed when the jar is removed, and the electrodes are close together, was found to be exceedingly rich in invisible rays, especially invisible rays of moderate refrangibility. These exhibited lines independent of the electrodes, and therefore referable to the air. This blue light has a very appreciable duration, and is formed by what the author calls an arc discharge.

The paper concludes with some speculations as to the cause of the superiority of broad electrodes, and of the heating of the negative electrode.

5. *On the Reflexion of Polarized Light on Polished Surfaces;* by the Rev. SAMUEL HAUGHTON.—When a plane-polarized beam of light is incident on a polished surface at a certain angle of incidence, and polarized in a certain azimuth, the reflected beam of light is circularly polarized.

The tangent of this angle of incidence is called by the author the Coefficient of Refraction, and upon it appears to depend the *brilliancy* of a polished surface.

The cotangent of the azimuth of incident polarization is called the Coefficient of Reflexion, and upon it appears to depend the rich *lustre*, strikingly exhibited in polished copper and gold.

The paper contains an account of the experiments made to determine, with precision, these constants for the following substances:—

A. *Transparent Bodies.*

1. Munich glass (*a*).
2. Munich glass (*b*).
3. Paris glass.
4. Fluor-spar.
5. Glass of antimony.
6. Quartz crystal.

B. *Pure Metals.*

1. Silver.	7. Zinc.
2. Gold.	8. Lead.
3. Mercury.	9. Bismuth.
4. Platinum.	10. Tin.
5. Palladium.	11. Iron and steel.
6. Copper.	12. Aluminum.

C. *Alloys.*

1. Copper and tin (speculum metal).	9. Copper and zinc (3Cu+Zn).
2. Copper and zinc (10Cu+Zn).	10. " " (2Cu+Zn).
3. " " (9Cu+Zn).	11. " " (Cu+Zn).
4. " " (8Cu+Zn).	12. " " (Cu+2Zn).
5. " " (7Cu+Zn).	13. " " (Cu+3Zn).
6. " " (6Cu+Zn).	14. " " (Cu+4Zn).
7. " " (5Cu+Zn).	15. " " (Cu+5Zn).
8. " " (4Cu+Zn).	

The determination of the optical constants of these substances leads to many interesting conclusions; among which the following may be stated:—

1. That Transparent bodies, as well as metals, possess a coefficient of reflexion, which is sometimes very sensible, although there are bodies in which it is very small.

2. That *Silver* is the only substance which possesses the qualities of *brilliancy* and *lustre*, represented by the coefficients of refraction and reflexion, in a high degree.

3. Of the metals which have high *brilliancy* and little *lustre* may be named *Mercury*, *Palladium*, *Zinc*, and *Iron*.

4. Of the metals which have high *lustre* and little *brilliancy* there are only two, *Gold* and *Copper*.

5. Results of the highest interest appear from an examination of the optical constants of the alloys of copper and zinc, which cannot be given in an abstract.

6. In the details of the several experiments, the author calls attention to several remarkable laws, or indications of laws, which appear to him to require some notice from theorists.

a. When the azimuth of the incident beam is less than the circular limit, the axis major of the reflected ellipse, at the principal incidence, lies in the plane of incidence; but when the azimuth is greater than the circular limit, it is perpendicular to the plane of incidence, and as the incidence varies, the axis major twice approaches to a minimum distance from that plane.

b. There appears to the author to be some indication in the experiments on metals, that the quantity known to theorists as $\left(\frac{J}{I}\right)$ is not a function of the incidence only; a conclusion which, if correct, would require the intervention of a third wave suppressed, or some such theoretical supposition, to account for it.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS.

1. *Gemsbart Electroscope.*—Prof. Kobell made an interesting communication to the mathematico-physical class of the Bavarian Academy of Sciences, in their session of Jan. 10th, 1863, on the electroscopic properties of the so-called "Gemsbart," a name given by the Alpine hunters to the long hairs which grow along the back of the male Chamois in the autumn of the year, and are well known as the trophies with which the Tyrolese hunter decorates his hat. These hairs from a four years' buck reach the length of six inches and more, are very fine and generally terminate in a white point. If several are taken together by the root and stripped through the fingers, they repel each other to a great distance; if held by the points and rubbed towards the roots, a similar but weaker effect is produced. The electricity in the former case is positive, in the latter negative, and the hairs thus become respectively + and — indicators. This property adapts them in an eminent degree to the purposes of an electroscope, being more sensitive than the usual Hauy's apparatus. For use they are fastened with wax to a glass or sealing-wax rod.

If the plane of a crystal, in which electricity has been excited by friction, pressure or heating, repels one of these indicators, the kind of electricity is *eo ipso* determined, and also the fact that the body is an insulator. If *one* of the indicators is attracted, the electricity is either the opposite, (in which case the other indicator is to be applied) or the body is non-electric. If both are attracted, the body is either non-electric, or a good conductor, in which latter case it must be insulated. To determine the poles of a thermo-electric crystal, it suffices to hold it between the thin points of an elastic steel forceps ending in a wooden handle, and then to examine it with the stronger + indicator, which has to be drawn through the fingers from time to time. By this method the poles of small crystals of boracite, thin needles of scolezite, calamine and Brazilian topaz were easily determined, small crystals generally giving a more constant and decisive reaction than larger ones.

Highly electric crystals, as those of tourmaline, often show the poles plainly, even after they have become perfectly cool externally. For their examination it is most convenient to attach the hair in the middle with wax to a Hauy's needle, and at right angles to the same, and then in the aforesaid manner to excite the opposite electricities in the two ends, when the brass needle is immediately set in motion upon the approach of the electric tourmaline, and the poles can be distinctly shown by alternate repulsion and attraction.

The two-fold electricity of such a hair is evidently connected with its structure, for it is smooth from root to point, and feels rough in the opposite direction. This is corroborated by the fact, that, if a hair by frequent use has been made smooth in the latter direction, it changes its negative electric character into the positive. This occurs after about one hundred experiments, when it can no longer be used as an — indicator. Prof. Bischof, who examined the hairs with the microscope, states that they have a highly developed epithelium, while the fibrous cortical substance is subordinate, and is almost entirely wanting towards the lower part, where it is replaced by the epithelium. The cortical fibres, as usual, contain the pigment. They are distinguished by the large proportion of pith, which begins a short distance from the point and very soon constitutes almost the entire thickness of the hair. The pith consists of large polygonal cells. Thus the opposite electrical state of the two ends may possibly be caused by the prevalence of the cortical fibre in the upper and of the pithy substance in the lower part, this pith being filled with air cells.

Prof. Kobell considers a closer study of the electrical properties of crystals, induced by friction, very desirable; he recommends the use of deer-skin stretched over a wooden pestle as a rubber, giving it the preference over woolen and silk on account of the frequent admixture of the latter with other fibre. Employing this rubber, (and in case of thin laminæ, as those of mica, merely the dry fingers,) and then examining the electricity with the "Gemsbart," he has made the following electrical groups of minerals:

I GROUP. *Good insulators*

When rubbed attract the indicator.

1. Div. *Electro-positive insulators*
repel the + indicator.

Calcite, aragonite, fluor, barytes, glauberite, gypsum, anhydrite, apatite, quartz, topaz, emerald, grossular, idocrase, kyanite, orthoclase, albite, tourmaline, axinite, zircon, muscovite (Grafton, N. H.), spinel, alum, rock-salt, etc.

2. Div. *Electro-negative insulators*
repel the − indicator.

Talc, sulphur, orpiment, amber, asphaltum.

II GROUP. *Good conductors.*

Do not attract the indicator, and are coated by metallic copper, when immersed with a zinc holder in a solution of sulph. copper. Graphite, gold, silver, galena, pyrites, mispickel, chalcopyrite, cobaltine, smaltine, magnetite, etc.

III GROUP. *Bad conductors and bad insulator as compared with Group II.*

Do not attract the indicator, or only feebly, and are not coated with copper if treated as in Group II.

Diamond, celestine, almandine, melanite, biotite and phlogopite, ripidolite and clinochlore, pennine, analcime, sphene, stibnite, hematite, franklinite, zinkenite, jamesonite, chromic iron, red copper, pyrolusite, manganite, psilomelane, hausmannite.

To determine the kind of electricity of Groups II and III, the minerals must be insulated; this is readily done by fastening them with wax on the end of a glass rod of sufficient diameter, taking care that the face to be rubbed projects far enough beyond the wax, to prevent the rubber from coming in contact with the latter.

In the examination of small crystals, it is often convenient to mount them on wax, thereby insulating them to determine whether the mineral belongs to Group I, or is near it, it is only necessary, after rubbing it, to touch a part of the crystal with the finger, when if it be a good insulator it will not loose its electricity. W. T. R.

2. *Conductibility and specific heat of Thallium.*—De la Rive has compared the conducting power of thallium with that of mercury by the method of Wheatstone. The density of the metal was found to be 11,853 at 11° C., which agrees well with the determination of Lamy, namely, 11,862 at 0°; the density of the wire is 11,808. The conducting power of silver being taken as 100, that of mercury is 1·63, and that of thallium 8·64, a value which lies between those for lead and tin, and which is much lower than the corresponding values for the alkaline metals. The specific heat of thallium was found by Regnault to be 0·03355, as a mean of two experiments. The product of this number by the equivalent 204, gives 85·55, half of which is 42·77, so that thallium in its thermic relations is associated with the alkaline metals, and the formula of its protoxyd should be T_2O if potash is written K_2O.—*Comptes Rendus*, lv, 887 and lvi, 588. W. G.

II. CHEMISTRY.

1. *On the tungstates, fluo-tungstates and silico-tungstates.*—Marignac has communicated the results of an extended study of the tungstates, a class of salts exhibiting remarkable anomalies in constitution. The author finds the results of Scheibler in regard to the meta-tungstates to be correct. These salts may all be expressed by the general formula $RO, 4WO_3+x\,aq$, and the acid in them is not precipitated by stronger acids. The ordinary tungstates contain the insoluble modification of tungstic acid. They are frequently very complex in constitution, and appear to be in these cases compounds of bi-tungstates and ter-tungstates. One class of these salts was distinguished by Laurent under the name of paratungstates, and possess, according to that chemist, the general formula $5RO, 12WO_3$. Lotz and Scheibler gave them the general formula $3RO, 7WO_3$, but Marignac is disposed to return to the formula of Laurent, considering the compounds, however, as double salts. The author did not succeed in obtaining fluo-tungstates free from oxygen compounds, agreeing in this respect with Berzelius, whose results were expressed by the general formula RO, WO_3+RF, WF_3. According to Marignac, the same two salts combine in other proportions also, but the most remarkable circumstance is the isomorphism of the copper-salt, CuO, WO_3+CuF, WF_3, with the fluo-silicate, fluo stannate and fluotitanate of copper, the last having the formula CuF, TiF_2. This isomorphism becomes intelligible when the formulas are written $Cu_2W_2\Theta^2F_4$, and $Cu_2Ti_2F_6$, so that it must be admitted that fluorine and oxygen may in certain cases replace each other *atom* for *atom*, though not equivalent for equivalent, and further that Berzelius' mode of viewing the constitution of the salt cannot be correct, since it furnishes no explanation of the isomorphism. The silico-tungstates form a new class of salts, and may in general be easily obtained by boiling a solution of an acid tungstate with gelatinous silicic acid: they are easily soluble, and usually crystallize well. They are not decomposed by boiling with chlorhydric acid or by evaporation to dryness with it, but are simply converted into acid silico-tungstates. Marignac considers the salts of silico-tungstic acid to contain 10

eqs. of tungstic to 1 eq. of silicic acid: the neutral salts contain four equivalents of base, but it is perhaps more proper to double the formulas, and give the neutral salts the general formula $8RO, 20WO_3, 2SiO_2$, in order to include the acid and double salts.—*Comptes Rendus*, lv, 888, *Ann. der Chemie und Pharm.*, cxxv, 362. W. G.

2. *On the preparation and properties of metallic rubidium.*—Bunsen has prepared metallic rubidium, by igniting in a proper apparatus the carbonized bitartrate of the oxyd. From 75 grammes of the salt, 5 grammes of metal were obtained in a single mass. Rubidium is very brilliant, like silver, white with a scarcely perceptible tinge of yellow. In the air it oxydizes instantly to bluish-grey suboxyd, and takes fire, after a few minutes, much more easily than potassium. At $-10°$ C. it is still as soft as iron: it melts at 58°·5 C., and below a red heat is converted into a blue vapor with a shade of green. According to Bunsen, the true fusing point of sodium is 95°·6 C., and that of potassium 62°·5 C.; the latter does *not* pass through an intermediate pasty condition in fusing. The density of rubidium is about 1·52. It is considerably more electro-positive than potassium, takes fire upon water and burns with a flame which cannot be distinguished from that of potassium by the eye. Rubidium burns with brilliancy in chlorine and in the vapor of bromine, iodine, sulphur, and arsenic.—*Ann. der Chemie und Pharmacie*, cxxv, 367. W. G.

3. *On the preparation and properties of metallic magnesium.*—H. Sainte-Claire Deville and H. Caron have given a description of the most recent and improved method of preparing magnesium, and of the properties of the metal in a state of absolute purity. Their results are by no means wholly new, but are interesting as being complete in detail. To obtain pure magnesium, the authors in the first place weigh and mix rapidly 600 grammes of pure chlorid of magnesium obtained by Liebig's method, 480 grammes of pure fluorid of calcium, and 230 grammes of metallic sodium carefully cleaned and cut into small pieces. The mixture is projected into a red hot earthen crucible, which is then covered; a weight is placed on the cover to hold it in its place. When the reaction is finished, the mass is stirred with a clean iron rod, and removed from the fire. Pure and dry fluorid of calcium in fine powder is then thrown in, and the stirring repeated. The iron rod gradually collects all the globules of magnesium into a single mass, which collects near the surface of the heavier saline mass. On breaking up this mass, after cooling, about 92 grammes of magnesium may be collected, which is nearly $\frac{3}{4}$ of the theoretical quantity. The scoria from which the magnesium has been separated may be fused a second time and more magnesium obtained, so that sometimes 100 grammes of sodium will give 45 grammes of raw magnesium. The globules of magnesium may be brought into a single mass by fusing together 60 grammes of chlorid of sodium and 75 grammes of chlorid of potassium (Wöhler's flux), breaking up the cooled mass, and fusing with the magnesium. The metal floats at first, but, as the flux cools, the magnesium becomes specifically heavier, sinks to the bottom of the crucible, and forms a single mass. As thus prepared, magnesium contains carbon, silicon and nitruret of magnesium. To obtain the pure metal, the authors distil the raw product in tubes of gas-retort carbon, surrounded by earthenware tubes, and traversed by a current of hydrogen.

The pure metal has a density of 1·75, is very ductile, and gives plates of great lustre and of a slightly bluish or violet tint when burnished. The surface tarnishes in the air, but not more rapidly than zinc, and the oxydation is never very deep. Magnesium fuses at about the fusing point of zinc: at a little higher temperature it burns with an intensely brilliant flame, as already observed by Bunsen. The light of burning magnesium contains all the rays peculiar to the metal, without the inversion observed by Fizeau in the combustion of sodium.—*Annales de Chimie et de Physique*, lxvii, p. 340. W. G.

4. *On the chemical constitution of the American rock oil.*—SCHORLEMMER has examined the oils obtained by the distillation of cannel coal at low temperatures, the mixture of which is known in the United States as kerosene. The oil in question is found to contain a series of homologous hydrocarbons of the general formula CnHn+2 and consisting of the hydrurets of the alcohol radicals. The oil, which boils below 120° C., contains the four hydrurets,

$C_{10}H_{12}$,	hydruret of	amyl,	boiling at	39° C.,
$C_{12}H_{14}$,	"	hexyl,	"	68° C.,
$C_{14}H_{16}$,	"	heptyl,	"	98° C.,
$C_{16}H_{18}$,	"	octyl,	"	119° C.

The author found precisely the same products in the American petroleum or rock oil. The oils are first purified by strong nitric acid, which leaves the greater part unattacked, but removes benzole and toluole. After washing, drying over caustic potash, and distillation with sodium, the four hydrurets already mentioned were obtained as in coal tar. Benzole and toluole are found in larger proportion in cannel coal tar than in petroleum. —*Proc. Manchester Phil. Soc.*, March 11, 1863. W. G.

5. *On some new organic compounds of silicon.*—FRIEDEL and CRAFTS have prepared, in the laboratory of Wurtz, some interesting compounds of silicon with organic radicals. By heating together silicic ether, $\left.\begin{matrix}\overset{iv}{Si_2}\\(C_4H_5)_4\end{matrix}\right\}O_8$, and bichlorid of silicon, Si_2Cl_4, the authors obtained a new compound having the formula $\left.\begin{matrix}\overset{iv}{Si_2}\\3C_4H_5\\Cl\end{matrix}\right\}O_6$. This body boils at about 156° C.; the density of its vapor is 7·05 by experiment and 6·87 by theory. It may be regarded as silicic ether in which one equivalent of the body, $C_4H_5O_2$, is replaced by one equivalent of chlorine. A second product is found at the same time, the formula of which is $\left.\begin{matrix}\overset{iv}{Si_2}\\2(C_4H_5)\\Cl_2\end{matrix}\right\}O_4$ and which is therefore the dichlorhydrine of silicic ether. Equal equivalents of the mono-chlorhydrine and the amylic alcohol give a new ether, the formula of which is $\left.\begin{matrix}\overset{iv}{Si_2}\\3(C_4H_5)\\C_{10}H_{11}\end{matrix}\right\}O_8$, that is, normal silicate of ethyl,

in which one atom of ethyl is replaced by one atom of amyl. When chlorid of silicon and zinc-ethyl are heated together in a sealed tube, a limpid liquid is obtained, which is insoluble in water, and not acted upon by a concentrated solution of potash or by nitric acid. This liquid is silicon-ethyl, the formula of which is $\left.\begin{matrix}\overset{\text{iv}}{\text{Si}}\\ 4(C_4H_5)\end{matrix}\right\}$. The density of the vapor of silicon ethyl is 5·13 by observation and 4 99 by theory: it corresponds in constitution to distannethyl, $Sn_2(C_4H_5)_4$, and diplumbethyl, $Pb_2(C_4H_5)_4$. The authors promise a more extended study of this body, which, as the first organic compound consisting only of silicon, carbon, and hydrogen, is of much interest. The formula SiO_2 or Si_2O_4 for silicic acid appears to be at last definitively established.—*Comptes Rendus*, lvi, 590. W. G.

6. *On the coloration of flame by phosphorus and its compounds.*—CHRISTOPLE and BEILSTEIN find that, when phosphorus is added to the materials for preparing hydrogen, the flame takes a beautiful emerald-green color. With the spectroscope, this flame gives two magnificent green lines having about the same degree of intensity, and a third rather less distinct between the two first and the sodium line D. Red phosphorus, phosphorous and hypophosphorous acids give the same result. This reaction is extremely sensitive, and may be used in cases of poisoning by phosphorus, and in detecting the presence of minute quantities of phosphorus in iron.—*Comptes Rendus*, lv, 399. W. G.

ANALYTICAL CHEMISTRY.

7. *On estimation of nitric acid by conversion into ammonia.*—On the 14th of June, 1848, J. C. Nesbit read before the Chemical Society of London (*vide* its *Quarterly Journal*, vol. i, pp. 281–5) a paper "On a New Method for the Quantitative Determination of Nitric Acid, and other Compounds of Nitrogen." Nesbit founded his method on the old observation that "nascent hydrogen" or the metals tin, iron, and zinc, when undergoing solution in a dilute acid, decompose nitric acid with formation of ammonia. He especially refers to the statement of Kuhlmann that the addition of a nitrate, to a mixture of zinc or iron with dilute chlorhydric or nitric acid, causes the evolution of gas to cease for a while, a salt of ammonia being at the same time produced according to the equation—

$$NO_6H + 8H = NH_3 + 6HO.$$

Nesbit ascertained that, by the observance of certain precautions, the whole of the nitrogen of nitric acid or of nitrates may be thus transferred to hydrogen, and obtained as ammonia. His directions are as follows: "If ten grains of salt, such as nitrate of potash, be taken for analysis, about 200 or 300 grains of thin clear (clean ?) fragments of zinc are put into a small flask, to which a portion of water is added. From half to three quarters of an ounce of chlorhydric acid, sp. gr. 1·17, must be poured out into a small measure, and about one-tenth part added to the zinc and water. When effervescence has fairly commenced, a portion of the nitrate, previously dissolved in a small quantity of water, is added to the mixture. The temperature of the whole must, if necessary, be kept low, by placing the vessel in cold water. After a short period, a little more acid is added, and then a little nitrate, until all the solution of the nitrate with the washings is poured in, and about one-fourth of the acid is left. Care should be taken that, for the first hour, the effervescence is slow. When the whole of the solution of the nitrate is poured in, the remainder of the acid must be added from time to time, and the whole left until effervescence ceases. The liquid is then carefully separated from the undissolved zinc, which is well washed with the smallest quantity of water, and the whole distilled with hydrate of lime, the ammonia being collected in a proper condenser. The great danger to be avoided consists in allowing the hydrogen to be liberated too rapidly, by means of which so much heat is generated as to cause a portion of the nitrogen to escape as binoxyd of nitrogen." Nesbit estimated the ammonia by a volumetric method. He gives the results of sixteen determinations of nitric acid, by five different operators, in nitre both pure and mixed with 6 to 9 times its weight of common salt, and in the nitrates of baryta and lead. In each case, the accuracy of the estimation left nothing to be desired.

Nesbit's method has been employed in this laboratory with satisfaction, though several trials were requisite for learning the precise method of procedure.

We have thus noticed the method of Nesbit, because it has not, to our knowledge, been described in any treatise on chemical analysis, and because, since its publication, others have proposed methods based on the same principle which are more or less worthy of notice.

Several years after the method of Nesbit was published, Martin gave out the same process as original, *Comptes Rendus*, xxxvii, 947, with this important difference, that the necessary precautions are indistinctly stated. He directs that pure zinc, in quantity four to five times as much as the nitric acid to be reduced, be placed in a glass, together with the salt or the solution to be analyzed, and pure sulphuric or chlorhydric acid be repeatedly added. Martin mentions that uric acid, sulphate of quinine, gelatine, and "the nitrogenous substance which occurs in water," do not affect the accuracy of the process, though in presence of gelatine the reduction proceeds very slowly.

In 1861, Schulze, of Rostock, proposed to convert nitric acid into ammonia by the action of sodium-amalgam or of platinized zinc in presence of excess of alkali (*Chem. Centralblatt*, 1861, pp. 657 and 833). The manipulations described by Schulze for obtaining good results are somewhat troublesome, and the method, according to Wolff, often practically fails from the slowness of the reduction. Wolff recommends to modify the process by employing a mixture of finely rasped zinc and iron-filings with a soda lye made by dissolving one part of hydrate of soda in eight parts of water, (*Chem. Centralblatt*, 1862, p. 379,) Runge having long ago observed that zinc in contact with iron, in alkaline liquors, suffers solution to a far greater degree than when in contact with platinum. To determine the ammonia, Wolff employs a method elaborated by Knop and himself, in which the nitrogen of the ammonia is liberated by a mixture of hypochlorite of soda and bromine, and estimated by measurement.

The analysis of nitrates by reduction with a mixture of zinc and iron in alkaline liquid has also been the subject of nearly cotemporary study with A. Vernon Harcourt and Mr. Siewert, *Ann. Ch. u. Ph.*, 1863, p. 293. Harcourt obtained excellent results, but, owing to the difficulty of distilling ammonia from a solution containing oxyd of zinc and caustic alkali without some of the latter being carried over, was obliged to employ a complicated apparatus. Siewert presents the simplest method of operating. For the reduction of 0·54 grm. of NO_5, he uses 4 grm. of iron and 8–10 grm. of zinc filings, 16 grm. of solid caustic potash, and 100 c. c. of alcohol of sp. gr. 0·825. The substitution of alcohol for water prevents the troublesome frothing of the mixture when undergoing distillation. The apparatus employed is an evolution flask of 300 to 350 c. c. capacity, which is connected by a rather long doubly-bent tube with a condensing arrangement consisting of two flasks of 200 c. c. content each, that are also connected together by a doubly-bent tube. The tube uniting the evolution flask with the first condensing flask has its extremities cut obliqnely so that no adhering drops of liquid can be carried mechanically either way, and they just pass through the corks which fit them vapor-tight into the flasks. The tube that joins the two condensing flasks, on the other hand, passes nearly to the bottom of these flasks. In the middle flask a quantity of standard acid, more than enough to fix the ammonia yielded by an experiment, is placed, and the mixture of nitrate, potash, zinc, iron, and alcohol being brought into the evolution flask, the corks are tightly fixed, and the gas evolution is assisted by a very gentle heat. In half an hour, the ammonia begins to pass over with alcohol vapor. When all the alcohol has gone over, only traces of ammonia usually remain in the evolution flask. These may be removed, either by continuing the heat until water distils, great care being taken that frothing or spirting do not endanger the result, or better, 10 to 15 c. c. of alcohol are poured into the evolution flask, quickly raising the cork for that purpose. If needful, the addition of alcohol may be repeated. After distilling 2 to 3 hours, the ammonia in the receivers may be estimated by the approved volumetric processes.[1]

Siewert gives the results of a number of determinations which indicate that his method is satisfactory, and its simplicity gives it preference over all the others heretofore devised which are based on the same principle.

It is scarcely needful to remind the analyst that several of the reagents employed are liable to contain either ammonia or nitric acid, and that their purity must be carefully attended to. S. W. J.

III. MINERALOGY AND GEOLOGY.

1. *Annual Report of the State Geologist of California for the year* 1862, 12 pp., 8vo. San Francisco, 1863.—From this brief letter, addressed by Prof. Whitney to the Governor of California, and from a further communication made to the California Academy of Natural Sciences, we abstract the following facts in regard to the progress of the survey: "In the topographical department, a series of maps, forty-nine in number, has been compiled by Mr. Hoffmann from the original documents at the United States Surveyor General's Office: the scale of these is half an inch to the mile. They contain a compilation of nearly all that is known

[1] See this Journal, [2], xxxv, 279.

at that office in regard to the geography of the State. The maps, as thus blocked out, have been used by us in the field, by filling in the topography wherever our route has laid.

The maps which have been or are now being prepared for publication are:

1st. A map of the vicinity of the Bay of San Francisco, on a scale of half an inch to the mile, four feet by three; it extends from near Santa Cruz on the south to Napa on the north, and from the Pacific to Corral Hollow, east and west. The area of land which it covers is 4,248 square miles, which is just twice that of the State of Delaware, and only lacks two hundred square miles of equaling that of Connecticut. As near as can be ascertained, it contains one-third of the population of the State, and has about thirty inhabitants to the square mile—the average density of the population of California being but little over two to the square mile. This map, on which all the details of the topography are given, as minutely as the scale allows, is nearly completed, and will be soon ready for the engraver.

2d. A detailed map, on a scale of two inches to the mile, of the vicinity of Mount Diablo; this is about two and one-half by three feet in dimensions, and includes the most important coal mining district yet known to exist in the State. The map can be made ready for the engraver in a few days.

3d. A map of the Coast Ranges, from the Bay of Monterey south to Santa Barbara. It is about three feet by two and one-half in dimensions, is on a scale of six miles to the inch, and embraces about 16,000 square miles of territory. To complete it will require about another year's work in the field with two sub-parties.

4th. Map of the Washoe silver-mining region—three and one-half by two and one-half feet in dimensions, on a scale of two inches to the mile—and extending over all the important mining ground of the district. This map is from an accurate trigonometrical survey by V. Wackenreuder; it is nearly completed.

5th. Map of the Comstock Lode, on a scale of four hundred feet to the inch, completed.

6th. Map of the central portion of the Sierra Nevada; scale not yet determined on. Extensive surveys have been made by Mr. Wackenreuder for this part of the work, and these will be continued during the present season.

Of the above mentioned maps, Nos. 1 and 2 will accompany the first volume of the Report. Nos. 4, 5, and probably 6, the second volume.

It is intended, if the survey is carried to completion, to construct a final map of the State on a scale of six miles to the inch, in nine sheets, each about three feet square.

In addition to the regular topographical work, an extensive series of barometrical observations has been made, for the determination of altitudes, and some two hundred and fifty important points have been ascended and measured. The most interesting operation in this department was the determination of the height of Mount Shasta, which, by an elaborate series of observations, we found to be 14,440 feet above the sea level. This is the first of the lofty volcanic peaks of the Sierra Nevada which has been accurately measured.

In the department of geology proper, our explorations have extended over portions of forty of the forty-six counties into which the State is divided; and when it is remembered that the average size of a county is equal to half that of the State of Massachusets, (California having just twenty-four times the area of that State,) some idea of the magnitude of our work may be obtained. The chain of the Sierra Nevada may be parallelized with that of the Alps for extent and average elevation; while the Coast Ranges are nearly as extensive as the Appalachian chain of mountains.

We have obtained a pretty clear idea of the general structure of the Coast Ranges from Los Angeles to Clear Lake; the vicinity of the Bay of San Francisco has been worked out in considerable detail, including all of San Francisco, San Mateo, Santa Clara, Alameda, Contra Costa, and Marin Counties, with portions of Santa Cruz, Solano, Napa, and Sonoma. Considerable field-work has been done in the Sierra Nevada, chiefly in the lower portion of the range between Mariposa and Shasta Counties. Our observations have also been extended to the Washoe Region, and we have received considerable collections of fossils from the Humboldt Mining District, (known by this name on the Pacific Coast, but designated on Warren's Map as the "West Humboldt River Range," and in longitude 118°,) by which we have been able to fix the age of the formations in that region.

Mr. Gabb has been chiefly occupied, the past year, in figuring and describing the Cretaceous fossils of the Coast Ranges and the foot-hills of the Sierra, of which he has nearly two hundred new species ready for publication. He has also described the Triassic fossils, collected by the Survey at Washoe, and by Gorham Blake, Esq., in the Humboldt Range. The fossils older than the Trias have been referred to Mr. Meek for investigation. A portion of the fossil plants have been placed in the hands of Dr. J. S. Newberry for description.

It is to the department of General Geology that, up to the present time, by far the greater portion of our attention has been given, since the first thing required in a geological survey is a knowledge of the general geological structure of the State, the age of the various formations which occur in it, and their range and extent, or the position which they occupy on the surface, and their relations to each other. Each group of strata, thus determined by its lithological peculiarities, and by the fossils which it contains, is then to be laid down upon the map, in the position in which its outcrop occupies on the surface. The general character of the minerals and ores which occur in each formation or group of strata having been thus determined, the details of their mode of occurrence, their relative abundance, and the facilities which may exist in each separate district for making them economically available must, after the preliminary general work has been done, be the object of more special and detailed examinations. It is not, however, the business of a geological surveying corps to act, to any considerable extent, as a prospecting party; to do this, would require that we should confine our operations to a very limited area; the labors of the whole corps for an entire season would not suffice to thoroughly prospect more than a few hundred square miles in a very rich mineral region, and we should have often to engage in expensive mining operations to decide what was really of permanent value. It is our task, rather, to limit the field of research, and to show to others where their labors will be best bestowed, preventing foolish expenditures of time and money in searching for what our general geological investigations have determined not to exist in sufficient quantity, in certain formations, to be worth working. Especially in the first years of our work, in a State of such an immense area as California, our labors have more the character of a geological reconnoissance than of a detailed survey.

Already, however, during the progress of our work, a large amount of information has been collected in regard to the mode of occurrence and abundance of the useful ores and minerals of this State and the adjoining Territories. The principal deposits of coal have been carefully examined, and their geological position ascertained. Most of the important quartz mines of the State have been visited by Mr. Ashburner, and a large amount of information has been collected by him, preparatory to an elaborate investigation and report on this important branch of the industry of the Pacific Coast. Considerable work has been done, preliminary to a full report on the geology, mineralogy, and metallurgy of the Washoe region.

In the department of botany and agricultural geology, the work has thus far been chiefly confined to collecting the plants of the State.

Extensive duplicate suites have been preserved, both for study and for exchange, the specimens now collected amounting to not less than twelve thousand or fifteen thousand in number, and embracing probably half of all the species described from the State, besides many new and undescribed ones. The collections have been made by Professor Brewer while engaged in geological explorations, at a very trifling expenditure of time and money.

In the department of agriculture proper, less has been done, owing to limited means. Partial preparation was made for investigating the subject of grape culture, and the production of wines; but discontinued from the same cause. Especial attention has been paid to our native forage plants, to aid in devising some means of arresting the rapid decrease of forage in this State, and correspondence entered into to obtain all possible information on this subject from other regions whose climates are similar to our own.

In the zoological department—in charge of Dr. J. G. Cooper, who has been employed about half the time since the Survey was commenced—the annexed table gives a succinct idea of what has been accomplished, up to the close of the year 1862, in the way of collecting.

Class.	Number of species in the collection.	Of which there are new to California.	Believed to be new, or undescribed.	Other Californian species not yet collected.	Total No. credited to California.	Of which there are found east of the Mississippi.
Mammals	32	10	3	45	77	14
Birds	170	28	4(?)	150	320	141
Reptiles	36	6	3	9	45	0
Fishes	58	16	16	75	133	0
Mollusks	335	123	123	65	400	0(?)

Of Articulates and Radiates no statistics can be given for want of works especially devoted to the California species.

From this it appears that, notwithstanding the large collections made by government expeditions and by individuals, during the last ten years, which have been elaborately described in the Pacific Railroad and Mexican Boundary Reports, the Smithsonian publications, and various other works, we have been able to add materially to the known Fauna of California, and of the country at large, even among the highest and best known classes.

Arrangements have been made for having the collections in natural history referred to the highest authorities in each branch, and portions of our materials have already been placed at the disposition of eminent men in Europe and the United States for examination and description.

Deferring the fitting up of a laboratory, and the engaging of a special assistant in the chemical department, until a suitable permanent place could be provided in the State Museum building, Mr. Ashburner went East in the spring of 1862, and commenced the examination of some of the ores and minerals of the State in the laboratory of the Sheffield Scientific School of Yale College, under the direction of Professor Brush, who has charge of the metallurgical department of that institution. The reduction of the appropriation to fifteen thousand (15,000) dollars for the year, made it necessary to suspend this work soon after it was commenced, in order that the whole force of the Survey might be concentrated on the field operations.

A small sum has been allowed to Mr. F. H. Storer, of Boston, for a chemical investigation of the bituminous substances found in different parts of the State. His researches will probably be embodied in the first or second volume of the annual reports. Qualitative examinations, as well as a few quantitative ones, have been made at the office of the Survey, of specimens which have been collected. A considerable number of coals have been analyzed. Information in regard to ores and minerals has been given to a large number of persons who have applied for the same by letter or otherwise, as will always be done when practicable."

The full results of this work, together with what may be finished during the present year, will probably be published in 1864. These results will make two, or perhaps three, royal octavo volumes, and will be illustrated with maps, sections, plates of fossils, etc. The appropriation for the present year is $20,000, an amount entirely inadequate to carry on the Survey in as complete a manner as contemplated in the Act of the Legislature. We are confident that the people will soon see the great practical advantage to be derived from this Survey in developing the immense mineral and agricultural resources of California, and will make liberal appropriations to have it conducted in a manner alike beneficial and honorable to the State. We have no doubt that, in the establishment of such a laboratory as proposed by Prof. Whitney for the investigation of questions in regard to the composition and metallurgical treatment of ores, more than enough would be saved in one year to pay the whole expenses of the Geological Survey. G. J. B.

2. *Discovery of Childrenite at Hebron in Maine.*—The "minute prismatic crystals of a hair-brown mineral" mentioned in my article on

amblygonite[1] prove, on further examination, to be childrenite. Mr. O. D. Allen has recently visited the locality, and obtained a sufficient quantity of the mineral to determine its specific characters, and a description of these, including crystallographic measurements, by Prof. J. P. Cooke, Jr., will appear in the next number of this Journal. G. J. Brush.

3. *On the Height of Mt. Shasta, California;* by J. D. Whitney, in charge of the Geological Survey of California.—A careful and elaborate series of barometrical observations by the State Geological Corps of California, made in September, 1862, has fixed the elevation of Mt. Shasta at 14,440 feet.[2] Previous to this, the height of Shasta had been variously estimated at from 13,905 to 18,000 feet. The number 13,905 was the result of a barometrical observation made by Mr. W. S. Moses, August 20th, 1861; 18,000 feet was the height as estimated by the Pacific Rail Road expedition, under Lieut. Williamson; Fremont's estimate was 15,000 feet, which is much nearer the truth than Williamson's. It is a very curious fact, that the height of Mt. Shasta, as given by the editor of Colton's Atlas and author of the article on California in the New American Cyclopædia is 14,390 feet, which is a very close approximation. Where these figures were obtained, I have been unable to ascertain.[3] It is pretty certain that they were not the result of any actual measurement, as it is known that Mr. Moses was the first person to ascend the mountain with a barometer.

4. *The Human Jaw at Abbeville.*—Vague and inaccurate statements have been going the rounds of some of the daily and weekly papers regarding the proceedings of the conference of men of science—English and French—which was engaged at Paris last week in investigating the case of the asserted discovery of a human jaw at Abbeville in the fossil state.

The following is a *résumé* of the proceedings:—The English deputies consisted of Mr. Prestwich, Dr. Falconer, Dr. Carpenter, and Mr. Busk, three of whom reached Paris on the 9th and the other on the 10th. The French members were, M. Milne-Edwards (President), M. de Quatrefages, M. Lartet, M. Delesse and M. Desnoyers. Three days were occupied in discussing the question of the flint *haches*, and in the examination of the jaw, the latter of which was taken up on the third day. No decisive result was arrived at. The English members of the Commission maintained the unauthentic character of all the flint *haches* which were yielded by the "black band," and nothing was established on the other side to shake their convictions. The jaw was sawn up and washed; the black coating was removed from it with the utmost facility; there was no infiltration of metallic matter through the walls of the bone, and the section was comparatively fresh looking. The tooth also was in every respect remarkably fresh-looking. The confidence of some of the French members of the Commission was seriously shaken by the characters yielded by the jaw, which, so far as internal evidence went, was want-

[1] This Journal, [2], xxxiv, 243.

[2] A sketch of this mountain, as seen from the South-west-by-South (compass course) is contained in this Journal, [2], vii, 251.

[3] Wilkes says "it is said to be 14,350 feet; but Lieut. Emmons thinks it is not so high."

ing in every appearance which commonly distinguishes fossil bones, and especially those found elsewhere in the Somme deposits. Had the conference been closed at Paris, it is not improbable that the result might have been the Scotch verdict of 'Not proven,' but, at the suggestion of the President, the Commission adjourned to Abbeville on the 12th, when the complexion of the case was at once altered.

Haches of the supposed unauthentic character were disengaged from the cliff of the gravel-pit of Moulin-Quignon, under the very eyes of the Commission, and direct testimony to the actual occurence of the jaw in the "black band" was brought forward to the conviction of the Commission. But there was not the same unanimity respecting the age of the jaw itself. Two of the English members of the Commission, Dr. Falconer and Mr. Busk, handed in notes of the opinions at which they had arrived on the general case. These we insert.—

"Abbeville, May 13, 1863.

"I am of opinion that the finding of the human jaw at Moulin-Quignon is authentic; but that the characters which it presents, taken in connexion with the conditions under which it lay, are not consistent with the said jaw being of any very great antiquity. H. FALCONER."

"Abbeville, May 13, 1863.

"Mr. Busk desires to add, that although he is of opinion, judging from the *external* condition of the jaw, and from other considerations of a more circumstantial nature, that there is no longer reason to doubt that the jaw was found in the situation and under the conditions reported by M. Boucher de Perthes, nevertheless it appears to him that the *internal* condition of the bone is wholly irreconcilable with an antiquity equal to that assigned to the deposits in which it was found."

Mr. Busk of course refers here to the received opinion that the Moulin-Quignon deposits belong to the "high level" gravels of Mr. Prestwich, which are considered to be the oldest of the Somme beds.

From all this, it will be seen that the question of the relative antiquity of the relic is left open to discussion. It is manifest that the evidence was very conflicting; that it is in some respects of an incompatible character; and that a great deal still remains to be cleared up before the scientific world can arrive at a definite judgment on the case. We may further add, that the subject was again brought before the Academy of Sciences, on Monday last, in two distinct notes, by M. Milne-Edwards and M. de Quatrefages, who, we understand, did ample justice to the candor and frankness of their English opponents, and recognized in terms of praise the readiness which they had exhibited in proceeding to Paris when invited, in order to confront the evidence on the spot. We may add another remarkable incident in the case: that, after the communication of their remarks, M. Elie de Beaumont stated that, in his opinion, the gravel deposit of Moulin-Quignon did not belong to the Quaternary or Diluvian age at all, but that it was a member of the *terrains meubles* of the *actual* or *modern* period, in which he would not be the least surprised if human bones were found; adding, moreover, that he did not believe in the asserted existence of man as a contemporary of the extinct elephants, rhinoceroses, &c. of the Quaternary period! The opinion of this very eminent and veteran geologist imports a new element of doubt into the question.

We understand that the English *savants* were received everywhere by their French opponents in the most cordial and friendly manner, and that the various questions involved were discussed in the best possible spirit. The conference lasted five days.

The *Moniteur* of Saturday last, the 16th inst., contains an article by M. Milne-Edwards, giving a brief *résumé* of the constitution and labors of the conference, and of the results to which they were conducted. It is clear that we have still much to learn regarding this very remarkable case, alike in its geological, paleontological and archeological aspects.—*Athen.*, No. 1856, May 23, 1863.

5. *The Geological evidences of the Antiquity of Man, with Remarks on Theories of the Origin of Species;* by Sir CHARLES LYELL, F.R.S. 518 pp., 8vo, with woodcuts. Philadelphia, 1863. George W. Childs. —Man is now the absorbing subject in science. Geology and zoology are bending themselves towards archeology, in order, together, to illustrate his origin and antiquity. Sir Charles Lyell presents us, in his new volume, a review of the progress which investigation has already made; and the extent to which the work has sold, both in this country and Britain, shows that in preparing it he has responded to a public demand. He reviews at length the geological developments of the few years past bearing on the subject, stating the facts with discrimination and fairness, and with all essential details. It is a work, therefore, of real value; and when science has gone forward to established conclusions, it will stand to mark a stage of progress in the important investigation.

The subject is so new that it is not reasonable to regard the work as other than an exhibition of the existing phase of this branch of science. The calculation of time from geological evidence is a mathematical problem involving many variables of unknown limits, and hence all approximations are necessarily rude. *Change of level* is one of these variables of vast influence. Geology proves it to have been in progress in all time; and even, since the Romans were in Britain, a part of Scotland, it says, has been raised 27 feet. The interior of a continent may be supposed to have changed some scores, or even hundreds, of feet in a single period, without doing violence to geological probability. This, then, is one variable affecting seriously all calculations from alluvial or delta formations. For an elevation from such a cause would increase the condensation of moisture about the heights, enlarge rivers, augment their eroding and transporting power both by increasing slope and amount of water, and thereby rapidly thicken the resulting deposits. Moreover, the same action, either in high latitudes or in low, may change, as Lyell has shown, the climate of an entire continent. This is one example of a variable, of wholly unknown limits of variation;—and one affecting calculations from coral reefs and seashore formations, as well as from alluvial beds. It is sufficient of itself to show that the future has yet much to do, before present inferences can command full confidence. Moreover, the doubts connected with the Abbeville deposits, mentioned on the preceding page, show that there are still other variables or unknown quantities to be considered.

It is quite unnecessary to give here an abstract of a volume which is made accessible to all readers, in a convenient and excellent form, through an American republication.

The later part of the volume is occupied with the Darwinian hypothesis with regard to the origin of species, which is presented with an evident leaning towards its views. The hypothesis accords with the Lyellian *uniformitarian* canon, that existing causes *under their present intensity* are sufficient to account for all past events. That caution which leads the author to hesitate before the most obvious generalizations in geology, on the ground that *all* facts are not yet known, here readily yields, and the conclusion that creation has been going on since Man as rapidly as in preceding time, and in the same way, although it has no decisive facts to support it, is accepted with favor. The above canon has done the science good service; but the whole course of geological history is against its too rigid application. We are told that judgment with regard to the transmutation hypothesis as applied to the derivation of Man from Apes should be suspended until Africa and the East Indies have been searched for the missing links. Inductive science says, on the contrary, have no faith in any hypothesis of the kind until the missing links are found. *Facts, then faith*, is the motto for science.

The proper method in geology is to present generalizations and conclusions, just as they naturally flow from known facts—using the *ignorance-argument*, not to set aside obvious deductions from the general array of facts, but to fix the limits of confidence in them; and, then, to modify these generalizations, as time moves on, wherever, and just so far as, this may be required. The uniformitarian canon, in our view, strikes in the head this method, and sets up an assumption in place of an induction.

6. *Ichnographs from the Sandstone of Connecticut River;* by James Deane, M.D. 62 pp. 4to, with 37 lithographic and 9 photographic plates.—For this beautiful volume, on the footprints of the Connecticut valley Mesozoic sandstone, the public are in a large degree indebted to the liberality and science of Thomas T. Bouvé, Esq., of Boston. Dr. Deane, as all readers of this Journal well know, was one of the earliest discoverers and most persevering investigators in this branch of American Paleontology. During the later years of his life, he was not only assiduous in making observations, but labored with great success in lithography, in order that he might make his own illustrations of his papers; and he had a work on the subject well advanced when he died. The volume, the title of which is given above, contains the plates which Dr. Deane had completed, together with the author's brief and unfinished notes, additional explanations by Mr. Bouvé, and a biographical discourse by Henry L. Bowditch, M.D. The lithographs, as well as photographs, are remarkably faithful delineations of the specimens. The footprints are referred in the work to birds, reptiles, insects, crustaceans, and worms. We are especially struck with the beauty and impressiveness of the two photographic plates of rain-prints, one, of ancient, and the other, of recent for comparison. The photographs of footprints also are excellent, and none are more interesting than those of insects and other Articulates. Eleven slabs of these minute tracks are represented on three plates.

No one can turn over the leaves of this volume without a feeling of constant regret that death should have brought the author's labors to so untimely a close; nor without a sense of gratitude towards Mr. Bouvé for the publication of the unfinished work of his friend.

7. *Notice of some new species of Fossils, from a locality of the Niagara group in Indiana, with a list of identified species from the same place;* by Prof. JAMES HALL. Published May 2, 1863. 34 pp. 8vo. Abstract read before the Albany Institute, April 29th, 1862.

8. *Die Ctenodipterinen des Devonischen Systems;* von Dr. CHRISTIAN HEINRICH PANDER. 66 pp. 4to. St. Petersburg, 1858.—*Ueber die Saurodipterinen, Dendrodonten, Glyptolepiden, und Cheirolepiden des Devonischen Systems;* von CHRISTIAN HEINRICH PANDER. 90 pp. 4to, with 17 lithographic plates in folio. St. Petersburg, 1860.—These works by Pander, on Devonian fossil fishes, are published by the Russian government, and are well worthy of the distinction thus given them. The plates are of large size, and are drawn and engraved evidently with minute accuracy. They represent the various specimens found, and also views of sections of the teeth, showing a great variety in their labyrinthine character.

9. *On the Fossil Remains of a long-tailed Bird (Archæopteryx macrurus* Ow.) *from the Lithographic Slate of Solenhofen;* by Prof. RICHARD OWEN, F.R.S.—The author details the circumstances connected with the discovery of the fossil remains, with the impressions of feathers, in the Lithographic slates of Solenhofen, of the Oxfordian or Corallian stage of the Oolitic period, and of the acquisition for the British Museum of the specimen which forms the subject of his paper.

The exposed parts of the skeleton are,—the lower portion of the furculum; part of the left os innominatum; nineteen caudal vertebræ in a consecutive series; several ribs, or portions of ribs; the two scapulæ, humeri, and antibrachial bones; parts of the carpus and metacarpus, with two unguiculate phalanges, probably belonging to the right wing; both femora and tibiæ, and the bones of the right foot; impressions of the quill-feathers radiating fan-wise from each carpus, and diverging in pairs from each side of the long and slender tail. The above parts indicate the size of the winged and feathered creature to have been about that of a rook. The several bones, with their impressions and those of the feathers, are described, and the bones are compared with their homologues in different Birds and in Pterodactyls: whence it appears that, with the exception of the caudal region of the vertebral column, and apparently of a biunguiculate manus, with a less confluent condition of the metacarpus, the preserved parts of the skeleton of the feathered animal accord with the ornithic modifications of the vertebrate skeleton. The main departure therefrom is in a part of that skeleton most subject to variety. Twenty caudal vertebræ extend from the sacrum in a consecutive and naturally articulated series, resembling in structure and proportions those of a squirrel. The tail-feathers are in pairs corresponding in number with the vertebræ, diverging therefrom at an angle of 45° backward, becoming more acute near the end, and the last pair extending nearly parallel with, and 3½ inches beyond, the last caudal vertebra. This feathered tail is 11 inches long and 3½ inches broad, with an obtusely rounded end. This character of the tail is novel and unexpected because of the constancy with which all known existing and Tertiary birds have presented the short bony tail, with the terminal modification, in most of them, of the ploughshare bone.

Professor Owen next gives the results of investigations into the ostegeny of embryo birds, showing the number of vertebræ corresponding to the anterior caudals in *Archæopteryx* which coalesce with the pelvis in the course of growth, and the degree to which the posterior caudals retain a resemblance to those of *Archæopteryx* in the Birds with rudimental wings. From eighteen to twenty caudal vertebræ may be counted in the young Ostrich. In *Archæopteryx* the embryonal separation persists, with such continued growth of the individual caudal vertebræ as is commonly seen in long-tailed Vertebrates, whether Reptilian or Mammalian. The author remarks that the modification and specialization of the terminal bones of the spinal column in modern birds is closely analogous to that which converts the long, slender, many-jointed tail of the modern embryo fish into that short and deep symmetrical shape, with coalescence of terminal vertebræ into a compressed lamelliform bone, like the 'os en charrue' of birds, to which the term 'homocercal' applies—such extreme development and transformation usually passing through the heterocercal stage, at which, in Paleozoic and many Mesozoic fishes, it was arrested. Thus he discerns, in the main differential character of the Mesozoic bird, a retention of structure which is embryonal and transitory in the modern representatives of the class, and consequently a closer adhesion to the general vertebrate type.

The least equivocal parts of the present fossil declare it to be a Bird, with rare peculiarities indicative of a distinct order in that class. Although the head is absent, the author predicts, by the law of correlation, a beak-shaped mouth for the preening of the plumage; and he also infers a broad and keeled sternum in correlation with the remains of feathered organs of flight.

The paper is accompanied by drawings of the fossil and its parts, and of homologous parts in Birds and Pterodactyls. The author assigns to the fossil animal the name of *Archæopteryx macrurus.—Proceedings of Royal Society*, Nov. 20, 1862, in the *Ann. Mag. Nat. Hist.*, [3], vol. xi, No. 62.

VI. BOTANY AND ZOOLOGY.

1. *Botanical Papers in the Transactions of the St. Louis Academy of Science*, vol. ii, part 1.—Dr. C. C. Parry, in a letter to Dr. Torrey, and by him communicated to the St. Louis Academy, gives a lively and graphic account of his ascent of Pike's Peak on the first of July, 1862, with interesting botanical memoranda. Dr. Engelmann, the President of the Academy, follows with a paper on the altitude of Pike's Peak and other points in Colorado Territory, from a reduction of all the recent observations, mainly those of Dr. Parry, with an account of all the earlier *data*. The limit of trees is found at the elevation of 11,643 feet on the eastern slope of Gray's Peak, at 12,043 feet on the northern slope of Pike's Peak. The determinations of the higher summits and other interesting points will doubtless be reproduced in another part of this Journal. Dr. Engel-

mann also contributes an interesting account of the structure and morphology of *Nelumbium luteum;* a note on the nature of the pulp in the fruit of *Cacteæ*, showing that the pulp belongs to the funiculi of the seed or its appendages; also on the pulp of currants and gooseberries, which he finds "to consist of the arillus and of the modified epidermis of the testa;" a considerable paper entitled "Additions to the Cactus-Flora of the territory of the United States," mostly from the collections of Dr. Newberry (in the Colorado Expedition, 1857–58) and of Henry Engelmann in Capt. Simpson's Expedition to explore emigrant routes into Utah. On *Pinus aristata*, a new species of Pine discovered by Dr. C. C. Parry in the Alpine regions of Colorado Territory, and on some other Pines of the Rocky Mountains. *P. aristata* was probably confounded by the late Dr. James with his *P. flexilis*, but was distinguished by Dr. Parry. Both are five-leaved species, but they belong to two very different sections. They are now well cleared up by Dr. Engelmann; and, as seeds collected by Dr. Parry in considerable quantity have germinated in this country and in England, they may hereafter be familiar. Dr. Engelmann appends to this paper a revision of the character of the genera of *Abietineæ*, in a somewhat new arrangement, adopting as genera, 1. *Abies* Link. (non Linn.), the Balsam Firs; 2. *Tsuga*, Hemlock Spruce; 3. *Larix*, (these all in one group characterized mainly by the anthers); 4. *Cedrus*, Cedar, and 5. *Picea* Link, non Linn., the spruces proper, in another group; and finally, with biennial fructification, 6. *Pinus*. Dr. Engelmann also has a paper on "New Species of *Gentiana* from the Alpine regions of the Rocky Mountains," with some emendations of other species, &c. The plates, from t. 5 to t. 11, neatly illustrate *Pinus aristata*, *Gentiana Wislizeni*, *G. heterosepala*, *G. humilis*, *G. acuta*, var. and *G. prostrata*, *G. Parryi*, and *G. barbellata*, of Engelmann. A. G.

2. *The Enumeration of the Species of Plants collected by Dr. C. C. Parry and Messrs. Elihu Hall and J. P. Harbour, during the Summer and Autumn of* 1862, *on or near the Rocky Mountains in Colorado Territory*, lat. 39°–41°, is published in the Proceedings of the Academy of Natural Sciences, Philadelphia, under date of March 24, 1863. The collection, which has already been mentioned in this Journal, proves to be very interesting. A. G.

3. *Paullinia sorbilis and its products.*—Mr. Archer, in a paper read to the Botanical Society of Edinburgh, April 9, states that from the large seeds of this Sapindaceous plant "is manufactured the substance called *Guarana*, which is extensively used in Brazil, Guatemala, Costa Rica and other parts of South America, as a nervous stimulant and restorative. The seeds, deprived of their covering, are pounded into a paste, which hardened in the sun constitutes *Guarana*. It is used both as a remedy for various diseases, and also as a material for making a most refreshing beverage. . . . . . The presence of an alkaloid, which he called *Guaranine*, was discovered some years ago in Guarana by Dr. Theodore von Martius of Erlangen; but its identity with Theine was soon established; and subsequent analyses, especially one by Dr. Stenhouse in 1856, proved that not only was the active principle of Guarana identical with Theine, but that, as far as known, no other substance yields it so abundantly, —the amount being 5·07 per cent, as against good black Tea, which

yields 2·13, and Coffee, from 0·8 to 1·00. The mode of using the Guarana is curious and interesting. It is carried in the pocket of almost every traveller, and with it the palate bone or a scale of the large fish, *Sudis gigas*, locally called 'pirarucu,' the rough surfaces of which form a rasp upon which the Guarana is grated; a few grains of the powder so formed are added to water, and drunk as a substitute for tea. The effect is very agreeable, but, as there is a large quantity of tannic acid also present, it is not a good thing for weak digestions."—*Extr. Gard. Chron.*, May 2, p. 414.

4. *Aerial rootlets on the stems of Virginia Creeper* (*Ampelopsis quinquefolia*).—Referring to the note on p. 445 of the last No. of this Journal, we may add that we have received, from Dr. Parry of Iowa, specimens of the stem of this plant abundantly garnished with aerial rootlets, thus confirming the character in Michaux's *Flora*, and in the lamented Dr. Darlington's *Flora Cestrica*. A. G.

5. *Martius, Flora Brasiliensis*, fasc. xxxi, xxxii. (Jan., 1863).—These new parts of this great work contain the *Dilleniaceæ*, by Dr. Eichler, with 14 plates; and the *Sapoteæ*, by Prof. Miquel, 31 plates. Both orders appear to be well elaborated. A. G.

6. *Dr. Charles Wilkins Short* died at his residence at Louisville, Kentucky, on the 7th of March last, in the 69th year of his age.[1] He was born in Woodford County of that State, on the 6th of October, 1794, was educated in the school of Mr. Joshua Fry, near Danville,—a distinguished teacher of those days,—pursued his professional studies mainly in Philadelphia, where he took the degree of M.D. from the University of Pennsylvania in the year 1815. For ten years he devoted himself to the practice of medicine, until, in the year 1825, he was called to the chair of Materia Medica and Medical Botany in the Transylvania University at Lexington, where he contributed to the reputation of that celebrated school. Relinquishing medical practice, for which he had no liking, he devoted his powers with zeal and success to the more congenial duties of his professorship, and to the cultivation of botany, the favorite pursuit of his life. At the close of the year 1838, he removed, along with some of his distinguished colleagues, to Louisville, filling the same chair in the University of that city, until 1849, when he retired from public functions. The remainder of his honorable life was passed at Hayfield, his tasteful residence near Louisville, in the bosom of his family; in the exercise of kindly but unostentatious hospitality and of all good offices; in quietly enjoying and causing others to enjoy the blessings of a handsome fortune, to which by inheritance, combined with the fruits of his own industry, he had now attained, and in the cultivation of "the amiable science" to which he was devotedly attached.

Dr. Short's botanical publications were neither large nor many. They were chiefly articles contributed to the Transylvania Journal of Medicine, &c., of which he was for some time one of the editors. The most important is his Catalogue of the plants of his native State (which he widely and assiduously explored), and several Supplements; with well considered characters of some new species, and acute and discriminating notes upon several imperfectly known plants. These, and the copious MSS. observations which he was accustomed for many years to commu-

[1] See this Journ., xxxv, 451.

nicate to his botanical correspondents, showed what he was capable of accomplishing, had not a most retiring and unambitious disposition unduly limited his exertion. It was not activity or persevering labor, but publicity, that he shrunk from. He was a very industrious botanist, and an effectual promoter of our science in this country. His great usefulness in this field was mainly owing to the extent and the particular excellence of his personal collections, and to the generous profusion with which he distributed them far and wide among his fellow-laborers in this and other lands. He and the late Mr. Oakes—the one in the west and the other in the east, but independently—were the first in this country to prepare on an ample scale dried specimens of uniform and superlative excellence and beauty, and in lavish abundance for the purpose of supplying all who could need them. Dr. Short's disinterested activity in these respects has enriched almost every considerable herbarium both at home and abroad, and set an example which has produced large and good results among us. The vast improvement in the character of the dried specimens now generally made by our botanists may be mainly traced to the example and influence of Dr. Short and Mr. Oakes. As might be expected, Dr. Short's own herbarium is a model of taste and neatness. It is also large and important. To one himself so solicitous "to do good and to communicate," contributions from numerous sources naturally flowed in abundantly. He, moreover, subscribed to all the North American distributed collections within his reach, and he set on foot or efficiently furthered several distant or difficult botanical explorations. He purchased, at a liberal price, the important botanical collections of Texas and Northern Mexico, left by Berlandier, which Lieut. (now General) Couch acquired of his widow, and sent on to Washington; and, retaining one set for his own herbarium, he caused the rest to be distributed among the botanists to whom they would be most useful,—especially including two Swiss botanists who had contributed to send out Berlandier to Mexico as a collector, but from whom (apparently through Berlandier's dishonesty,) they had failed to receive any adequate return. It is understood that Dr. Short's rich herbarium—to which a lifetime of thoughtful attention and much expense were lovingly devoted—is now offered, by a wise bequest, to the custody of the Smithsonian Institution, under instructions that it shall be permanently well cared for and always open to be consulted by botanists. It will there form an excellent and conspicuous nucleus for a collection of American herbaria, such as our science needs, and the country ought to possess.

The natural effects upon his scientific career of a fastidious taste, an unwarrantable diffidence, and a too retiring disposition, were enhanced by a constitutional tendency to depression of spirits. But this never obscured the native kindness of his heart, and the real, though so quiet, geniality of his disposition, or checked an unobtrusive and considerate benevolence. With an uncompromising sense of right and justice, and a keen hatred of everything mean and unworthy, he was never harsh nor even cynical. All who knew him well, and also his more intimate correspondents who never enjoyed the privilege of a personal acquaintance, can testify to the nobility and Christian excellence of his character. An appreciative tribute to his memory, from the pen of a former colleague, will be found in the *Louisville Journal*, issued a few days after Dr. Short's lamented death.

Two or three species of Kentucky plants commemorate the name of Dr. Short as their discoverer. Also a new genus, *Shortia*, inhabiting the Alleghany Mountains, was dedicated to him by the present writer. But, alas! too like the botanist for whom it was named, it is so retiring in its habits that it is not known as it ought to be, but lives as yet unseen, except by a single botanist of a former generation, in some secluded recess of the Black Mountain of North Carolina. It will some day be found again and appreciated. A. G.

7. *The late Wm. Darlington.*—Not unexpectedly are we called to add to the list of the departed, the name of this venerable and excellent man. The Nestor of American Botanists died, at his residence in West Chester, Penn., on the 23rd of April last, having nearly completed the 81st year of his age. The death of this most charming and unaffectedly good man, although occurring in the fulness of time, and following close (as was meet) upon the bodily infirmities which at length arrested the serene activity of the octagenarian,—waiting, but laboring still as strength and occasion served,—is sensibly felt, not only in the town and county where he has long been honored and venerated, but also by a wide circle of friends and correspondents throughout the country and in other lands. If not a very profound, he was a most accurate and faithful botanist, one who appreciated and largely imbibed the spirit of all the great advances in botanical philosophy, and especially in morphology, which have been made in his day. His *forte* was in the clear and accurate description of plants; his desire, to make perfectly known the plants of his native county; his modest estimate of his own labors in his favorite pursuit was expressed in the motto of his *Florula Cestrica*, in 1826, and repeated in his classical *Flora Cestrica* (one of the very best local floras ever written) in 1837 and 1853: "*Ore trahit quodcunque potest, atque addit acervo:*" his love for the familiar objects which had attracted his life-long interest was characteristically shown in the inscription which he wrote for the stone that now covers his mortal remains: "*Plantæ Cestrienses quas dilexit atque illustravit super tumulum ejus semper floreant;*" while higher feelings and sacred hopes were fittingly expressed when on receiving the first warning, of which he knew well the significance, he said to those around him, "My work is done; and I think I can say, with Simeon of old, 'Lord, now lettest thou thy servant depart in peace.'"

But, much as he cultivated Botany, this was only the side-issue, the recreation of his life, which was actively devoted to professional and various civic occupations, and to the discharge of many honorable trusts. Such biographical notices as may well be added here, we will select from a "Memorial of Wm. Darlington, M.D.", drawn up by one of his townsmen, of which a copy has just been received by us.

"He was born near the ancient village of *Dilworth*, now called Dilworthstown, in Birmingham township, Chester county, Pennsylvania, April 28, 1782.

"His great grandfather, Abraham Darlington, the son of Job and Mary Darlington of Darnhall, in Cheshire, England, came, while a young man, with his brother John, to Pennsylvania, in the beginning of the last century, and settled at first near Chester. * *

"The grandfather of William Darlington was a farmer, and resided in East Bradford township, Chester county. He married Hannah, a daughter of Edward Brinton, a member of an old and respectable family that had come over to America amongst the earlier settlers of Pennsylvania. He raised and educated nine sons and two daughters, and died in the Autumn of 1808.

"Edward Darlington, the eldest son of Thomas, and father of William, was educated a farmer by his maternal grandfather, from whom he received, by will, the farm in Birmingham township, on which he was reared, and which is now in the possession of his grandchildren.

"He married Hannah, a daughter of John Townsend, of East Bradford, Chester county, by whom he had five sons and two daughters. He was an intelligent man, self-educated, and exercised a considerable influence amongst the citizens of his county, by whom he was several times elected a member of the State Legislature. He died in 1825.

"William Darlington, of whom we shall now speak, was the eldest child of Edward and Hannah Darlington, and descended from ancestors, each branch of which, as far as it can be traced, was an unmixed race of plain English Quakers. He was early inured to the severe labors of agricultural life, and, when old enough to drive or hold the plough, was kept at work in the summer, and only permitted to go to school in the winter season. The common country schools of that day were lamentably deficient as compared with those of modern times, yet he succeeded in obtaining a plain English education, under John Forsythe, an Irish Friend, one of the best teachers of that time in the county, and who, during a long period spent in that vocation, imparted the rudiments of education to many who have since become eminent and useful citizens of the republic.

"Becoming tired and disgusted with the drudgery of farm labor, which then was not one tithe as attractive as it has since been made by the inducements offered and efforts made by Agricultural Chemistry, and Agricultural and Horticultural Societies, and the improvement in machinery whereby so much of the labor of the farm is now avoided, William, after much difficulty, induced his father to permit him to study medicine. With this view, in the Spring of 1800, he entered the office of Dr. John Vaughan, a respectable physician of Wilmington, in the State of Delaware.

"Whilst pursuing with assiduity the study of that profession which he had selected as the business of his life, he devoted those hours which, with many, would have been given to idle recreation, to acquiring a knowledge of the French language under a private teacher, and there developed a passion for the study of languages which remained with him for life, and enabled him subsequently to make an excellent and satisfactory acquaintance with the French, Latin, Spanish and German, when opportunity was afforded. So strong was his taste for such acquisitions, that at the age of fifty, a period when many men think their labors are over, he embarked in the study of the noble Castilian tongue with all the ardor of a schoolboy of seventeen, and mastered it thoroughly.

"In 1802 the malignant Yellow Fever was fearfully prevalent in many places throughout the Union, and scourged the country with a violence that made the boldest physicians shrink aghast from its awful ravages. Amongst other places, it visited Wilmington, carrying terror and desolation in its train. Large numbers of the citizens sought safety in flight; even *physicians* left the place, and the only medical personages that remained were Dr. Vaughan and his pupil William Darlington,—who with great moral courage faithfully continued at their posts, and rendered their services to those afflicted with the fearful epidemic.

"In the Winters of 1802–3 and 1803–4, William Darlington attended the medical lectures in the University of Pennsylvania, and on the sixth of June, 1804, he received the degree of Doctor of Medicine, being, as the writer believes, the first citizen of Chester county who took that degree in that University. For a long term of years, and until he relinquished the duties of his profession, he was confessedly the head of that profession in the county of his birth. The subject of his inaugural Thesis was "the mutual influence of habits and disease," an essay which, from the soundness of its views and depth of scientific research, received a flattering compliment from Professor Rush, at a public examination on the day prior to the commencement.

"Whilst preparing his Thesis, after the close of his second course of medical lectures, Dr. Darlington attended the botanical lectures of Professor Benjamin Smith Barton, and thus began his first acquaintance with that science whose beauties and pleasures he has, in later years, done so much to illustrate, and in so successful a manner as to make his name known and respected throughout the botanical world.

"On receiving his diploma, he returned to his native place and commenced the practice of medicine, and in his leisure hours availed himself of the first opportunity that presented, of making himself familiar with the Latin language, which in those days seemed to hold the key of the temple of the physical and natural sciences. In the following year, he was appointed physician to the Chester County Alms House, and also surgeon to a regiment of militia. The latter appointment, however, caused his disownment by the Society of Friends, of which he was a member, as it was contrary to their discipline to assist in, or encourage, war, in any manner whatever.

"Since that day, however, the views of the 'Friends' seem to have changed somewhat, upon this subject, and the former rigidity of the discipline in regard to it has relaxed, and there are now in the Union armies large numbers of young Friends, who are offering their lives in the service of their country, in as earnest and effectual a manner, and with as unselfish a patriotism, as the men of any other religious denomination.

"In 1806, Dr. Darlington received the appointment of surgeon to an East India Merchantman, belonging to Philadelphia, and made a voyage to Calcutta,[1] whence he returned the following year. He availed himself of the leisure afforded him in the long voyage, to make an acquaintance with some of the best works then extant in English literature. A sketch of the observations made during this voyage was, some years afterwards, published in the form of familiar letters in the Analectic Magazine.

"In the year succeeding his return from Calcutta, he settled in West Chester, and resumed the practice of medicine, and was soon in the enjoyment of an extensive and profitable business, which embraced a large extent of country, and required laborious industry and perseverance to give it the requisite attention, as physicians then were few and far between, in the rural districts. The famous embargo, in Jefferson's administration, prevented any further voyaging by sea, but circumstances had occurred in the meantime which would have detained him at home without an act of Congress; for, on the first of June of that year (1808), he was married to Catharine, daughter of General John Lacey, of New Jersey, an officer who had served with credit and ability in the revolutionary war.

"Always anxious for self-improvement, Doctor Darlington commenced the German language about that time under a private tutor, and soon made himself sufficiently familiar with it to be enabled to enter into the spirit and enjoy the beauties of the great writers in that tongue. The love of the German then acquired increased with years, and at the ripe age of 81, and up to the hour of his death, he enjoyed the immortal works of Schiller, Lessing, Goethe, and other German authors, with which his library was stored, with all the zest which the strength of diction, harmony of verse, and beauty of thought, that characterize the writings of those eminent men, are so well calculated to

[[1] Where he made the congenial acquaintance of the late Dr. Wallich, the well known botanist, and director of the Calcutta Botanic Garden.—Eds.]

inspire. He was fortunate, too, in having instilled into one of his daughters the same love of language which imbued his own mind, and her familiar knowledge of the Latin, French, and German tongues enabled her to add to his happiness in his later years, by sharing with him those pleasures which frequent converse with the best authors in those languages never fails to ensure.

"When the war with England broke out in 1812, the subject of this sketch, with other young men of the neighborhood, offered their services in defense of the altars and firesides of their country in case of invasion. A volunteer company was formed and drilled at West Chester, ready to serve when called upon, and in September, 1814, on a requisition by the Governor of Pennsylvania for volunteer troops to aid in the protection of Philadelphia, which was supposed to be threatened by the enemy then in Chesapeake Bay, he went to the camp on the banks of the Delaware as an ensign in the "American Grays." Having some taste and skill in military tactics, the regiment into which his company was incorporated chose him Major of the first battalion. In this post he served until the corps was disbanded, and was rewarded like his fellow-soldiers with the meagre pay of that day, and the still more meagre national grant of forty acres of the public domain.

"In the meantime, however, his fellow citizens at home, appreciating his worth as a physician, a friend of education, a citizen soldier, and an enlightened statesman, elected him, unsolicited, a member of the 14th Congress. In 1816, in consequence of dissatisfaction existing towards his colleague in another county, (the single district system not having been then adopted,) he lost his election by the small majority of seven votes, but this defeat was amply atoned for by triumphant elections to the 16th and 17th Congress, from the same district.

"During his second term the celebrated Missouri question agitated the Union from one end to the other, and called forth the ablest efforts of the best men in Congress. On that question Dr. Darlington was found ranked with those who were desirous to restrict slavery, and raised his voice in an able and excellent speech in opposition to its extension.

"On that occasion he said: 'We are solemnly bound not only to secure our own welfare, but to provide, as far as we can, for that of our posterity. When we *know* that the welfare of our descendants in Missouri, as well as in the United States generally, *requires the restriction of slavery*, how can we reconcile it to our sense of duty to permit the unnecessary introduction and diffusion of an evil which we are sure *will be the scourge of countless generations*.'" * * * *

"Dr. Darlington was one of the members of the first board of Canal Commissioners, and was associated with such men as Albert Gallatin, John Sergeant, Robert W. Patterson, and David Scott, whose names hold a distinguished place in our country's annals. He served in that station two years, during the last of which he was President of the board."

"The duties alluded to, however, though arduous and exacting, did not prevent Dr. Darlington from bestowing some attention to Natural Science, and indulging his taste for botany. In 1826, in conjunction with some of his intimate friends, he assisted in organizing the Chester County Cabinet of Natural Science, of which institution he was President from its origin; and in the same year he published his 'Florula Cestrica,' being a catalogue of plants growing around the borough of West Chester, Pennsylvania.

"The arduous duties of the office of Canal Commissioner, being then performed gratuitously, and calling him away from home more than was either convenient or agreeable, he resigned that office the next year, and was almost immediately thereafter appointed Prothonotary and Clerk of the Courts of his native county, by his political and personal friend, the late lamented Governor Schulze, the duties of which office he continued to discharge till 1830.

"Whilst in the office of Prothonotary Dr. Darlington and some of his medical friends co-operated, and formed the Medical Society of Chester county, an institution which has had the good effect of uniting in a fraternal union almost all the physicians of the county. Through its periodical meetings, addresses, written communications, and debates, it has been the means of promoting the increase of medical knowledge, of establishing an *esprit de corps* amongst medical men, and of removing those petty jealousies which are too apt to arise in a profession whose country members live in comparative isolation, and have very little communication with one another. From his long standing in his profession, and the skill which he had acquired by an extensive practice, Dr. Darlington was unanimously placed at the head of the Society, which position he held till 1852, when he resigned and was immediately elected an honorary member."

"About the same time he assisted in exploring a route for a railroad from West Chester towards Philadelphia to intersect the Columbia railroad.

"Through the exertions of himself and a few gentlemen of West Chester, a company was formed, of which he was made the first President, and superintended the construction of the road, which was the first private tributary to the line of public works."

"In 1830, he was elected president of the Bank of Chester County, of which institution he had been one of the commissioners named in the Charter for receiving subscriptions of its capital stock, and a director almost ever since its establishment in 1814. He was re-elected annually, and continued in that station to the time of his death."

"Its currency was so regulated, and its discounts so discreetly made, that it still continued to be an instrument of good to the citizens of the county in which it was located. It possessed their entire confidence, and its notes were eagerly sought after in preference to those of most other banks within the range of its circulation. These happy results were mainly due to the financial abilities of the president and his old and long tried friend, David Townsend, late cashier of the bank, a gentleman who, it is not improper to state here, was associated with Dr. Darlington in nearly all of the public enterprises of a local character in which the latter was engaged. His acquirements in the Doctor's favorite science of botany, together with the excellence and value of his exchanges of plants with European botanists, obtained for him the high compliment of having his name conferred upon a new and interesting genus of Arctic American and Rocky Mountain plants, by his friend Professor Hooker, the learned and talented Director of the Royal Botanical Gardens at Kew, near London.

"A similar honor was conferred on Dr. Darlington in 1825, by Professor DeCandolle, of Geneva, for his eminent services in the beautiful science. The genus dedicated to him by DeCandolle did not, however, prove to be sufficiently distinct to maintain its place as an independent genus, and his friend Professor Torrey, of New York, dedicated to him a new and splendid genus of California Plants, of the natural family of Sarraceniaceæ, which, from its rarity and beauty, constitutes a worthy and fitting compliment to an industrious laborer in the agreeable fields of botanical science.

"To his botanical friends it may be interesting to learn, that Professor Gray, of Cambridge, has just succeeded in raising it on the Atlantic slope, and we may soon have the pleasure of cultivating in our gardens the beautiful Darlingtonia.

"It is too seldom that we find a love for natural science, or the fine arts, in a temple devoted to mammon. The Bank of Chester County, however, is an exception. The President and Cashier of that institution prosecuted their scientific researches together, collecting treasures for their herbaria o'er hill and valley, and making exchanges with many of the most eminent botanists of Europe, whilst the present cashier, (Mr. William W. Jefferis,) is an excellent mineralogist, and has collected one of the best mineral cabinets in the State, and furnished many valuable contributions to mineralogy, in the discovery of new and heretofore undescribed species and varieties.

"In the year 1837, Dr. Darlington published his 'Flora Cestrica,' a description of the flowering plants of Chester county, which was a new edition of his former work, much enlarged and greatly improved.

"This work is regarded as one of the most complete local Floras extant, and is a model for all works of a similar character that may be constructed on the artificial system of botany.

"Conceiving the idea of assisting the farmers of our country by a work expressly devoted to an account of those plants which it more especially concerns them to know, he prepared and published in 1847 his 'Agricultural Botany,' in which are described in plain and familiar terms not only the useful *cultivated* plants, but all those which a careful and industrious farmer should extirpate from his soil. This work is one of the practical benefits which natural science sometimes bestows upon mankind, and there is good reason to believe that its influence has already produced a beneficial effect upon husbandry, not only in Chester county but elsewhere.

"The deep interest he always felt in every votary of natural science, together with a strong personal attachment for a friend, induced him at an earlier day, (about 1843,) to collect together the letters, memoranda, &c., of Dr. William Baldwin, a native of his own county, who was also passionately devoted to botany, but who died at an early age while on the expedition up the Missouri, under Major Long. These remains were given to the world in a volume entitled 'Reliquiæ Baldwinianæ.'

"The pioneers of botany in Pennsylvania were Humphrey Marshall and John Bartram; the former residing near West Chester, the latter at Darby, near Philadelphia. Dr. Darlington collected, in 1849, such portions of their correspondence as still remained in existence, comprising, together with their own letters, those of many eminent botanists of the day, and published them in one large volume, with illustrations of their homes, under the title of 'Memorials of Bartram and Marshall.'

"This correspondence of our earlier botanists affords a pleasant insight into their scientific labors, and shows the dangers they underwent, and the difficulties they had to encounter in the early settlement of the country, during their expeditions into the wilderness in the prosecution of their favorite science. The former home of Humphrey Marshall still stands at Marshalton, in Chester county, and the rare and curious forest trees that he planted with his own hands around it have grown with years, until they have become objects of great interest to every votary of botanical learning.

"Dr. Darlington's latest labors in the cause of natural science consist in a new edition of the 'Flora Cestrica,' revised and reconstructed on the natural method, which seems to be the system most generally adopted by scientific botanists at the present day. Besides this, in connection with some of the liberal minded men of his neighborhood, he was engaged in his latter years in the composition of a work descriptive of the objects of the Natural History of Chester County in all its branches.

"In the Spring of 1862, he was attacked by a slight stroke of paralysis, from which he partially recovered, but with some prostration of his physical vigor. This was followed in the early part of 1863 by another attack of the same disease, from the effects of which he gradually sank, until on Thursday, the 23d of April, 1863, aged nearly 81 years, he went to his final rest, with his mental vigor unimpaired, having evinced in his conversation with his children, during his last illness, the same love of science and literature which had characterized him through a life protracted much beyond the usual period allotted to man."

"In order that all the people of the county, who desire improvement in natural science, may continue to have, after his death, the same sources of knowledge as he could afford them in his life, he has bequeathed his most valuable herbarium of plants, and all his botanical and most of his other scientific works to the Chester County Cabinet of Natural Science, on whose shelves they are designed to remain as a rich mine from which the earnest students of nature may always be enabled to glean most precious fruits.

"He enjoyed, in an eminent degree, the friendship of the best botanists of his day, and his correspondence with the distinguished DeCandolle, and Sir William Jackson Hooker, of the old world, and Doctors Torrey and Gray of the new, attest the high value they placed on his contributions to the gentle science of which he was so fond, and which, with them he assisted so much to illustrate.

"In his social relations he was the kind friend, whose heart and purse were ever open to assist struggling merit, in whatever walk of life it might be found, and his contributions to all purposes of benevolence, philanthopy, or knowledge, were, according to his means, of the most generous character. He was an indulgent parent, whose earnest desire was to make his family useful to themselves and the community, in which he has happily succeeded, and he was the pleasant neighbor, whose extensive knowledge, excellent memory, and agreeable conversational powers, made him a most interesting companion.

"His mind was, through his whole life, ever prompt and active, and in the last work of his hands, 'Notæ Cestrienses,' or sketches of the most distinguished men of his county, which was undertaken with his friend, J. Smith Futhey, when he was nearly eighty years of age, and which was finished only a few months before his death, he felt that each biographical sketch might be the last, and he labored upon it with youthful zeal and earnestness, that it might be finished before he heard the Master's call.

"He died as he lived, a christian gentleman, of great purity and simplicity of character, whose whole life was never stained by a mean, ungenerous, or dishonest action.

"From this slight sketch of Dr. Darlington, it will be observed that he has been a man of both thought and action, of books and deeds, and has spent a busy life in the service of his county, his state and nation, and endeavored, in a quiet and unostentatious manner, to disseminate information amongst the masses of the people.

"Although greatly esteemed for his literary abilities, which have been highly self-cultivated, yet his strongest hold on the public regard arose from the earnest efforts he has been ever ready to make in the cause of natural science and popular education, from the time, when, like the widow of old, he gave his mite, being all he was then worth, towards the good cause, down to the last years of his life, of which he devoted the hours most men give to rest, to the agreeable and useful task of diffusing knowledge among men.

"It is pleasant to know that those labors have been properly appreciated by men whose commendations are of value, as may be found in the fact that the self-taught farmer's lad has had his name and fame bequeathed to future time so long as plants shall grow and bloom; that he received in 1848 the highly honorable degree of LL.D., from Yale College, and was elected a member of more than forty literary and scientific associations.

"The West Chester Academy, the Medical Society of this county, the Chester County Cabinet of Natural Science, the West Chester Library, the Agricultural and Horticultural Societies of Chester county, were all indebted to him for his valuable aid in giving the popular impulses which brought them into existence, and which have since continued and extended their usefulness.

"His example and instructions, his books and pamphlets, his discourses and lectures, infused a taste for literary and scientific information into the minds of the young around him. * * * They have been the means of awakening a thirst of knowledge amongst the people of the place of his residence, and a desire for good educational institutions, until Chester county has become noted for the general intelligence of its citizens, and the excellence of its numerous schools.

"Temperate in his habits, moral and religious in his character, in the full maturity of years, and with his mental faculties almost unimpaired to the last, he enjoyed with satisfaction, at a good old age, the consciousness of a life well spent, and the contemplation of the ripened fruits produced as the results of his earlier and later labors, and in the enjoyment of the respect of a grateful community, he was enabled to feel that his career had been a useful one to the people amongst whom Providence had placed him, and that his years were not employed like those of the fool or the sluggard, but improved to the benefit of himself and of future generations, as the seeds of knowledge once sown are not for a single harvest, but for all future time."

It is understood that Dr. Darlington has left in the hands of a friend an autobiography. We know not whether this was written in view of future publication; but it may probably with propriety be printed, after some lapse of time, along with selections from his correspondence, for the gratification of the numerous friends of the writer, or even for the instruction of a wider circle of readers. A. G.

ZOOLOGY—

1. *Observations on the genus Unio, together with descriptions of new species, their soft parts and embryonic forms, in the family Unionidæ, and descriptions of new genera and species of the Melanidæ.* By ISAAC LEA, LL.D., President Acad. Nat. Sci., &c. vol. ix. With sixteen plates. Read before the Academy of Natural Sciences of Philadelphia, and published in its Journal.—We have again the pleasure of noticing a volume of Mr. Lea's works, which is perhaps superior in value and research to any of his former ones. This memoir forms the greater part of vol. v, of the new series of the Journal of the Academy of Natural Sciences, and is accompanied with sixteen quarto plates most admirably executed, and constitutes vol. ix of "*Observations on the genus Unio*, &c." The Unionidæ embrace descriptions of 28 new species, illustrated by ten plates, giving three views of each species. The remaining six plates embrace 229 new species of Melanidæ, the figures executed on stone with remarkable beauty and correctness. For the greater part of these Melanidæ Mr. Lea has proposed three new genera, *Trypanostoma*, *Goniobasis*, and *Strephobasis*, considering that they do not belong to the genus *Melania*, as described by Cuvier and Lamarck. Mr. Lea has entered into a full discussion of these new genera in the work before us, and he finds them sustained by the anatomy of the soft parts. He also gives the following brief analysis of the genera of Melanidæ.

Melania having a regular loop-form aperture.

Anculosa having a rounded aperture and a callous columella.

Io having a greater or lesser elongate channel or spout at the base.

Lithasia having a callus on the columella above and below, and a notch at the base.

Schizostoma having a cut in the upper part of the outer lip.

Strephobasis having a retrorse callus at base, and usually a nearly square aperture.

Trypanostoma having an expanded outer lip, and an auger-shaped aperture.

Goniobasis having usually a sub-rhomboidal aperture, sub-angular at base and without a channel.

Amnicola having a round mouth and no callus.

The number of new species of Unionidæ which Mr. Lea has described, since he commenced the study of this family in 1827, already amounts to over 550, nearly all of which are indigenous. Full descriptions, accompanied by excellent figures, have been given of each, and, when the soft parts could be obtained, descriptions of these have been added, and, to some extent, drawings of their anatomy. A great advance has been made by describing and illustrating the embryonic forms, by which many of the species are shown to differ more than in the adult or mature state. This is a branch of study which few European Malacologists have attempted.

According to Mr. Lea, we have now, in the United States, 633 known species of the family Unionidæ, divided as follows: *Unio* 542, *Margaritana* 31, *Anodonta* 60. To these may be added, for the remaining part of North America, as far as yet known, 41 species, making together 674 species of this family. Mr. Lea has many other new species undescribed, and vol. x of "Observations" is in an advanced stage of preparation.

The figures of the Melanidæ are among the best figures of shells ever published, and they do credit to our American artists. Alabama seems to be the geographical center of this family, or the region having the greatest number of species and individuals. Other new genera yet may be required; and when so, it is important that they should be based, as far as possible, on characters derived from the structure of the animals, all others being liable to lead to error. Only careful study can teach what modifications of form depend upon fundamental points in the animal structure. We are pleased to see dimensions given with so much care: this shortens the labors of those looking up species, especially where there are no figures. Mr. Lea would do a great service to science by preparing a paper on the species found in all the great river basins, both Unionidæ and Melanidæ, with a view to the zoological regions.

It has been stated by a recent writer in Paris, in noticing the large number of species described by Mr. Lea, that probably not a tenth of them would prove to be distinct. It may well surprise a European naturalist that the species should be so numerous in America. But the remark is a betrayal of ignorance of the subject, and also of those hygrometric conditions of the continents of Europe and America in which they so widely differ, and which are the basis of great zoological distinctions. A view of the finely executed figures should alone satisfy a conchologist of judgment, that the various forms, so remarkable in our Western and Southern rivers, could not fail to prove to be, with few exceptions, at least, distinct species. It is quite possible that there have been some duplications, for this could scarcely be otherwise where specimens are rare; and especially in the earlier descriptions, before the difference of form in the two sexes, a fact unappreciated by European naturalists, had been observed.

Science is greatly indebted to Mr. Lea, for his untiring labors through so many years in this department of Natural History. H.

2. *Researches upon the Anatomy and Physiology of Respiration in the Chelonia;* by S. Weir Mitchill, M.D., and George R. Morehouse, M.D. (*Smithsonian Contributions to Knowledge*, xiii, 169.)—This memoir is one of great interest, not only on account of the admirable manner in which the results have been worked out, and the valuable contribution which they are to physiology, but as showing how the labors of a careful and truthful observer may pass for more than a half of a century neglected, until, falling under the notice of those who know how to appreciate them, the place which is their due is claimed for them in the history of science.

The authors have made a thorough demonstration of the manner in which the respiratory movements are executed in turtles, and have shown that, with one exception, all writers on the subject from Malpighi to Agassiz, including no less authorities than Cuvier, Johannes Muller, and Milne-Edwards, have fallen into error. The mechanism of breathing in these animals, as described by naturalists, has been supposed to be as follows: by the depression of the hyoid apparatus and tongue, air is drawn into the mouth through the nostrils; these are then closed, and, by the raising of the hyoid, air is driven from the mouth through the glottis and trachea into the lungs, when inspiration is completed; expiration is effected by the contraction of the abdominal muscles, and the consequent compression of the lungs. This is all wrong.

The authors of the memoir have proved that the hyoid apparatus has nothing whatever to do with ordinary breathing, and that the movements of respiration are really effected in the following manner: inspiration by the abdominal muscles, which, when in repose, form a deep concavity in either flank behind the sternum, those on each side somewhat resembling a diaphragm in shape; as they contract, they become flattened, descend, draw down the viscera, and in so doing enlarge the cavity of the trunk, which enlargement is followed by a rush of air through the trachea into the lungs, when inspiration is completed. Expiration is produced by the action of a peculiar muscle, which the authors have for the first time completely described, and which previous writers have for the most part overlooked, or have attached but little or no importance to it. This has by some been doubtfully considered as the homologue of the diaphragm, and may be described as a broad digastric muscle, one of the bellies arising from nearly the whole breadth of the fore, the other from the hind part of the shield and from the pelvis, and the two united by a broad tendon across the middle of the abdominal cavity; between this muscle in front and the shield behind are included the viscera. By the contraction of it, and the consequent compression of the lungs, the movement of expiration is effected. We have taken pains to test this explanation on a living animal, and have constantly found that the flanks descended and were flattened during inspiration, and ascended and became more concave during expiration, the hyoid apparatus all the while being motionless. Thus the comparison which, since the days of Malpighi, has been made between the respiration of frogs and turtles, proves to be unfounded,—and the abdominal muscles, which in other air-breathing vertebrates are expiratory, become inspiratory in the turtles, while the presumed homologue of the diaphragm is the true muscle of expiration.

The authors had reached the conclusion set forth in their memoir, when, on looking into the literature of the subject, they found that in some respects they had been anticipated, as appears by reference to a *Dissertation on the Respiration of a Tortoise, by Robert Townson, LL.D.*, written at Gottingen, May, 1795. This the authors have reprinted in full, "as an act of justice; for, having conducted our enquiry with a full knowledge of the opinion of modern authorities, we were surprised, on afterwards learning the singularly truthful views of Townson, to find that they had fallen unappreciated, and that in many instances they had not even been honored by a notice, or when noticed had been mentioned only to be condemned."

In the dissertation above referred to, Townson fairly demonstrated the action of the abdominal muscles, and fairly recognized their true use, his conclusions being based upon a series of well devised experiments. He however had but an imperfect knowledge of the compressor muscle, being ignorant of the anterior portion of it, and supposing that pressure was applied only to the hinder lobes of the lungs instead of the whole of them. Townson's explanation, which has been not only for the most part neglected, but criticised by Cuvier as erroneous, has at length been raised from obscurity, and proved, as far as it goes, in all respects true, and will hereafter receive the credit which it so richly deserves.

In addition to the above mentioned results, the memoir contains a very important, and, so far as we are informed, an entirely new, investigation of the physiology of the nerves governing the movements of the glottis. We should pass beyond the limits of a merely bibliographical notice if we entered upon the anatomical and experimental details on which it rests, and will simply add the following summary of them by the authors, so far as they relate to the nerves of the larynx.

1st. In Chelonians, the superior laryngeal nerve is distributed both to the opening and closing muscles of the glottis.

2d. The inferior laryngeal nerve is distributed solely to the opening muscle of the glottis.

3d. A true chiasm exists between the two superior laryngeal nerves.

This last proposition covers a curious and hitherto undescribed distribution of the laryngeal nerves, viz: a complete decussation or intermingling, by means of the chiasm, of filaments from opposite sides, in a manner analogous to those of the optic nerve; so that the muscles of the right half of the larynx not only get fibres of the superior laryngeal nerve of the same side, but, as it appears, in about an equal proportion from the opposite nerve. The results obtained in experimenting upon this nerve will be readily explained by the anatomical facts just mentioned. After dividing the lower or recurrent laryngeal nerves, the motions of the glottis continue, since the upper nerve supplies both opening and closing muscles; if in addition the upper nerve of one side is divided, they still continue, because filaments of the undivided nerve not only pass to the muscles of the same side, but, by means of the chiasm, to those of the opposite; if the upper nerves of both sides are divided, the paralysis is complete.

The memoir contains a large number of anatomical details and of physiological experiments of great interest, which we must leave unnoticed: we will only add that the investigations have been conducted throughout with great care and skill, showing that the authors are adepts in the only school which is likely to enlarge the boundaries of physiological knowledge, viz: the school of observation and experiment.

J. W.

V. ASTRONOMY AND METEOROLOGY.

1. *Discovery of Asteroid* (78).—This planet was discovered by Dr. R. Luther, at Bilk, March 15, 1863. It appeared as a star of the 10th magnitude, and has received the name of Diana. The following elements have been computed from observations of March 15, 21, 25, and 31.

Epoch, 1863, March 31·5, Berlin m. t.

M	=	65° 44′ 54″·1
π	=	92 16 9 ·3
☊	=	339 10 12 ·9
i	=	11 28 56 ·4
φ	=	13 12 13 ·0
μ	=	772″·860
log. a	=	0·441271

2. *Comet II*, 1863.—On the 12th of April, M. Klinkerfues discovered a new comet in R. A. 309° and Dec. 3° S. It was discovered independently by M. Donati at Florence on the 15th. On the 19th of May, this comet was only ten degrees distant from the North pole, presenting the appearance of a round nebulosity 5 or 6 minutes in diameter. The following elements have been computed by M. Loewy.

Perihelion passage, 1863, April 4·89477, Greenwich m. t.

Longitude of perihelion,	255° 15′ 17″ mean eq. 1863·0
Longitude of node,	251 15 51
Inclination,	112 37 57
Log. perihelion distance,	0·026080

3. *Comet III*, 1863.—This comet was discovered by M. Respighi at Bologna, April 13th, near β Pegasi. The nucleus had the brightness of a star of the sixth magnitude, and the tail extended 40 minutes of arc. The same comet was also discovered about simultaneously by M. Becker, near Berlin, and by M. Tempel at Marseilles. On the 25th of April its tail was 2 degrees in length. M. Valz has communicated the following elements deduced from the observations of M. Tempel.

Perihelion passage, April 22·089, Marseilles m. t.

Longitude of perihelion,	298° 58′
Longitude of node,	243 29
Inclination,	85 27
Perihelion distance,	0·6144
	Motion direct.

4. *Observations of the Zodiacal Light;* by STILLMAN MASTERMAN.—The *zodiacal light* is one of the most difficult to observe, in our latitudes, of the celestial phenomena. Besides the unfavorable position or invisibility in the twilight, rendering it unobservable for a considerable portion

of the year, the moon and even the larger planets completely eclipse its mild light much of the time when it might otherwise be seen.

Since the commencement of 1859, I have been able to obtain a few observations of this body, mostly during the winter of 1861, which have been confined principally to the determination of the position of the *apex* of the luminous cone in the celestial sphere. My method of observation is as follows:—By means of a good star chart, I determine, as nearly as practicable, the *right ascension* and *declination* of the *vertex* or extremity of the axis of the cone; these coördinates being then easily convertible into those of *longitude* and *latitude*, thus giving the position referred to the ecliptic.

I have devoted very little attention to the physical phenomena of this body. Perhaps the phenomenon of rapid variations in brightness, observed more than once, may not be without interest. These are not wave-like pulsations like those observed in the aurora borealis; but the alternate brightenings and dimmings of the whole area of the light simultaneously. Sometimes a sudden brightening or dimming is observed without any other change following for a number of minutes. The latter was particularly observable on Jan. 9, last.

In the annexed small table of observations, the column headed $\lambda - \odot$ contains the difference between the observed longitude of the apex of the light, and that of the sun taken from an ephemeris; in other words, the angular distance of the vertex from the solar centre. That headed β shows the observed latitude of the vertex, north latitudes being regarded as positive.

Date.	$\lambda - \odot$	β
h	°	° ′
1859, Jan. 23, —	104·8	+ 2 49
26, 8·2	103·8	+ 3 17
31, 7·5	101·7	+ 3 31
1861, Jan. 4, 7·5	86·5	+ 0 38
9, 7·5	84·4	+ 2 38
Feb. 1, 7·2	72·9	− 2 14
3, 8·0	78·1	− 2 47
4, 8·5	74·8	− 3 3
Mar. 6, 8·0	70·8	+ 3 33
7, 8·5	79·7	+ 1 31
28, 8·5	72·4	− 2 8
Apr. 7, 8·5	71·9	− 0 28
1862, Dec. 11, 7·0	71·4	+ 3 17
1863, Jan. 9, 6·8	86·8	+ 3 7
18, 8·3	90·5	− 0 8

Weld, Franklin Co., Maine, Apr. 15, 1863.

5. *Results of Observations of Variable Stars at Weld, Franklin Co., Maine;* by Stillman Masterman.—I send you the results of my observations of variable stars since the commencement of the year. I had formed a design of observing all of those variables visible to the naked eye, also such as require only a small optical power for that purpose; but a long-continued illness, from which I now find myself far from being exempt, has limited me to those determinations annexed.

β Persei.

Minimum, 1863, Feb. 2, $7^h\ 24^m\ 48^s$, Wash. M. T.—Wt., ½.

λ Tauri.

Minimum, 1863, Jan. 18, $10^h\ 23^m$, Wash. M. T.—Wt., 1.

δ Cephei.

Minimum, 1863, Jan. 8,	$11^h{\cdot}6$,—	Weld, M. T.—Wt.,	3.
19,	7 ·1,—	"	4.
Feb. 4,	8 ·5,—	"	4.

ζ Geminorum.

Minimum.			*Maximum.*		
1863, Mar. 23,	0^h—Wt.,	1.	1863, Feb. 25,	7^h—Wt.,	3.
Apr. 1,	15 "	2.	Mar. 17,	16 "	4.
12,	1 "	3.			
22,	6 "	4.			
May 1,	7 "	2.			

β Lyræ.

Principal Minimum.	*Maximum.*
1863, Jan. 1, $6^h{\cdot}3$—Wt., 4.	1863, Jan. 17, $5^h{\cdot}3$—Wt., 1.

Second Minimum.

1863, Jan. 19. $22^h{\cdot}8$—Wt., 1.

α Herculis.

Observations discontinued on this star, Feb. 15.

Approximate time of Maximum, 1863, Feb. 6.

Weld, May 30, 1863.

6. *Evidence of the cosmical origin of shooting stars derived from the dates of early star-showers.*—Mr. Quetelet, in his *Physique du Globe*, devotes a chapter to shooting stars. Doubts seem to have arisen in the mind of the distinguished author respecting the origin of these phenomena. Yet from this same chapter can be drawn a simple, and, as we think, a very strong argument, that the star-showers, at least, are caused by the entrance into our atmosphere of bodies revolving about the sun. And if this be admitted for the shooting stars of the annual periods, probably no one will deny that the sporadic meteors have a similar character and origin.

The return of the August and other showers on fixed days of the year might *possibly* be due to meteorological changes. But if the magnetism, the heat, the electricity, or the other properties of the atmosphere produce these *annual* phenomena, the period should evidently be the *tropical* year. On the other hand, if rings of bodies revolving about the sun are met by the earth in April, August, November, &c., thus causing these showers the cycle should be the *sidereal* year, or rather, should not be the tropical year. The nodes of the rings move along the ecliptic, but at a rate different from the precession of the equinoxes. The dates of the earlier showers show quite clearly that the true period is not widely different from the sidereal year.

In the following tables are given the historic dates of star-showers from

Quetelet's list, in some cases changed to agree with the authority cited. They are expressed in the Gregorian calendar, and therefore represent approximately corresponding dates of the tropical year. The later dates are generally omitted. To express these dates in a sidereal year there is given at the same time the *corresponding* day (and fraction of a day) of 1850; that is, the time when the earth's longitude in her orbit, measured from a fixed equinox, was the same as on the day of the shower. The following formula is used in the computation.

Let x be the number of days to be added to the recorded date expressed in the Gregorian calendar, t the given year of the Christian era, n the number of leap years between the given date and A.D. 1850, and l the length in days of the sidereal year. Then, evidently,

$$(1850-t)\,l=x+365\,(1850-t)+\mathrm{N}.$$

To reduce this to a form better suited for computation, observe that N is equal to the integral part of $\frac{1}{4}(1851-t)$, minus 12, plus the correction between the Gregorian and Julian calendars for the given date. Let c be this correction, ε be the remainder after dividing $1851-t$ by 4, $l=365^{\mathrm{d}}\cdot256374$, and we obtain, by reducing,

$$x=(1850-t)\times\cdot006374+\tfrac{1}{4}(\varepsilon-1)+12-c.$$

The integral part of $\frac{1899-t}{100}$, minus the integral part of $\frac{1999-t}{400}$, gives the value of $12-c$.

It will be observed that the secular variation in the value of l, the motion of the apsis of the earth's orbit, the diminution of its excentricity, and the periodic perturbations are neglected. The terms dependent on these would together rarely amount to one-tenth of a day. The equation of the center is therefore omitted. I have subtracted three-tenths from the Chinese dates, and added two-tenths to the American dates, for difference of longitude.

The noon of the historic date is taken when it is not stated whether the shower was in the morning or the evening. This involves an error not exceeding seven-tenths of a day. It should also be borne in mind that the shower in August (and probably those in other months) must be considered as continuing through more than a single day.

Particulars respecting the several showers are given in Quetelet's catalogue; also in Arago's *Astronomie Populaire*, iv, 292. In the following table, reference is made to the memoir of Ed. Biot, *Mémoires des Savans Etrangéres*, vol. x, Paris, 1848, to the paper of Chasles in the *Comptes Rendus*, xii, 499, and to Herrick's catalogue of star-showers, this Journal, [1], xl, 349. The dates reported by Biot are from the Chinese annals.

I. *The April shower.*—The following seem to belong to this period:[1]

B.C.	687, Mar. 16,	corr. to	A.D. 1850,	Apr.	19·9.	*Biot.*
	15, " 25,	"	"	"	19·6.	"
A.D.	582, " 31,	"	"	"	18·1.	*Chasles.*
	1093, Apr. 9·6,	"	"	"	20·7.	"
	1094, " 10,	"	"	"	20·8.	"

[1] Compare with these the following showers for which the exact date is not given:

A.D. 590, before Easter, or Apr. 4, corr. to Apr. 22·1. *Chasles.*
741, " " 13, " " 28·3. "

A.D.	1095, Apr. 9·6,	corr. to A.D. 1850,	Apr.	20·2.	*Herrick.*
	1096, " 10,	" "	"	21·3.	"
	1122, " 10·6,	" "	"	20·2.	"
	1123, " 11,	" "	"	20·4.	*Chasles.*
	1803, " 19·6,	" "	"	19·9.	*Herrick.*

II. *The August shower.*—To this belong the following dates:

A.D.	830, July 26,	corr. to A.D. 1850,	Aug.	9·2.	*Biot.*
	833, " 27,	" "	"	10·4.	"
	835, " 26,	" "	"	8·9.	"
	841, " 25,	" "	"	8·4.	"
	924, " 26–28,	" "	"	8·1–10·1.	"
	925, " 27, 28,	" "	"	8·8, 9·8.	"
	926, " 27,	" "	"	8·6.	"
	933, " 25–30,	" "	"	5·8–10·8.	"
	1243, Aug. 2,	" "	"	10·6.	*Herrick.*
	1451, " 5,	" "	"	10·0.	*Biot.*[2]

III. *The November shower.*—The following appear to be exhibitions of this shower:[3]

A.D.	585, Oct. 25,	corr. to A.D. 1850,	Nov.	12·3.	*Chasles.*
	902, " 29 or 30,	" " "	"	11·0 or 12·0.	*Herrick.*
	1582, Nov. 7,	" "	"	10·7.	*Wartmann.*
	1698, " 8·6,	" "	"	11·6.	"
	1799, " 11·6,	" "	"	12·9.	*Humboldt.*
	1833, " 12·7,	" "	"	13·3.	*Olmsted.*

IV. *The December periods.*—There appear to be two epochs in December, each marking a distinct shower, viz: Dec. 6th–7th, and Dec. 12th. There is no early date corresponding to the first epoch. The following belong to the second:

A.D.	901, Nov. 30,	corr. to A.D. 1850,	Dec.	13·3.	*Herrick.*
	930, " 29,	" "	"	11·6.	*Biot.*
	1571, Dec. 8,	" "	"	11·5.	*Wartmann.*

The remaining early dates of Quetelet's catalogue are arranged here for comparison with those already given. A few, which do not appear to indicate star-showers, are omitted. Many of those retained doubtless refer to auroras, or to moderate exhibitions of shooting stars.

January.

A.D.	599–600, Dec. 28,	corr. to A.D. 1850,	Jan.	16·2.	*Biot.*
	745, Jan. 5,	" "	"	20·2.	*Perrey.*
	765, " 3, 5,	" "	"	18·2, 20·2.	*Biot* and *Perrey.*
	848–849, Dec. 31,	" "	"	14·1.	*Chasles.*
	1118–1119, " 27,	" "	"	5·4.	"

February.

A.D.	308, Jan. 20,	corr. to A.D. 1850,	Feb.	9·0.	*Biot.*
	913, Feb. 7,	" "	"	20·2.	*Chasles.*
	918, " 6,	" "	"	18·9.	"
	919, " 6,	" "	"	18·6.	"
	1106, " 19,	" "	"	28·7.	*Herrick.*

[2] Biot gives July 27th, O. S., or Aug. 7th, N. S., which are not consistent. As the other dates are Old Style the former is presumed to be the correct day.

[3] A great shower occurred in the beginning of the year of the Hegira 596. This year began A.D. 1199, Oct. 30, which corresponds to Nov. 8·4.—*Herrick's Catalogue*, No. 28.

March.

A.D.	36, Feb. 7,	corr. to A.D. 1850,	Mar. 2·1.	*Biot.*
	807, Mar. 2,	" "	" 16·4.	*Chasles.*
	842, " 5,	" "	" 19·4.	"
	842, " 17,	" "	" 31·4.	"
	861, " 14,	" "	" 28·6.	"
	937, Feb. 19,	" "	" 4·1.	"
	1584, " 28,	" "	" 3·2.	*Wartmann.*

April.

A.D.	401, Apr. 9,	corr. to A.D. 1850,	Apr. 29·2.	*Biot.*
	538, " 6,	" "	" 24·4.	*Chasles.*
	839, Mar. 29,	" "	" 12·2.	"
	839, Apr. 17,	" "	" 30·9.	*Biot.*
	840, " 1,	" "	" 15·9.	*Chasles.*
	927, " 17,	" "	" 29·3.	*Biot.*
	934, " 18,	" "	" 30·8.	"
	1000, " 4,	" "	" 15·9.	*Chasles.*
	1008, " 2,	" "	" 13·6.	*Biot.*
	1009, " 16,	" "	" 27·6.	*Chasles.*

May.

A.D.	839, May 12,	corr. to A.D. 1850,	May 26·2.	*Chasles.*
	842, " 5,	" "	" 19·4.	"
	954, " 11,	" "	" 23·7.	"
	965, " 17,	" "	" 29·7.	"
	1158, " 3,	" "	" 12·4.	"

No shower in June.

July.

A.D.	36, June 25,	corr. to A.D. 1850,	July 20·8.	*Biot.*
	784, July 14,	" "	" 29·0·	"
	1022, June 28–30,	" "	" 9·3–11·3.	*Chasles.*

August.

A.D.	714, July 19,	corr. to A.D. 1850,	Aug. 2·9.	*Biot.*
	865, Aug. 5,	" "	" 19·3.	"

September.

A.D.	532, Aug. 30,	corr. to A.D. 1850,	Sept. 17·6.	*Biot.*
	1012, Sept. 17,	" "	" 28·5.	"
	1037, Aug. 27,	" "	" 7·1.	"
	1063, " 28,	" "	" 7·5.	"

October.

A.D.	288, Sept. 26,	corr. to A.D. 1850,	Oct. 18·2.	*Biot.*
	585, " 25,	" "	" 13·0.	"
	931, Oct. 19,	" "	" 31·3.	"
	934, " 19,	" "	" 31·5.	*Chasles.*
	945, Sept. 20,	" "	" 3·0.	"
	1002, Oct. 20,	" "	" 31·1.	*Biot.*
	1436, " 11,	" "	" 16·8.	"
	1439, " 14,	" "	" 19·1.	"
	1743, " 15,	" "	" 16·4.	*Herrick.*
	1798, " 14·5,	" "	" 15·8.	*Brandes.*

November.

A.D. 855, Oct. 21,	corr. to A.D. 1850,	Nov.	4·1.	*Chasles.*
856, " 21,	"	"	"	4·8.	"
970, Nov. 8,	"	"	"	20·3.	*Biot.*
979, " 2,	"	"	"	14·3.	*Chasles.*
1058, " 7,	"	"	"	18·0.	"
1101, Oct. 24,	"	"	"	3·0.	*Perrey.*
1202, " 26,	"	"	"	4·1.	*Herrick.*
1366, " 29·5,	"	"	"	5·6.	"
1533, Nov. 3,	"	"	"	7·0.	*Biot.*

December.

A.D. 848, Dec. 1,	corr. to A.D. 1850,	Dec.	15·9.	*Chasles.*
899, Nov. 18,	"	"	"	1·8.	*Herrick.*
1565, Dec. 13,	"	"	"	17·1.[4]	This Jour., [2], xxxv, 461.

The following dates are indicated by this table as deserving the attention of observers, viz: Jan. 15–19, Feb. 19, Mar. 1–4, Apr. 28–30, Oct. 16–18, and Oct. 31 to Nov. 6. During this last period have occurred several of the most remarkable showers on record. By giving to the nodes of the November ring a procession of one day in 70 years most of these would be brought into the November period.

For five of the dates given above, the day of the month depends upon the time of Easter. An error of the year, which might easily be made, would change the date. If for 538, 840, 1000, 1009, and 1158 we could read 536, 842, 1002, 1010, and 1160, the dates of the showers would correspond respectively to April 18·9, 20·4, 20·4, 19·4, and 18·9.

Yale College, June 16, 1863. H. A. Newton.

7. *The meteoric iron from Newstead.*—In the year 1827, whilst digging a cellar in the village of Newstead, Roxburghshire, Scotland, sticking in the clay at a depth of from 3 to 4 feet, the second and largest mass of meteoric iron yet found in Great Britain was discovered. Although its true nature had been overlooked, it has been preserved as a curiosity during 35 years, until it attracted the attention of Dr. John Alex. Smith, who read a paper on it at the meeting of the Royal Physical Society of Edinburgh, (April 23d, 1862) from which we learn the following particulars:

Its external surface is rough and irregular; its shape principally consisting of a large rounded and lobulated mass, which tapers rapidly at one end to a four-sided pyramidal extremity, and terminates in an obliquely truncated point. For a more detailed description it may be divided into two portions: the larger rounded extremity, and the smaller, flattened, smoother on its surface, and tapering to a blunt point. The larger portion terminates behind in a broad blunt edge, and is formed by a clustering mass of rounded lobes irregularly grouped together, variable in size and in their greater or less projection from its surface. A deep furrow runs obliquely round the whole mass, and towards the pointed extremity rises a large round prominent lobe with a smaller one on one side and two more irregularly shaped projecting masses on the other. The smaller portion of the meteorite lies immediately in front of the prominent lobes just described, its rounded outlines rapidly changing into

[4] I assume that the date, Dec. 3d, is old style. If not, the corrected date should be Dec. 7·1, and this belongs to the first December shower.

four irregular smoother planes, which, meeting one another with two acute and two obtuse angles, form a somewhat pyramidal four-sided figure, tapering rapidly towards this end of the mass, and terminating in an irregular quadrilateral extremity.

In its greatest length it measures 10¾ inches, and in its widest part, about the middle of its length, 7 inches; its circumference round the larger extremity is 1 foot 3 inches, in its widest part, round the lobular projections, 1 foot 8½ inches, while within 1½ inches of the point its circumference is only 9½ inches.

It weighed 32lbs., 11 ounces and 1½ drachms avoirdupois.

Its surface has a dark reddish brown, in some parts a blackish, color; the lobulated parts show here and there, especially in the furrows, spots of a brighter red color. For the purpose of preserving the original shape, plaster casts were taken before it was cut up.

It was cut longitudinally into two portions, and one of them again into smaller slices.

It was found to be entirely free from any foreign admixtures, such as olivine, etc., and of a bright white color, and solid, dense, and steel-like throughout. In attempting to file off a portion for analysis, the filings were black, and showed little of the metallic appearance of pure iron, the hands becoming much blackened, as if plumbago was mixed with the iron. It was extremely hard and tough, but the mass varied in its resistance to the file and a graving tool. The lobed and rounded portion resembled cast iron, but was harder than untempered steel of the best quality, though not so hard as the prepared steelplate of the engraver. The inner portions were softer and more open in texture than that next to its outer surface; the prism-like point was tougher and more like hammered iron. The mass was apparently not malleable but brittle. Etching by a mixture of nitric and acetic acids showed upon the surfaces the rough irregular projecting lines of the crystalline structure of the mass, which had been but slightly acted upon, the dark spaces and lines showing where the acids had the greatest effect. A small patch near the middle of the rounded or lobed portion etched with dilute nitric acid has a similar appearance, but displays more distinctly the characteristic and frosted-like lines of crystallization crossing each other at various angles. These lines are very fine and minute in texture, and the meteorite resembles in structure that of hazelnut size found many years ago at Leadhills, and described by R. P. Greg, Esq.

Dr. Murray Thomson found the spec. grav. of different portions, 6·1919, 6·499 and 6·7400; that of the pyramidal portion 6·750; that of the lobed portion 6·350, that of the whole mass =6·517. The composition of the meteorite is according to Dr. M. Thomson:

Iron, - - -	93·51
Nickel, - - -	4·86
Silica, - - -	0·91
Carbon, - - -	0·59
	98·87

8. *The meteoric iron from Sarepta.*—Director Wm. Haidinger made at the meeting of July 24th, 1862, of the Imperial Academy of Vienna, some interesting observations on the meteoric iron from Sarepta. His paper is accompanied by two plates, one showing the peculiar appearance of the

Sarepta iron in three positions, the other representing prints from a galvanoplastic copperplate, prepared from the etched slices of Sarepta and Arva iron, showing their structure; together with two prints from the etched plates themselves.

Although it is very difficult to form without these illustrations a correct idea of the appearance and structure of this meteorite, we will give the following abstracts:

It was found in 1854 on the right bank of the river Volga in the steppes of the Kalmucs, 30 miles (German) from Sarepta in the district of Zarizin, Govt. Saratow in Russia. Its original weight was 32lbs. 58 zolotnik, =31·58 lbs. avoirdupois or 14325 grs. The first notice of it was given at the meeting of Nov. 18th, 1854, of the Imp. Soc. of Naturalists of Moscow, by Dr. Auerbach, who exhibited it for Constantine Glitsch of Sarepta, at whose direction plaster casts were made of the mass, the original however being cut to pieces for distribution.

It is a compact iron mass, pretty rich in nickel, rounded at the edges, and entirely free from olivine or any other foreign substances. The Sarepta meteorite is one of the most remarkable and peculiar known; a most characteristic difference, distinguishing its two sides, can readily be perceived. The front side has the form of a gently sloping arch, similar to a spherical surface, the radius of which is about 9½ inches. The roundish depressions upon it are only from 1 to 3 lines in depth, their diameter being from 1 to 1½ inches; the back part, on the contrary, is full of rounded indentations, excavated to a depth of 1½ inches, the formation of which can be easily imagined to have been produced by the melting off by pointed flames uniting backwards. The model does not show well the characteristic sharply turned up ridges produced by the melting of the crust.

From all these data there seems to be no doubt that the position here suggested was really that which the meteorite had during the cosmical part of its path. The centre of gravity lies certainly nearer to the flat spherical convexity than inside of the ruggedly juxtaposed cones and excavations, notwithstanding the flat mass which projects here with its broad plane. The stone of Stannern, which Haidinger described a short time ago (*this Journal*, [2], xxxii, 138), has a similar disc-like shape; but the large mass from Agram of a flat disc-like shape, would answer still better for comparison, the two sides of which are so different, that with the greatest probability we might infer that it had a rotation in its plane round the axis of its orbit.

The Sarepta iron does not appear to have been very long exposed to the atmospheric influences, its surface being hardly acted upon by rust. It presents in its structure many analogies with that of Arva. The schreibersite in the figures upon the plates of the Sarepta iron does not show any interruption in its direction, although it does not penetrate them uniformly. The meteoric iron immediately adjoining the schreibersite shows distinctly fine characteristic striæ and hatchings, most plainly in the darkest portions, and through them a damasked appearance, if examined with a magnifier. The different parts on account of the different reflexion of light appear paler or darker grey, some even quite dark and without lustre, but, cut in a different direction just these portions would present more lustre and damask, and others which are now the most brilliant would show it less. Yet these padded particles which produce such different impressions do not appear to be of different nature, their position only in the compound causes the different appearance. The Sarepta plate consists, in one of its corners, of meteoric iron only without any schreibersite, but of two peculiar varieties; one is pale gray, smooth, brilliant and full of striæ and hatchings, intersecting at right and oblique angles, nearly as a square and its two diagonals; the other is quite dull, dark gray, more granular in texture and abruptly separated from the other. Held towards the light in very oblique angles, it shows a paler gray color, and a little lustre caused by the damask. Scattered through the dull dark mass are very minute brighter shining particles.

The Arva plate shows the intersections of the brilliant schreibersite in three directions at angles of 60 and 120°, the iron being dull and dark gray without striæ; in one corner there is some graphite, inclosing a roundish parcel of sulphid of iron. The plates from Arva iron, however, present great differences amongst themselves. F. A. Gth.

9. *Meteoric Iron from Tucson, Arizona.*—A mass of meteoric iron from Tucson has been presented to the city of San Francisco by General Carleton. In a recent letter, Prof. Whitney states that this iron is 4 feet 1 inch long, and weighs 632 lbs. It was found at or near Tucson, Arizona, by Gen. Carleton's California column on their march through that region, and has evidently been used for an anvil, although it is not the one figured by Bartlett as having served that purpose.

A specimen of this meteorite was sent to Prof. Brush, of Yale College, for chemical investigation, and we condense the following account of this examination from a letter addressed to Prof. Whitney and published in vol. iii. of the *Proceedings of the California Academy of Natural Sciences:* "An inspection of the specimen with a lens showed it to be dotted with little cavities which, on the fresh fracture, were lined with a white silicious mineral, giving the surface a porphyritic, or pseudo-porphyritic, appearance. Its specific gravity is 7·29. When a fragment of it was placed in a solution of neutral sulphate of copper, it became quickly coated with metallic copper, proving the iron to be 'active.' Attacked with an acid, a portion of the iron was dissolved, leaving the silicious mineral projecting from the surface of the specimen; and, with a magnifier, black particles of schreibersite could be seen. After complete solution of the iron, a careful microscopic examination was made of the insoluble residue. With a magnifying power of 25 diameters, it appeared to consist chiefly of two substances: one a milk-white to transparent mineral, having a fused, rounded surface, occurring in little globules, or elongated, rounded particles; while the other constituent was black and angular, and attractable by the magnet. The first named substance, when observed with a magnifying power of 100 diameters, proved to contain minute specks of the black mineral disseminated through it; some of the silicious fragments were translucent and of a milk-white color, and others colorless and transparent; a large number, however, were transparent at one end, shading into milk-white at the other, thus seeming to indicate that the transparent and translucent portions were not two distinct minerals. A blowpipe examination of the silicious mineral showed it to have characters very much resembling *olivine.* The black mineral proved to be *schreibersite.* A minute trace of chromium was also observed in the insoluble residue.

The qualitative analysis of the portion soluble in nitric acid indicated the presence of iron, nickel, cobalt, copper, phosphorus, lime, and magnesia, with unweighable traces of chlorine, sulphur, and alumina. For the quantitative examination of the meteorite, a fragment weighing 4·3767 grammes was treated with nitro-chlorhydric acid (aqua regia), and after solution of the iron the whole was evaporated: on approaching dryness, gelatinous silica separated, showing that the silicate had been partially, at least, decomposed by the acid. After heating until the silica was rendered insoluble, it was repeatedly treated with acid and evaporated, so as to insure the oxydation of all the schreibersite, and finally the soluble part was taken up with chlorhydric acid, and on dilution separated by filtration from the silica and insoluble residue.

The *insoluble residue*, containing free silica and undecomposed silicate, was perfectly white and free from all traces of schreibersite. It weighed 0·1855 grm., equal to 4·24 per cent of the specimen analyzed. It was fused with carbonate of soda, and the silica and bases determined in the usual manner. It contained 0·159 grm. silica; 0·0054 protoxyd of iron, with a minute trace of alumina; 0·0028 lime, and 0·0168 magnesia.

The soluble and insoluble portions gave in the analysis the following percentage composition:

			Considering the silica to exist as olivine.
Iron	81·56		79·44
Nickel	9·17		9·17
Cobalt	0·44		0·44
Copper	0·08		0·08
Phosphorus	0·49		0·49
Silica	3·63	Combined with 2·73 Protoxyd of iron, making olivine..	10·07
Protoxyd of iron, with a trace of alumina	0·12		
Lime	1·16		
Magnesia	2·43		
Chlorine, Sulphur, Chromium	minute traces		traces.
	99·08		99·69

If the silica found in this analysis be considered to exist in combination with lime, magnesia, and iron, in the proportions to form olivine, it will be necessary to deduct 2·12 per cent from the amount of metallic iron (equal to 2·73 per cent of protoxyd of iron), in order to give the silicate the olivine formula ($3RO, SiO_3$). Admitting this to be the correct view, the mass analyzed contains 10·07 per cent of olivine, and by the addition of the oxygen of the protoxyd of iron the analysis adds up 99·69 instead of 99·08.

The composition of this meteorite corresponds very closely with another meteoric iron from Tucson, discovered by Mr. Bartlett, and described by Prof. J. Lawrence Smith, in the *Am. Journ. of Science*, 2d ser., vol. xix, page 161. Dr. Smith's analysis gives iron 85·54, nickel 8·55, cobalt 0·61, copper 0·03, phosphorus 0·12, chromic oxyd 0·21, magnesia 2·04, silica 3·02, alumina trace, =100·18. He considers it to correspond to nickeliferous iron 93·81, chrome iron 0·41, schreibersite 0·84, olivine 5·06 =100·18. By an evident inadvertence, Dr. Smith adds the magnesia and silica together, and gives the sum as olivine; these substances are obviously not in the proportions to form the silicate $3RO, SiO_3$, and if we consider the silicate to be olivine, we must reckon the excess of silica as combined with protoxyd of iron. To do this, we must deduct 2·83 from the amount of metallic iron (equal to 3·64 protoxyd of iron), necessary to be combined with the silica and magnesia to give the olivine formula. The amount of olivine contained in the Bartlett meteoric iron will then be 8·70 per cent. Thus the two masses of iron will be seen to agree very nearly in composition, the only trifling difference being, that Dr. Smith has determined quantitatively the small amount of chromium contained in the Bartlett meteorite, while I have found a little lime and traces of sulphur and chlorine in the specimen you sent to me. The specific gravity I have stated to be 7·29; this was taken on about 12·5 grammes of the iron, and probably is somewhat higher than the portion which I analyzed, as the two surfaces of the larger specimen had been rubbed down, and as thus a considerable portion of the exposed silicate would be mechanically removed, it would make the density correspondingly higher." G. J. B.

10. *Meteor of April 19th seen at Philadelphia.*—A brilliant meteor was seen at Philadelphia and vicinity, on Sunday evening, April 19th, at 10 minutes before 8 o'clock.

Its apparent size was rather less than that of the full moon, its brilliancy considerably greater, form globular, and direction of motion nearly from west to east, tending slightly southward.

It seems probable that its first appearance was over the eastern edge of Chester County, although the data are not sufficient to determine satisfactorily either the place of beginning or the velocity. It is very clearly proved, however, by comparing observations made at Philadelphia, West Town (4 miles east of West Chester), Wilmington and Odessa, Del., that it disappeared over the western part of Camden County, N. J., (probably about 5 miles north of Glassboro), at a height of between 12 and 20 miles.

The duration of visibility was estimated at from 3 to 6 seconds.

Both at Wilmington and West Town, it was followed, after an interval of about 3 minutes, by a noise like thunder. At the former place, a gentleman who did not see the meteor compared the sound to that of a cannon at Fort Delaware, ten miles distant.

VI. SCIENTIFIC CORRESPONDENCE.—*Observations on Stellar Spectra.*

EDITORS OF THE AM. JOURNAL, &c.

Gentlemen: During the past year several European astronomers have been occupied with the observation of stellar spectra. I have now before me the results obtained by Donati,[1] Airy,[2] and Secchi.[3] I will at present confine myself to a short notice of the forms of instruments used by each as described in their published notices. Donati used a large burning glass, fifteen inches aperture and sixty-two inches focal distance, not achromatised, mounted equatorially, the cone of light passed through a fine slit before reaching the focal point; after crossing at the focus it traversed a cylindrical lens, thence, after being rendered parallel by an achromatic lens, fell upon a flint glass prism of about 61°, emerging from

[1] *Monthly Notices*, January 9, 1863. [2] Ib., April 10, 1863. [3] *Astr. Nach.*, 1405.

which it was received by a small achromatic objective, and observed with an eye-piece magnifying twelve times, in the focus of which was placed a bar movable by a micrometer screw.

From my experience in the observations of stellar spectra, I should note several defects in this arrangement. 1st. The absence of achromatism in the great condenser, in consequence of which but a small portion of the spectrum can at any time be brought to an approximate focus, and fine definition of the lines nowhere obtained.

2d. Too much power in the observing telescope, the objections to which are manifest.

3d. The uncertainty of making a contact of the micrometer bar with the lines, there being no illumination.

4th. The want of a check, such as the presence of a flame line in the field of view, to insure the detection of such small displacements of the spectrum as even a fine slit will permit.

The results of these instrumental defects will I think be seen when we hereafter examine the observations.

The Astronomer Royal describes the form of spectroscope used at Greenwich, where it is attached to the eye-tube of the great equatorial of twelve inches aperture. He says "The pencil of light from the object glass, which has converged to form the image of the star, then diverges, and falls in a wide and divergent state upon the prism; after emergence it is received on a combination of lenses which causes the pencils for the different colors to converge."

The image is observed with a micrometer, the field being illuminated by an annular reflector. In this form neither slit nor cylindrical lens is used, but breadth is given to the spectrum of the star by the creation of aberrations in two ways; first, by placing the prism in a position not that of least deviation, and second, by the uncorrected state of the "combination of lenses" through which the light reaches the micrometer after traversing the prism.

Reference is made to the lines of the solar spectrum in the following manner: A diaphragm, pierced by a hole $\frac{1}{510}$ of an inch in diameter, is placed in the focus of the great objective; when turned upon the sun, this hole represents the image of a "star of solar matter," and its lines are measured by the micrometer; an eye-piece containing a wire of reference is inserted with a reflector in a lateral tube and the position of the small hole observed. At night, the diaphragm is removed, and the image of the star is made to occupy the same position when seen in the lateral eye-piece occupied by the sun-illuminated hole, the eye-piece and reflector are then removed, and the observation made by the micrometer upon the star striæ.

Mr. Airy, in his description, speaks of this form of spectroscope as experimental, and expresses some doubts of its ability to define with great sharpness. This defect must necessarily result from the construction of the instrument, since the lines are only rendered visible by the existence of aberrations, which are destructive of fine definition. The spectrum not being confined during the observation to any certain part of the field of view, by a slit or other check, the truth of the measures depends entirely upon the exact running of the equatorial driving clock, which is not to be trusted. The illumination of the field must necessarily obliterate the extremities of faint spectra, and render the observation of many lines impossible.

Secchi's spectroscope is on the whole much better than either of the former. The light of the great Roman equatorial passes through a slit at the focal point, then traverses a cylindrical lens, then is rendered parallel by a lens, and then falls upon the prism, which is a compound structure composed of four flint glass prisms of 90° cemented to five crown glass prisms of the same angle, and so arranged that the axis of the spectrum or central green ray is a prolongation of the incident pencil: the amount of dispersion obtained by this form is the excess of the four flint prisms over the five crown, and with ordinary materials will amount to about the dispersion of two 65° prisms of flint glass. This dispersion is so great that the lines in the stellar spectra are seen without an observing telescope. They are referred for measurement to an illuminated scale reflected from the last surface of the prism. This scale is made by fine perforations in a metallic plate. The instrument is provided with a reflector for the purpose of comparing the star lines with those of a flame.

The only objection I can see to this form of instrument is the great thickness of glass in the prisms, the consequent absorption of light, and the increased danger of heterogeneity of material. Its great advantages are that it reaches the separation of the lines by the dispersion of the prisms rather than by the power of an observing telescope, and that it possesses the means of constant reference to a standard flame to check any deviation in the place of the spectrum.

The advantage first named is much greater than would at first be supposed, as I have recently proved by the substitution of a bisulphid of carbon prism for the flint glass formerly used in my star spectroscope. With an eye-piece of one half of the power formerly used, I have now a spectrum longer than before with more than double the intensity of light, and a consequent revelation of striæ not before seen.

I have perhaps trespassed too much upon your space in this note upon the instrumental agency so far brought to bear upon the spectral analysis of the stars. But these investigations are yet in the cradle, and if, as is predicted, they are destined to assume a great importance in the study of the constitution of the universe, it will not be amiss to point out to those about to embark in the new field of labor, the advantages and defects of the star spectroscope as it at present stands.

In the April No. of the *Monthly Notices*, Mr. Glaisher describes observations on the length of the spectrum obtained from the sky at different altitudes, made by him during his balloon ascension on the 31st of March last.

At the surface he saw the spectrum from B to beyond G, and all the principal lines. The length of the spectrum gradually decreased until at the altitude of 4½ miles he had no spectrum. He says, "The shortening of the spectrum with increase of elevation may have been, and most likely was, owing to want of light (although to my senses there was abundance); the sky at the time was of a deep dark blue, the sun was low and it is possible the light was insufficient."

The shortening of the spectrum observed by Mr. Glaisher is without doubt due to the diminution of light and to the monochromatic nature of the blue sky, which at that great altitude held but little vapor in suspension capable of reflecting white light. It is to be regretted that he did not make a series of observations upon the spectrum obtained directly from the sun. Such a series would have been of great value in determining the agency of our atmosphere in producing lines in the spectrum; and it is very necessary for the precision of astronomical chemistry to determine accurately which lines are telluric, and which are attributable to a celestial origin. You will remember that in my note to you of April last, contained in the May No. of this Journal, I sent you a diagram of the nine lines of which I found the solar D to be composed. I have since that date satisfied myself that of this group four only are truly solar lines and five telluric. Of this nature are the three faint lines on the red side of Kirchhoff's central line and the two faint ones next adjoining it on the green side. My proof is that these lines, although difficult objects at noonday with a battery of eleven prisms, are seen with ease near sunset with two. The whole of the yellow region of the spectrum is crowded with telluric lines, and it is most desirable that they should be accurately known. I am very respectfully yours, &c.,

LEWIS M. RUTHERFURD.

New York, June 8, 1863.

VII. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *On a System of Mounting Insects for the Microscope;* by HENRY T. VICKERS, B.A. (From the *Journal of the Royal Dublin Society*, No. 24, p. 271.)—The insects, after being caught, are put to steep in a solution of caustic potash, until they become clear, or nearly so; the strength of the solution which I think the best for general purposes is, half a drachm to the ounce. If stronger than this is used, I find it acts on the wings and delicate parts before the interior is sufficiently dissolved.

The next step is to place the insect in water, to wash away the potash. I generally allow it to remain twenty-four hours in water; I then take the slide on which it is finally to be mounted, and having rubbed it well to clean it, I float the insect on to the end of the slide, and not the middle (the reason for this will presently appear). I then place the insect as nearly as possible in the form in which I wish it finally to appear; and, taking another slide, at the end of which I hold a small bit of blotting paper, I lay it over the slide which has the object on it, the blotting paper being over and next the object; then gently and gradually press the two slides together, and pass over them a small clip made of flat brass-wire, and put the entire arrangement in water, to remain for twelve hours, at least.

Then take off the clip, turn the blotting paper which projects down over the slide which has the object on it, slip the other one off, and then gently peel off the blotting-paper, and in nine cases out of ten the object will adhere to the glass. Should you find the object sticking to the blotting-paper when you begin to peel it off, then try another corner, or even a third, before using a needle to it,—the great object being to use the needle in touching the insect as little as possible. Having got the blotting-paper off, and along with it most of the dirt, the next thing is to move the object to a clean part of the slide; and as the object is at the end of it, you can give the centre an extra rub before moving the insect into it. The moving is done, not with the needle directly, but by dropping water on the slide from a pipette; and you will find that the insect will float on the top of the water, and can be easily guided to the centre of the slip; a touch of the needle will retain it there, and the water can be drained off. And now the eye-glass or dissecting microscope must be brought into action, and the object finally set out. The parts are still limber, and can be arranged, which they could not be if the objects were soaked in turpentine to make them clear, in the first instance, as they used to be. The object, being arranged, must be left to dry.

The most convenient way I find to be, to tie up a number of slides together, with a bit of cork between each: I think it better not to leave them too long to dry, as the turpentine penetrates more easily if they are not too dry. I then put the entire bundle into a large, wide-mouthed bottle of turpentine, and the longer it is left there the bettter.

When proceeding to mount, I select a number of covering glasses, such as are likely to suit one of the bundles, and, after cleaning, place them on a long brass plate over a gas jet, and dropping as much Canada balsam as will just fit on the covers, and applying as much heat as will enable me to skim off the bubbles with a needle, I allow the covers and balsam to get *cold;* I then take one of the covers with a small forceps, and plunging it into a vessel of turpentine, apply it at once (balsam downwards, of course), to the object on the slide, slip on another brass clip, which is made so as to touch the cover only in one point, and that point the centre, then lay it on a little tin tray: when all the bundle or bundles are thus mounted and placed on the tray, I put the tray itself in a slow oven, and leave it there till all the turpentine is pressed out by the spring clip under the action of the low degree of heat. If too much heat be applied, the turpentine in the inside of the animal will boil, and bubbles will be produced, which you will find it very difficult or impossible to get rid of, and the object is spoiled.

It is in this part of the mounting that I consider my system has a great advantage over others which I have heard and read of. Some use balsam thinned with chloroform; when such is used before the specimens are perfectly dry, they are easily displaced, and in some instances entirely rubbed off the slide. I have heard of this having happened with several English mounted objects; and I myself have seen, in one of these which I have, the fluid mass moving in the inside, and running about among the legs of the animal, proving to me that I must be careful how I clean it; and I fancy that when the balsam does dry, it becomes very brittle. Mounting the way I do, when the slides have become clear and free from streaks in the oven, they are quite hard, and may be at once cleaned off with benzole and washing-soda.

2. *Collection of Minerals and Chemical Apparatus belonging to the late Prof. Manross.*[1]—This collection consists of about 500 good specimens of rocks and minerals, together with many hundreds of smaller size.

[[1] It will be remembered that Professor (Captain) Manross fell at the battle of Antietam, Sept. 17, 1862, while leading a charge. It would be a most worthy act of recognition of his patriotic devotion to purchase his instruments and collections for the benefit of his widow.]

They are mostly from New England, and embrace several large lumps of the Haddam chrysoberyl rock now no longer to be obtained. Many valuable specimens of gold and sulphur from South America and Mexico are also embraced in the collection. The entire cabinet has been appraised at the moderate sum of $100.

Among the apparatus are balances for analysis, a reflecting goniometer of the most finished and perfect German construction, and platinum and silver crucibles. The whole may be viewed at any time at the residence of Mrs. Manross, at Forrestville, Conn., or information respecting them may be obtained by application to Prof. C. U. Shepard (one of the appraisers of the estate) at Amherst College, Mass.

3. *Transactions of the Academy of Sciences of St. Louis*, Vol. II, No. 1. 218 pp., 8vo, with several plates. 1863.—This number contains many valuable papers, among which are the following: on Botany and Meteorology (noticed on page 128, this vol.), by G. Engelmann; on Geology and Paleontology, by B. F. Shumard, G. C. Swallow, and H. Engelmann; on Atmospheric electricity, by A. Wislizenus; on the Ascent of Pike's Peak, by Dr. C. C. Parry.

4. *Insecteans.*—On page 7, in mentioning names for the subdivisions of Insecteans, the word for the *second* division is written *Octopods.* As this is identical with the name for a group of Cephalopods, it would be better to substitute the equally, or even more, correct form of the word, *Octapods.*—J. D. D.

5. *Officers of the American Academy of Arts and Sciences*, chosen May 26, 1863.—*President*, ASA GRAY. *Vice-President*, CHARLES BECK. *Corresponding Secretary*, WILLIAM B. ROGERS. *Librarian*, JOSIAH P. COOKE. *Treasurer*, EDWARD WIGGLESWORTH. *Council*, The President, Vice-President, and the Secretaries, *ex officio*, THOMAS HILL, GEORGE P. BOND, JOHN B. HENCK, A. A. GOULD, LOUIS AGASSIZ, JEFFRIES WYMAN, ROBERT C. WINTHROP, GEORGE E. ELLIS, HENRY W. TORREY. *Rumford Committee*, JOSEPH LOVERING, MORRILL WYMAN, WILLIAM B. ROGERS, JOSEPH WINLOCH, CHARLES W. ELIOT, THEOPHILUS PARSONS, CYRUS M. WARREN. *Finance Committee*, The President and Treasurer, *ex officio*, J. INGERSOLL BOWDITCH. *Publication Committee*, JOSEPH LOVERING, JEFFRIES WYMAN, CHARLES BECK. *Library Committee*, A. A. GOULD, WILLIAM P. DEXTER, J. B. HENCK. *Auditing Committee*, THOMAS T. BOUVÉ, CHARLES E. WARE.

BOOK NOTICE.—

6. *Chauvenet's Spherical and Practical Astronomy.*[1]—In this work we have fresh evidence of the success with which astronomy has been cultivated of late years in the United States. The want, long ago felt, of some treatise on Practical Astronomy more modern and accessible than the bulky volumes of Pearson, was first met by the work of Prof. Loomis, published in 1855, which was at once adopted in the Universities of

[1] *A Manual of Spherical and Practical Astronomy:* embracing the General Problems of Spherical Astronomy, the Special Applications to Nautical Astronomy, and the Theory and Use of fixed and portable Astronomical Instruments. With an Appendix on the Method of Least Squares. By WILLIAM CHAUVENET, Professor of Mathematics and Astronomy in Washington University, Saint Louis, (Mo.) Vol. I, Spherical Astronomy. Vol. II, Practical Astronomy. Philadelphia: J. B. Lippincott & Co. London: Trubner & Co. 1863. Pp. 708, 632, 8vo.

Cambridge and Edinburgh as a text-book, and commended by leading English astronomers as the best in the language. The success of that work leads us to anticipate for Prof. Chauvenet's masterly and more extended treatise a reception, abroad as well as at home, every way worthy of its rare merits, and honorable in the highest degree to American science. It is fitting that, as we have already given to Practical Astronomy some of its most original and important improvements—as, for example, the American, or chronographic, method of transits, the telegraphic method of longitude, and Talcott's method of latitude—so we should also give to it a treatise of corresponding importance—such an one, in fact, as the work before us—the most complete and thorough that has yet appeared in any country or language.

Reserving for a following number of this Journal a more elaborate review of these volumes, we can here only indicate in brief their scope and leading features. The first volume, on Spherical Astronomy, discusses, with almost exhaustive completeness, the questions of parallax, refraction, time, latitude and longitude, eclipses, aberration, astronomical constants, etc. These discussions are characterized throughout by that remarkable generality and mathematical rigor, which belong distinctively to the German methods of investigation, so successfully employed by Bessel, and others of his school. Based upon these methods, Prof. Chauvenet's work represents astronomy in its most modern and perfected forms of research. Many of the investigations are, either wholly or in part, original—such, for example, as of some of the formulæ for latitude and eclipses, occultations of planets, improved methods of lunars, &c.

The same completeness and rigor of analysis characterize also the second volume, which embraces Practical Astronomy. The Theory of Instruments is particularly elaborate and exhaustive. Almost every conceivable use of each for any important purpose is carefully discussed. The chief topics of the volume are telescopes, the general theory of instruments for angles, instruments for time, the sextant, transit instrument, meridian circle, altitude and azimuth instrument, zenith telescope, equatorial telescope, heliometer, and the filar and ring micrometers. Old instruments and old methods are wholly discarded.

Not the least valuable part of the work is the Appendix of a hundred pages on the Method of Least Squares and Pierce's Criterion—examples of the application of which in the discussion of observations abound throughout the work. A few auxiliary tables—altogether too few—are given at the close, nearly half of them belonging to the author's method of lunars. The steel plates illustrative of instruments, though on a small scale, are very clear, and perhaps sufficiently in detail for the purpose they were intended to serve. The mechanical execution of the work—type, paper, &c.—are worthy of its scientific merits, and all that the most fastidious could desire.

The Computation of Orbits and Perturbations—topics, in part at least, belonging to practical astronomy, and naturally looked for in a work like this—obviously could not have been included without too great condensation or too great bulk, and are doubtless reserved by the author for another volume, which we trust the success of the present publication will induce him to prepare at an early day.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

Art. XV.—*On the Velocity of Light and the Sun's Distance;* by Prof. Joseph Lovering, of Harvard College.

Foucault's recent experiment on the velocity of light, though of a less popular character than his celebrated pendulum experiment to prove the earth's rotation, will, nevertheless, attract even more attention among men of science. If its results are placed beyond doubt, they will affect astronomy to a degree not possible for the pendulum experiment, unless it had come as early as the time of Galileo. I shall examine Foucault's investigation on the velocity of light: 1, as it influences the science of Optics; and 2, as it tells upon one at least of the vexed questions in astronomy.

In the circle of the sciences, the centre may be placed anywhere and the circumference will be everywhere: such is the mutual dependence of each upon all the rest. After the science of optics has furnished astronomy with the telescope, the astronomer discovers with it the satellites of Jupiter and the aberration of light, and with the help of these phenomena assigns the value of the velocity of light, and thus repays to optics the debt incurred by his own special science. Now, for the first time, the science of optics has relinquished the guardianship of astronomy; ascertained by direct experiment one of its own fundamental data; and thereby, possibly, put astronomy under a new obligation, to be cancelled, doubtless, with interest, hereafter.

Let us glance first at the two astronomical methods of measuring the velocity of light. While the senses of touch and taste act only by contact, those of hearing and seeing bring the mind into communication with distant objects. The air and the omnipresent ether supply the delicate and ever ramifying threads by which telegraphic intercourse is maintained with the ear and the eye. When the origin of the sound or the light is at a large distance, compared with the velocity of the acoustic or luminous wave, allowance must be made for the time taken by the news of an audible or visible event to come to us. Only the vast spaces of astronomy are commensurable with the great velocity of light, and furnish a sufficiently large theatre for a direct experiment upon it. But, in stellar astronomy, the magnificence of the extent of view so far transcends in magnitude even the velocity of light, that the luminous ray, vast as is its velocity, seems to loiter upon its long way.

Hence, in astronomy, a distinction exists between the *actual* interval of successive events and the *apparent* interval. For example, the first satellite of Jupiter revolves around its primary in about 42½ hours; and, therefore, enters the shadow of Jupiter, and is eclipsed, once every 42½ hours. As it takes light more than 40 minutes to pass over the average distance of Jupiter, the eclipse is not seen until so many minutes on the average after it has happened. If this delay were constant, the interval of successive eclipses would not be changed. But in the course of six months the distance of the earth from Jupiter increases by the diameter of the earth's orbit, and in the next six months changes back again; and when the earth is nearest to Jupiter, the news of an eclipse reaches us in about 32 minutes, whereas, if the earth is at the greatest distance, 50 minutes are required.

Consequently, the intervals between successive eclipses, as they exist for our eyes, are variable, being sometimes larger and sometimes smaller than the real intervals. This irregularity in the apparent intervals of the eclipses of the same satellite, at first attributed to errors of observation, finally conducted Römer, in 1675, to the discovery of the velocity of light. Delambre, after discussing 1000 of these eclipses, observed between 1662 and 1802, calculated the velocity of light to be such as to require 493·2 seconds to pass over the mean distance of the sun. If this time divides 95,360,000 statute miles, which is the sun's distance as given by the transits of Venus in 1761 and 1769, according to Encke's computations, the quotient, or 193,350 statute miles, is the velocity of light in a second.

The second process which astronomy has supplied for obtaining the velocity of light may be called the indirect method. It demands not a *space* but a *velocity* which is commensurable with the velocity of light. If two such velocities are compounded together, according to the principle of the *parallelogram of mo-*

tions, there is a resultant motion, the direction of which deviates sensibly from that even of the largest motion which enters into the composition. In nature, the velocity of the earth is compounded, in this way, with the velocity of light, and imparts to the light an apparent path differing by a small angle from the true path. The angular displacement which this causes between the apparent and real places of a star is called aberration, and was first discovered by Bradley in 1726: this astronomer explaining, on this simple principle, anomalies in observation which had hitherto been considered accidental. As the displacement of the star works opposite ways at opposite seasons of the year, half the difference between the extreme places is the distance from the apparent to the true place, or the constant of aberration. This, when known as an observed fact, establishes the ratio between the velocity of light and the velocity of the earth, and enables the astronomer to assign the value of the one with all the accuracy which pertains to his knowledge of the other. Accepting Struve's determination of the aberration, viz: 20″·45, the velocity of light is calculated to be 10,088 times as great as the velocity of the earth. The mean velocity of the earth is known with all the certainty which belongs to our knowledge of the magnitude of the earth's orbit: that is, of the sun's distance. Assuming, as before, that the distance derived from Encke's parallax is the most reliable, the velocity of the earth in one second of solar time is 18·977 miles. This multiplied by the aforesaid ratio gives 191,513 miles for the velocity of light by Bradley's principle. It appears therefore that the velocities by the two methods of astronomy (the direct and the indirect) differ by 1837 miles; a small quantity comparatively, being only *one per cent* of the whole velocity. Whatever other value is adopted for the sun's distance will alter these two results proportionally, without disturbing the ratio between them. I may add that the velocity which aberration ascribes to light belongs to it at the earth's surface; that is, in the dense atmosphere: whereas, the velocity discovered from the eclipses is that which extends through the planetary spaces. This distinction, however, will do little towards bringing the two results into greater accordance. The velocities of light in different media are proportional to the indices of refraction inversely: which in the case presented are as 1 to 1·000294. This theoretical difference of velocities is less than $\frac{1}{3000}$ of the whole, or less than 70 miles.

Compare with these conclusions of astronomy two experimental results on the same subject. Although Wheatstone's experiment on the velocity of electricity, published in 1834, suggested the possibility of measuring, in a similar way, other great velocities, I shall consider first a contrivance of Fizeau, equally applicable to light and to electricity. If a wheel finely cut into teeth on its circumference is put in rapid rotation, a ray of light, which escapes between two consecutive teeth, will, after being reflected perpendicularly by a mirror, return to strike the wheel at a different point, and either be intercepted by a tooth or admitted at another interstice. Suppose the velocity of the wheel just sufficient to bring the adjacent tooth to the position whence the ray first started, in the time which the light occupies in going to the mirror and returning. In this time the wheel has moved over an angle found by dividing 360° by twice the number of teeth which the wheel contains. Therefore the time taken by light, in going over a line equal to twice the distance of the mirror, is that portion of a second found by dividing unity by the product of the number of turns the wheel makes in a second, multiplied by double the number of teeth on the wheel; the velocity of the wheel being first made the smallest which will cause it to intercept the light. Such an experiment was made in 1849 by Fizeau, the wheel being placed in a tower at Suresne, near Paris, and the mirror upon a hill (Montmartre) at the distance of 8633 metres. As the wheel contained 720 teeth, and the slowest velocity which produced obscuration was 12·6 turns a second, it appeared that light required $\frac{1}{18144}$ of a second to go 8633 metres and return. Hence its velocity was 313,274,304 metres or 194,667 miles a second. The French Academy thought so favorably of this attempt that they referred the subject to a scientific commission consisting of Biot, Arago, Pouillet and Regnault, with authority to procure a grand machine for repeating the experiment.

When Arago advocated the claims of Wheatstone to the vacant place of Corresponding Member of the French Academy in the section of Physics, it was objected that Wheatstone had only made a single experiment without having discovered a principle. Arago engaged to prove that the candidate had introduced a fertile method of experimentation which would be felt in other sciences as well as electricity. For example: the corpuscular theory of light requires that the velocities of light in different media should vary directly as the indices of refraction, whereas the undulatory theory inverts this ratio. Arago prepared for the trial by experiments on rapid rotation, the mechanical difficulties to be overcome, and the comparative advantage of slower rotations assisted by several reflexions, in place of a single mirror turning with its maximum speed. Aided by the refined skill of Breguet, he realized velocities in the mirror of 1000 turns a second, and of the axis, detached from the mirror, of even 8000 turns. In the meanwhile his eyesight began to fail him, and younger physicists entered into the fruit of his labors. After Foucault and Fizeau, by separate efforts, had decided the question, in relation to the velocities of light in air and in water, in favor of the undulatory theory, and thus confirmed a conclusion which Arago reached by *diffraction* in 1838, and after Fizeau had studied the variation of the velocity of light in running water, according as the motions agree or differ in direction, Foucault was emboldened to attempt a measure of the *absolute* velocity of light, by an experiment which could be brought within the compass of a single room. I translate his own account of the arrangements made for this purpose.

"A pencil of solar light, reflected into a horizontal direction by a heliostat, falls upon the micrometric mark, which consists of a series of vertical lines distant from one another one-tenth of a millimetre. This mark, which in the experiment is the real standard of measure, has been divided very carefully by Froment. The rays, which have traversed this initial surface, fall upon a plane rotating mirror at the distance of a metre, where they suffer the first reflexion, which sends them to a concave mirror at the distance of four metres. Between these two mirrors, and as near as possible to the plane mirror, is placed an object-glass, having in one of its conjugate foci the virtual image of the mark, and in the other the surface of the concave mirror. These conditions being fulfilled, the pencil of light, after traversing the lens, forms an image of the mark on the surface of this concave mirror.

"Thence the pencil is reflected a second time, in a direction just oblique enough to avoid the rotating mirror, an image of which it forms in the air at a certain distance. At this place a second concave mirror is placed, facing so that the pencil, once more reflected, returns to the neighborhood of the first concave mirror, forming a second image of the mark. This is taken up by a third concave mirror, and so on to the formation of a last image of the mark on the surface of the last concave mirror of an odd number. I have been able to use five mirrors, which furnish a line twenty metres long for the ray to travel.

"The last of these mirrors, separated from the preceding one, which faces it, by a distance of four metres (equal to its radius of curvature), returns the pencil back on itself: a condition surely fulfilled when the returning image and the original image on the last mirror but one coalesce. Then we are sure that the pencil retraces its steps, returns in full to the plane mirror, and all the rays go back through the mark, point for point, as they went forth.

"This return of the pencil may be proved on an accessible image by reflecting the pencil to one side by a surface of glass at an angle of 45°, and examining it through a microscope of small power. The latter, resembling in all respects the micrometric microscopes in use for astronomical observations, forms, with the mark and the inclined glass, one solid piece of apparatus.

"The real image sent into the microscope, and formed by the returning rays partially reflected, occupies a definite position in relation to the glass and the mark itself. This position is precisely that of the virtual image of the mark seen by reflexion in the glass. At least, this is true when the plane rotating mirror is at rest. But when this mirror turns, the image changes its place: for, while the light is going and returning between the mirrors, the plane mirror has shifted its position, and the returning rays do not strike at the same angle of incidence as when they left it. Hence the image is displaced in the direction of the rotation, and this displacement increases with the velocity of rotation: it also increases with the length of the route passed over by the rays, and with the distance of the mark from the plane mirror.

"If we call V the velocity of light, n the number of times the mirror turns in a second, l the distance between the plane mirror and the last concave mirror, r the distance of the mark from the turning mirror, and d the observed displacement, we have $V = \frac{8\pi n l r}{d}$: an expression which gives the velocity of light when the other quantities are separately measured. The distances l and r are measured directly by a rule. The deviation is observed micrometrically: it remains to show how the number of turns (n) of the mirror is found.

"Let us describe first how a constant velocity is imparted to the mirror. This mirror, of silvered glass, and fourteen millimetres in diameter, is mounted directly upon the axis of a small air-turbine, of a well known model, admirably constructed by Froment. The air is supplied by a high pressure bellows of Cavaillé-Colle, justly distinguished for the manufacture of great organs. As it is important that the pressure should be very constant, the air, after leaving the bellows, traverses a regulator, recently contrived by Cavaillé, in which the pressure does not vary by one-fifth of a millimetre in a column of water of thirty centimetres. The fluid flowing through the orifices of the turbine represents a motive power of remarkable constancy. On the other hand, the mirror, when accelerated, soon encounters in the surrounding air a resistance which for a given velocity is also perfectly constant. The moving body placed between these two forces, which tend to equilibrium, cannot fail to receive and to preserve a uniform velocity. Any check whatever, acting upon the flow of the water, allows this velocity to be regulated within very extensive limits."

"It remains, to estimate the number of turns, or rather to impress on the moving body a determined velocity. This problem has been completely resolved in the following manner. Between the microscope and the reflecting glass, a circular disc is placed, the edge of which, cut in fine teeth, encroaches upon the mark and partly intercepts it. The disc turns uniformly on itself, so that, if the image shines steadily, the teeth at its circumference escape detection from the rapidity of the motion. But the image is not permanent: it results from a series of discontinuous appearances equal in number to the revolutions of the mirror; and, whenever the teeth of the screen succeed one another with the same frequency, there is produced on the eye an illusion easily explained, which makes the teeth appear immovable. Suppose then that the disc, with n teeth in its circumference, turns once in a second, and that the turbine starts up. If, by regulating the flow of air, the teeth are made to appear fixed, we are certain that the mirror makes n turns in a second.

"Froment, who made the turbine, wished to invent and construct a chronometric wheelwork to move the disc. It is a remarkable piece of clock-work, which resolves, in an elegant manner, the problem of uniform motion in the particular case in which there is no work to be done. The success is so complete that it is my daily experience to launch the mirror with 400 turns a second, and see the two pieces of apparatus march within $\frac{1}{10000}$ nearly of accordance during whole minutes.

"Notwithstanding the assurance I had gained in the measurement of time, I was surprised at proving in my results, discordances which were out of proportion to the precision of my means of measuring. After long research, I discovered the source of error in the micrometer, which did not allow of the degree of accuracy willingly attributed to it. To meet this difficulty, I have introduced into the system of observation a modification which amounts simply to a change of the variable. Instead of measuring micrometrically the deviation, I adopt for it a definite value in advance, suppose seven-tenths of a millimetre, or seven entire parts of the image; and I seek by experiment to find the distance between the mark and the turning mirror necessary to produce this deviation: the measures extending over a length of about a metre, the last fractions have a magnitude directly visible, and leave no room for error.

"By this means the apparatus has been purged of the principal cause of uncertainty: henceforth the results accorded, within the limits of errors of observation, and the means are settled in such a way that I am able to assign confidently the new number which appears to me to express nearly the velocity of light in space, viz: 298,000 kilometres in a second of mean time."

This value, reduced to statute miles, shows that the velocity of light is 185,177 miles in a second; which is less by 6336 miles than the velocity for light usually admitted into science, viz: the velocity obtained from the aberration of light. This discrepancy between the results of experiment and that of the astronomical determination, which comes nearest to it, is three times greater than the variation between the velocity deduced from aberration and that derived from eclipses.

Foucault states that the extreme difference of the results of various trials amounted to only $\frac{1}{100}$ of the whole quantity, and that the mean result can be trusted to the fraction of $\frac{1}{500}$. Moreover, the aberration of 20″·45, adopted by astronomers, cannot be supposed at fault by more than $\frac{1}{1800}$ of the whole. Neither the velocity by Foucault's experiment nor the value of aberration can be charged with a possible error of three per cent, or of any error approaching to this large discrepancy. How is the new velocity of light to be reconciled with the old value of aberration? I have said that aberration establishes only the *ratio* between the velocity of light and the velocity of the earth. If this ratio cannot be tampered with, and if one term of it (the velocity of light) must be diminished by three per cent, to suit Foucault's experiment, then we must at the same time diminish the other term (the velocity of the earth) proportionally; and the old ratio will be preserved, and the value of aberration will be left unchanged. Is it possible, therefore, that there can be an uncertainty to the extent of three per cent in the velocity of the earth? If so, the tables are turned: and, instead of employing the ratio which aberration supplies to calculate the velocity of light from the velocity of the earth, as the best known of the two, we henceforth must calculate the velocity of the earth from the velocity of light. For, Foucault has found the latter by experiment more accurately than astronomy gives the former. If there is an error of three per cent in the velocity of the earth, it is an error in space and not in time. To diminish the velocity of the earth sufficiently by a change of time would demand an increase in the length of the year amounting to eleven days nearly.

The only other way of reaching the velocity of the earth is by diminishing the circumference of the earth's orbit, and this, if diminished, changes proportionally the mean radius of the orbit; that is, the sun's mean distance. The question, therefore, resolves itself into this. Can the distance of the sun from the earth be considered uncertain to the extent of three per cent of the whole distance?

The answer to this question will lead me into a brief discussion of the processes by which the sun's distance from the earth has been determined, and the limits of accuracy which belong to the received value. To see the distance of any body is an act of *binocular* vision. When the body is near, the two eyes of the same individual converge upon it. The interval between the eyes is the little base-line, and the angle which the optic axes of the two eyes, when directed to the body, make with each other is the parallax; and by this simple triangulation, in which an instinctive geometrical sense supersedes the use of sines and logarithms, the distance of an object is roughly calculated. As the distance of the object increases, the base-line must be enlarged; but the geometrical method is the same, even when the object is a star and the base of the triangle the diameter of the earth's orbit. Substitute then for the two eyes of the same observer the two telescopes of different astronomers, planted at the opposite extremities of the earth's diameter, and any one will understand the principle upon which the binocular eye of science takes its stereoscopic view of the universe, and plunges into the depths of space. In this way it is that the distance of the sun from the earth is associated with the *solar parallax:* which is the angle between the directions in which two astronomers point their telescopes when they are looking at the sun at the same moment. To know the sun's distance, the astronomer studies the solar parallax. As Kepler's third law establishes a relation between the distances of the different planets from the sun, and their periods of revolution, if the astronomer finds either distance by observation, the others can be computed from this law. As the solar parallax is only about eight seconds, and an error of one-tenth of a second includes an error of more than a million of miles in the sun's distance, he takes advantage of the law of Kepler, and selects a planet which comes occasionally nearer to the earth than the sun. The choice lies between Venus at inferior conjunction and Mars at opposition. The parallax of Mars may vary from 20″·7 to 19″·1, according to the positions of Mars and the earth with respect to the perihelion of the orbit at the time of opposition. The parallax of Venus at conjunction may vary, for the same reasons, from 33″·9 to 29″·9. Venus, therefore, may be nearer to the earth than Mars, and the parallax more favorable. But Venus cannot be seen at conjunction except when its latitude is so small that a transit across the sun's disc occurs. Then the two observers refer its place not to a star but to the sun, and the quantity they determine is the difference of parallax between Venus and the sun; which will vary from about 21″ to 25″. Moreover, the difference of parallax is measured, not directly, but through the influence it produces on the duration of the transit at the two stations: and, therefore, upon a greatly enlarged scale.

What are the results which have been obtained: 1st, by observations of the transits of Venus, and 2d, by observations on Mars at opposition?

1. Only two transits of Venus have occurred since the time when the sagacious Dr. Halley invoked the attention of posterity to these rare astronomical events as pregnant with the grandest results to science; viz: those of 1761 and 1769. The astronomers of the last century did not neglect the charge which Halley consigned to them. The transit of 1769 was eminently favorable, offering a chance which comes only once in a millenium, as Professor Winthrop happily explained in his lectures on the last transits.

Whatever verdict posterity shall pronounce on the deductions from the observations then made, they will never, says Encke, reproach astronomers or governments with negligence or want of appreciation towards this golden opportunity. The solar parallax which Encke deduced from an elaborate discussion of all the observations, fifty years after they were made, is 8″·57116. This corresponds to a solar distance of 95,360,000 statute miles.

Although transits of Venus will take place in 1874 and 1882, and astronomers already begin to talk of preparing for them, I have the authority of Encke for declaring that, in comparison with that of 1769, the next two transits will be so unfavorable that nothing short of perfection in the construction of instruments, and in the art of observing, can compensate for the natural disadvantage: so that the reduction of the possible error in the sun's parallax within the limit of one one-hundredth of a second is hopeless for at least two centuries more.

2. The solar parallax may also be derived from the parallax of Mars, when this planet is in opposition. In 1740 the French astronomer, Lacaille, was sent to the Cape of Good Hope, and from the parallactic angle observed between the direction of Mars as seen from that station and from the observatory of Paris (deduced from observations of declination), the horizontal parallax of Mars was computed, and consequently that of the sun. The solar parallax thus found was 10″·20, with a possible error not exceeding 0″·25. Henderson, by comparing his own observations of the declination of Mars at its opposition in 1832 with corresponding observations at Greenwich, Cambridge, and Altona, computed the solar parallax at 9″·028.

The United States Naval Astronomical expedition to Chili, under the charge of Lieut. J. M. Gilliss, during the years 1849–1852, had for its object the advancement of our knowledge of the solar parallax, partly by observations of Mars at opposition, and partly by observations of Venus during the retrograde portion of her orbit, and especially at the stationary points, in conformity with a method suggested by Dr. Gerling; the whole to be compared with simultaneous observations at northern observatories. Although the observations at Chili were made on 217 nights, covering a period of nearly three years, the coöperation of northern astronomers was so insufficient that only 28 corresponding observations were made. On this account the second conjunction of Venus was useless: the other conjunction of Venus and the second opposition of Mars were of little value; and even the first opposition of Mars led to no significant result. Dr. B. A. Gould has computed the solar parallax from the first opposition of Mars, observed at Chili, at 8″·50.

3. The solar parallax can also be computed from the law of universal gravitation. The principle may be thus stated. The motion of the moon round the earth is disturbed by the unequal attraction of the sun on the two bodies. The magnitude of the disturbance will be in some proportion to the distance of the disturber when compared with the relative distance of the two disturbed bodies: and this ratio of distances is the inverse ratio of the parallaxes of the sun and moon. By selecting one of the perturbations in the moon's longitude particularly adapted to this purpose, Mayer, as early as 1760, computed the solar parallax at 7″·8. In 1824, Burg calculated this parallax from better observations at 8″·62. Laplace gives it at 8″·61. Fontenelle had said that Newton, without getting out of his armchair, found the figure of the earth more accurately than others had done by going to the ends of the earth. Laplace makes a

similar reflexion on this new triumph of theory. "It is wonderful that an astronomer, without going out of his observatory, should be able to determine exactly the size and figure of the earth, and its distance from the sun and moon, simply by comparing his observations with analysis, the knowledge of which formerly demanded long and laborious voyages into both hemispheres." The accordance of the results obtained by the two methods is one of the most striking proofs of universal gravitation. Pontecoulant makes the solar parallax by this method 8″·63. Lubboch, by combining Airy's empirical determination of the coëfficient with the mass of the moon as he finds it from the tides (viz: $\frac{1}{67}$), makes the solar parallax 8″·84. If the mass of $\frac{1}{75}$ is substituted, the parallax is changed to 8″·81. Finally, Hansen, in his new *Tables of the Moon*, adopts 8″·8762 as the value of the solar parallax. Moreover, Leverrier, in his *Theory of the apparent motion of the Sun*, deduces a solar parallax of 8″·95 from the phenomena of precession and nutation.

The conclusions of this whole review are summed up in the following table: in which the values of the solar parallax and of the sun's distance, by the three methods of astronomy, and by the experiment of Foucault, are placed in juxtaposition: also the different velocities of light found by astronomical observations and by experiment.

Observer or Computer.	Method.	Parallax.	Distance.
Encke,	By Venus (1761),	8″·53	95141830 miles.
Encke,	" " (1769),	8 ·59	95820610
Lacaille,	By Mars,	10″·20	76927900
Henderson,	" "	9 ·03	90164110
Gilliss and Gould,	" "	8 ·50	96160000
Mayer,	By Moon,	7″·80	104079100
Burg,	" "	8 ·62	94802440
Laplace,	" "	8 ·61	94915970
Pontecoulant,	" "	8 ·63	94689710
Lubboch,	" "	8 ·84	92313580
"	" "	8 ·81	92652970
Hansen,	" "	8 ·88	91861060
Leverrier,	" "	8 ·95	91066350
Foucault,	By light,	8″·86	92087342
Fizeau,	" "	8 ·51	95117000
Velocity of light,	By eclipses,		193350
" "	" aberration,		191513
" "	" Fizeau's experiment,		194667
" "	" Foucault's experiment,		185177

Foucault's experiment on the velocity of light has been popularly announced as making a "revolution in astronomical science." But it appears from the preceding sketch that it has raised no new question in astronomy, though it may have attracted popular attention to an old difficulty, and possibly given a solution to it. The three astronomical methods present solar distances, which, even if we select the most trustworthy decision of each, differ by three or four millions of miles: that is, by three or four per cent of the whole quantity. Though the best products of the first and third methods were at one time within a million of miles of each other, an increase of lunar observations, and especially improvements in the lunar tables, have now carried that difference up to four millions of miles. If Foucault's experiment were allowed to give the casting vote, it would decide in favor of the third method; thus making the reflexion of Laplace, which I have already quoted, still more memorable.

In regard to the commonly received distance of the sun, which is based upon Encke's profound discussion of all the observations made at the last two transits of Venus, the case stands thus. Encke decides, from the weights of the observations, discussed in the light of the mathematical principle of *least squares,* that the probable error of the sun's distance, as given by the transits, does not exceed $\frac{1}{230}$ of the whole quantity. Astronomers have also reason to believe that the adopted value of aberration is correct within $\frac{1}{1800}$ of the whole quantity. Moreover, Foucault is confident of his determination of the velocity of light within $\frac{1}{500}$ of the whole quantity; nay, he expects to improve his instruments so as to banish all errors larger than $\frac{1}{5000}$ of the whole quantity. Neither the velocity of light, aberration, nor the sun's distance can be suspected of an error to the extent of three or four per cent; and yet one at least must be wrong to this degree, as the best values of the three elements are irreconcilable with each other. Which shall be changed?

It may excite surprise in those who have heard of the *accuracy* of astronomy, without weighing the exact significance of the word as applied to so large a subject, that there should still be a lingering uncertainty, to the extent of three or four millions of miles, in the sun's distance from the earth. But the error, whatever it is, is propagated from the solar system into the deepest spaces which the telescope has ever traversed. The sun's distance is the measuring rod with which the astronomer metes out the distances of the fixed stars and the dimensions of stellar orbits. An error of three per cent in the sun's distance entails an error of three per cent in all these other distances and dimensions. Trifling as three per cent may seem, the correction runs up to 600,000 millions of miles in the distance of the nearest fixed star!

ART. XVI.—*Further Remarks on a method of Reducing Observations of Temperature;* by Professor J. D. EVERETT, of Kings College, Windsor, Nova Scotia.

IN an article in the January number of this Journal, I recommended for general use a method of comparing climates, as regards temperature, by reference to the curve whose equation is

$$y=A_0+A_1 \sin(x+E_1)$$

Professor Loomis, in his "*Remarks*" appended to my article, having impugned the accuracy of this mode of procedure, on the ground that the curve of temperature at a place does not possess the symmetry which characterizes the curve expressed by the above equation, I propose, in the present paper, to furnish a thorough investigation of the degree of accuracy which is attained.

We must first prove the following proposition: Any m numbers, (m being either $2n$ or $2n+1$,) can be *exactly* expressed by an equation of the form

$$y=A_0+A_1 \sin(x+E_1)+A_2 \sin(2x+E_2)+ \ldots\ldots +A_n \sin(nx+E_n),$$

in such a sense that, by giving x the successive values,

$$0, \frac{1}{m}\times 360°, \frac{2}{m}\times 360° \ldots\ldots \frac{m-1}{m}\times 360°,$$

the resulting values of y will be the given numbers in order. We shall hereafter denote the terms on the second side of the above equation, by the symbols t_0, t_1, t_2, &c. To prove the proposition, let the series be transformed, (see p. 25 of my former article,) into

$$y=A_0+P_1\cos x+Q_1\sin x+P_2\cos 2x+Q_2\sin 2x+\ldots\ldots+P_n\cos nx+Q_n\sin nx.$$

We can then form m equations, by writing the given numbers in order for y, and giving x the corresponding values above indicated. If $m=2n+1$, we shall have $2n+1$ equations, to determine the $2n+1$ constants A_0, P_1, Q_1, &c. If $m=2n$, the last term, $Q_n \sin nx$, is to be omitted; and we shall have $2n$ equations to determine $2n$ constants. Hence, by the theory of equations, there will always be one and only one solution.

The rule for obtaining the solution is extremely simple:—

To find the value of any one of the constants, multiply each equation by its coefficient of that constant, and add the m equations thus obtained.[1] It will be found that all the terms on the

[1] It is worthy of remark that this rule is identical with that laid down, in the theory of probabilities, for determining the most probable values of any number of unknown quantities from a greater number of simple equations, when the latter are all of equal weight. This general rule is thus given in "*Airy on Errors of Observations,*" p. 80: "Multiply every equation by its coefficient of one unknown quantity, and take the sum for a new equation; the same for the second unknown quantity; and so on for every unknown quantity; and thus a number of equations will be found equal to the number of unknown quantities."

second side destroy one another, except those which contain the constant we are seeking, and the sum of these will be $\frac{m}{2}$ times the constant, except when the constant is P_n in the case where $m=2n$, for which the sum will be mP_n. When there are 12 given numbers, this process resolves itself into that described in my former article,[1] (*this Journal*, [2], xxxv, 17), supplemented by the following formulæ:

$$6P_5=k_0-s_2(k_1-k_5)+s_1(k_2-k_4)$$
$$6Q_5=\quad s_1(k_1-k_5)-s_2(k_2-k_4)+k_3$$
$$12P_6=K_0+K_2+K_4-K_1-K_3-K_5$$

The monthly means at Greenwich, derived from the table in Professor Loomis' "*Remarks*," reckoning the 31st of January and 1st of March as part of February, are

36·6 38·6 41·4 46·2 52·9 59·0 61·8 61·0 56·5 49·9 43·2 39·3

These are exactly expressed by the formula,

$$y=48.87+12{\cdot}44\sin(x+262°\ 31')+{\cdot}84\sin(2x+57°\ 44')+{\cdot}18\sin(3x+338°\ 12')$$
$$+{\cdot}25\sin(4x+253°\ 26')+{\cdot}20\sin(5x+252°\ 29')+{\cdot}13\sin(6x+270°),$$

where x is 0° for January, 30° for February, 60° for March, and so on.

Here t_0 is the constant 48·87.

The values of t_1, for the 12 months in order, are

−12·33 −11·49 −7·57 −1·62 +4·76 +9·87 +12·33 +11·49 +7·57 +1·62 −4·76 −9·87.

The values of t_2 are

+·71 +·74 +·03 −·71 −·74 −·03 +·71 +74 +·03 −·71 −74 −·03

These values, it will be observed, repeat themselves after the first six; and the sum of any consecutive six is 0.

The values of t_3 are

−·07 +·17 +·07 −·17 −·07 +·17 +·07 − ·17 −·07 +·17 +·07 −·17

which repeat themselves after the first four, or go through their cycle in a third of a year. Also the sum of any consecutive four is 0.

The values of t_4 are

−·24 +·06 +·18 − ·24 +·06 +·18 −·24 +·06 +·18 −·24 +·06 +·18

which go through their cycle 4 times in the year. Also the sum of any consecutive three is 0.

The values of t_5 are

−·19 +·13 −·04 −·06 +·15 −·19 +·19 −·13 +·04 +·06 −·15 +·19

[1] The following errata in the formulæ there given require correction:—
p. 26, lines 9, 10, 11, for K_0+K_3 read K_0-K_3
" K_1+K_4 " K_1-K_4
" K_2+K_5 " K_2-K_5

which do not so plainly show the recurrence of cycles, because 5 is not a measure of 12. It is easy however to trace 5 maxima and 5 minima.

The values of t_6 are

$-\cdot13 + \cdot13 - \cdot13 + \cdot13 - \cdot13 + \cdot13 - \cdot13 + \cdot13 - \cdot13 + \cdot13 - 13 + \cdot13$

which go through their cycle 6 times in the year. Also the sum of any consecutive two is 0.

By addition, we find the values of $t_0+t_1+t_2+t_3+t_4+t_5+t_6$ to be

36·62 38·61 41·41 46·20 52·90 59·00 61·80 60·99 56·49 49·90 43·22 39·30

or, to one place of decimals,

36·6 38·6 41·4 46·2 52·9 59·0 61·8 61·0 56·5 49·9 43·2 39·3

which agree exactly with the monthly means.

The error committed by stopping at any term, is, of course, equal to the sum of the terms omitted. These sums are given in the following table; the first line containing the sum of all the terms after t_1, the second line the sum of all after t_2, and so on,—

+·08	+1·23	+·11	−1·05	−·73	+·26	+·60	+·63	+·05	−·59	−·87	+·30
−·63	+ ·49	+·08	− ·34	+·01	+·29	−·11	−·11	+·02	+·12	−·13	+·33
−·56	+ ·32	+·01	− ·17	+·08	+·12	−·18	+·06	+·09	−·05	−·20	+·50
−·32	+ ·26	−·17	+ ·07	+·02	−·06	+·06	+·00	−·09	+·19	−·26	+·32
−·13	+ ·13	−·13	+ ·13	−·13	+·13	−·13	+·13	−·13	+·13	−·13	+·13

The terms t_2, t_4, t_6 produce no effect upon the mean temperature of a half-year, for the sum of any 6 consecutive values of these terms is 0; hence, in deriving the mean temperature of a half-year from t_0+t_1, the error committed depends only on t_3 and t_5. The sums of t_3 and t_5, for each month, are

$-\cdot26 + \cdot30 + \cdot03 - \cdot23 + \cdot08 - \cdot02 + \cdot26 - \cdot30 - \cdot03 + \cdot23 - \cdot08 + \cdot02$

and the means of every consecutive six of these, commencing with October—March, are

$+\cdot04 - \cdot04 - \cdot01 - \cdot02 + \cdot07 - \cdot03 - \cdot04 + \cdot04 + \cdot01 + \cdot02 - \cdot07 + \cdot03$

which are the corrections required in determining the mean temperature of 6 consecutive calendar months from t_0+t_1. In fact, the means of t_0+t_1 for every 6 months, in the above order, are

41·47 40·93 42·52 45·81 49·92 53·75 56·27 56·81 55·22 51·93 47·82 43·99

while the means of the actual monthly temperatures are

41·50 40·88 42·50 45·78 49·98 53·72 56·23 56·85 55·23 51·95 47·75 44·02

showing the same corrections as above, allowing differences of 1 in the last figure for neglected places of decimals. Hence we see that the mean temperature of any 6 consecutive months at Greenwich can be calculated from t_0 and t_1 (or from their constants A_0 A_1 E_1), subject to errors not exceeding ·07 of a degree.

It may be shown in general, by a theorem proved on p. 30 of my former article, that, if the expression for monthly means be

$$y=t_0+t_1+t_2+t_3+t_4+t_5+t_6,$$

the expression for half-yearly means will be

$$Y=t_0+{\cdot}644t_1-{\cdot}236t_3+{\cdot}173t_5;$$

that is,

$$Y=A_0+{\cdot}644\,A_1\sin(x+E_1)-{\cdot}236A_3\sin(3x+E_3)+{\cdot}173A_5\sin(5x+E_5)$$

and since $\cdot236\,A_3$ and $\cdot173\,A_5$ are practically insignificant compared with $\cdot644\,A_1$, the two last terms may be neglected.

For Greenwich, we have

$$\cdot644A_1=\cdot644\times12\cdot44=8\cdot01,$$
$$\cdot236A_3=\cdot236\times\ \cdot18=\ \cdot04,$$
$$\cdot173A_5=\cdot173\times\ \cdot20=\ \cdot03.$$

Hence the sum of the two last terms in the expression for Y cannot exceed $\cdot04+\cdot03=\cdot07$, which is only $\frac{1}{114}$ of $\cdot644\,A_1$; and the greatest error produced by the omission of these terms in comparing two halves of the year cannot be more than about $\frac{1}{114}$ of the difference between the greatest and least values of Y. It is to be observed that the half-yearly means denoted by Y are not limited to means of 6 calendar months, but may belong to any 182½ consecutive days.

It thus appears that the values of A_0, A_1, E_1 are sufficient to determine, with competent accuracy, the mean temperature of any half-year, and hence to determine the range as measured by the difference between the warmest and coldest halves of the year, a system of measurement which I think is as fair as any that can be devised.

The determination of the precise dates of maximum and minimum is always difficult, whatever method be pursued, owing to the slow rate at which temperature changes when near its maximum or minimum. Even the table of daily temperatures at Greenwich, though derived from an average of 43 years, leaves both these dates doubtful, as a glance will show. It is always much easier to determine the dates at which the temperature is equal to the mean of the year, (or at which the curve of temperature intersects the line of mean annual temperature,) because at and near these dates the temperature changes with its greatest rapidity. This remark applies to half-yearly means, as well as to daily temperatures; hence we can determine more precisely "the dates which divide the year into halves whose temperatures are each equal to the mean of the year," than the dates which are the centres respectively of the warmest and coldest halves of the year. For the former determination depends upon the dates at which the value of Y is equal to the mean annual temperature, while the latter depends upon the dates at which Y is a maximum or minimum. And the above phrase in inverted commas may be advantageously substituted for "centres of warm and cold halves of the year" in the definitions of E_1 in my former article.

We will now proceed to test our determinations of range and date by the Greenwich table.

The range, as measured by the difference between the warmest and coldest halves of the year, is by our theory equal to $A_1 \times 1{\cdot}2879 = 12{\cdot}44 \times 1{\cdot}2879 = 16{\cdot}05$. This is in error by about ·07, the warmest 182 days, April 22—Oct. 30, having a mean temperature of 57·00, and the coldest 183 days, Oct. 31—April 21, a mean temperature of 40·88, showing a difference of 16·12.

As regards date, the value of E_1 is 262° 31′ or 82° 31′, according as we make A_1 positive or negative. Subtracting 15° from the latter, to reduce to the beginning of the year, we have for remainder 67° 31′, the complement of which is 22° 29′, equivalent to 22 8 days; and half a year or 182·5 days, added to this, gives 205·3 days. The 22d and 205th days of the year are January 22 and July 24; hence, by our theory, January 22·8 and July 24·3 should divide the year into halves whose mean temperatures are equal to each other and to the mean annual temperature.

From the Greenwich table we derive the following half-yearly means:—

Jan. 23	—July 23,	both inclusive,	182	days, mean temp.	49·0
23	24		183		49·2
22	22		182		48·8
22	23		183		48·9

the mean annual temperature being 48·9. Hence our determination of date is only 1 day in error.

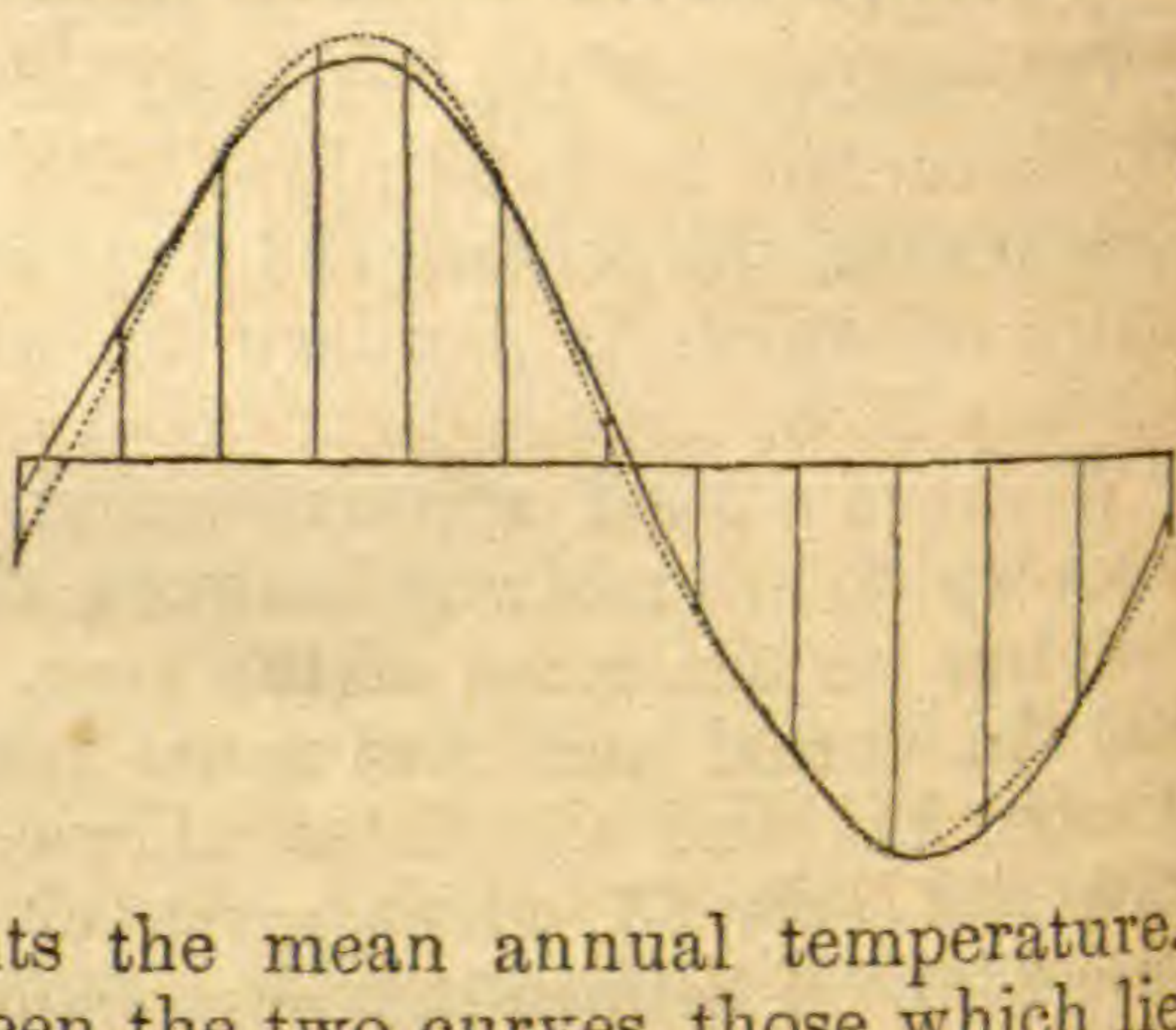
The centres of the warmest and coldest halves, as already stated, cannot be determined with so much precision. In the present case, the coldest 183 days are October 21—April 21, the centre of which is January 20, whereas our theory makes it January 22·8.

We have a good approximation, in the present case, to the dates which are midway between those whose temperatures are the same as the mean of the year. The latter, (which we may call the Vernal and Autumnal intersections,) are April 29 and October 21; and the dates midway between these are January 23·5 and July 25, which agree closely with January 22·8 and July 24·3. I believe this agreement will be generally found to exist, because the term t_1 is changing its value with nearly maximum rapidity at the dates of intersection, and thus overpowers the other terms, especially as t_2, the most considerable of them, cannot hasten or retard one of the intersections to any considerable extent, without producing an opposite and nearly equal effect upon the other.

The influence of the term t_2, in modifying the symmetrical curve $y=t_0+t_1$ for Greenwich, may be thus described:—It retards the vernal intersection, and the maximum; hastens the autumnal intersection, and the minimum; intensifies the maximum, and moderates the minimum. Hence the temperature rises higher above the mean than it falls below; but the number of days below the mean is greater than the number above. These features of climate will probably be found to prevail generally in extra-tropical latitudes; and they constitute the most marked departures of the curve of temperature from symmetry, the terms after t_2 being comparatively insignificant. In so far as they are common to all places, they are unimportant in the comparison of climates, which was the object proposed in my former paper; and, at all events, the first elements that claim attention are those in which the actual curve agrees with the symmetrical curve, that is to say, the elements of mean temperature, range, and general earliness, as represented by the three constants A_0 A_1 and E_1.

Of course, three numbers cannot express every feature of the curve of temperature at a place, for it is in the nature of things impossible that three numbers should express more than three independent elements. If the constants A_0 A_1 and E_1 are correct as measures of the three leading elements, it is all that we can expect of them; and if a closer approximation to the curve of temperature is desired, it can readily be obtained by computing the constants in one or more of the succeeding terms.

The subjoined diagram will give a clearer impression of the relation between the symmetrical and the actual curve than language can convey. The continuous line is the symmetrical curve for Greenwich; and the dotted line is a curve, drawn through points whose ordinates are the mean temperatures of the 12 months, beginning and ending with April, these points being situated on the vertical lines in the diagram.



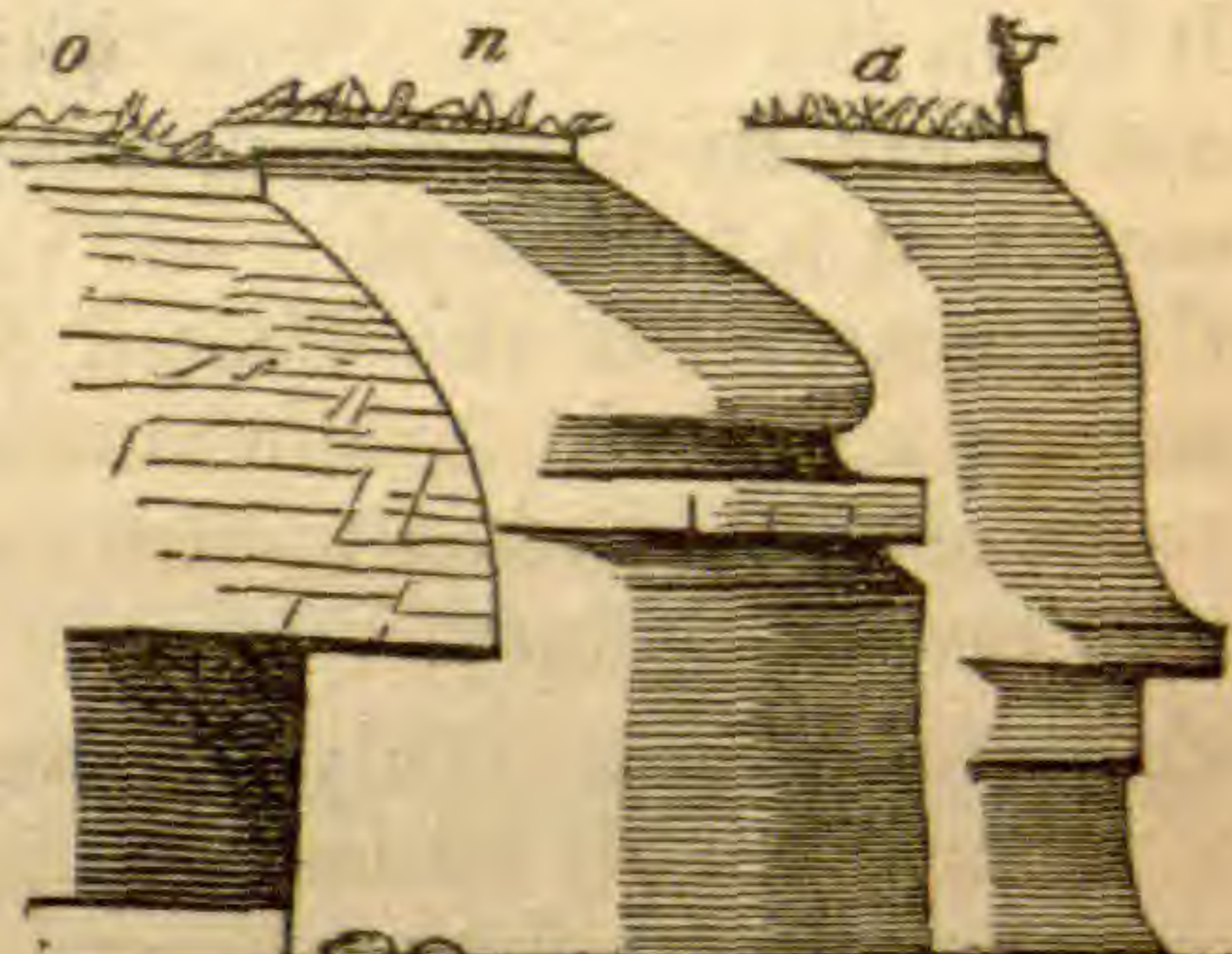
The horizontal line represents the mean annual temperature. Of the areas intercepted between the two curves, those which lie above either of the curves are together equal to those which lie below the same.

ART. XVII.—*On the Coal Measures of Cape Breton, N. B., with a Section;* by J. P. LESLEY.[1] (Communicated by the author.)

THE following section was obtained in August, 1862, from the cliffs between Lingan and Great Glace Bays, on the east coast of Cape Breton, from sixteen to twenty miles east of Sydney. Part of it was made out by means of a rope and ladder let down from the upper edge of the cliffs, where these overhung the sea, or occupied intervals between the short sand and gravel beaches. At the upper limit, which is also the northwestern end of the section, a square headland projects into the Gulf of St. Lawrence, along the axis of a synclinal basin with sloping sides of 4° or 5° dip. From this headland southeastward, the section was made out by an examination of each layer as it emerged from the sea, past the mouth of Little Glace Bay (where the new harbor is constructing, under the skillful and energetic direction of Captain William P. Parrot, Civil Engineer, of Boston, Mass.) and as far as to the mouth of Great Glace Bay.

	Feet.
Soft shales with a hard belt at the bottom, - - - -	20

These rocks cap the square headland projecting into the Gulf of St. Lawrence between the Burnt Head and Little Glace Bay. They are the highest Coal-measure rocks of this basin, and perhaps the highest Coal-measures south of Sydney Bay. The cliffs are about forty feet high, and exhibit a remarkable contour, caricaturing the human face in profile, by means of the overhanging ledge of hard sandrock at the bottom of the mass, and about half-way of the height of the cliff. See woodcut (*a*).



Red shale belt: red and green 10; red 10; green 2; red $1\frac{1}{2}$; green $\frac{1}{2}$; red $1\frac{1}{2}$; green $1\frac{1}{2}$, - - - - - - - -	27
Fire clay, the upper 5 feet crowded with small nodules of carbonate of iron; middle 2 feet sandy; lower 5 pure, - - - -	12
Red shale 2 feet, over 2 feet of fire-clay, under which runs the outcrop of a plate of carbonate of iron, from 4 to 8 inches thick, for hundreds of yards along the face of the cliff, - - -	4
Shales, with three black streaks, like the outcrops of coal beds, but mere discolorations of the shale: layers of small nodules of iron occur in the lower 10 feet; the lowest 2 feet are fire-clay, -	22
Sandstone cliffs 8 feet, over sandy shales 6 feet, - - -	14
Coal; good; on fire-clay passing down into, - - - -	1
Sandstone 6 feet; genuine black slate 2; fire-clay 8, - -	16

[1] Reprinted (with many changes and additions by the author) from the *Proceedings of the American Philosophical Society.*

Slate cliffs; the top rock of the great coal bed; varying in compactness, but essentially a homogeneous mass of finely levigated and foliated sandy mud, - - - - - - - - 40
Coal; the Hub Vein; slate 1½, soft coal 1½, solid 4, hard 1, - 8

Of this, only six feet is good workable coal, on the coast; but it increases westward, and with the omission of eighteen inches poorer top coal, yields from six to seven feet of good body coal. It is on this bed that the principal mines of the Glace Bay Company are situated, shipments, until lately, having been made from a long pier projecting into the sea, or in an open roadstead by lighters. Now, a railroad a mile long, on a sixty foot summit, lands the coal upon a long wharf occupying the north side of an artificial harbor, constructed out of the shallow Little Glace Bay. The old drift-works into the bed have been abandoned on account of the inflowing of the tide, and new works by a slope have been commenced a quarter of a mile inland.—The floor of the bed is not well seen, being covered by the ruins occasioned by the firing of the cliff coal, in the last century, by the French, after the fall of Louisburg, and the cession of Acadia to the British government. The miners report the sandstone, next to be described, as lying immediately beneath the coal.

Sandrock full of the moulds of plants, mostly stems, only occasionally blackened, - - - - - - - - - 20

This mass of building stone is a rare exhibition for these Coal-measures. It forms the long point on which the pier is built. Its thickness could not be exactly determined, because, like all the very sandy deposits of the section, it is false-bedded and variable. It is as true here as in the great Coal-measures of the United States that the principal masses of sandstone are reserved for the lowest parts of the formation. The great sandrocks of Cape Breton underlie all the productive Coal-measures, and are seen around Sydney.

Cannel coal bed. This is no true cannel but a coal shale, compactly foliated, highly bituminous, burning well, but with much ash, and crowded with fish-scales and minute shells. It varies, and sometimes reads thus; cannel, 8 inches; bituminous coal, 8 inches; clay, 1½ inch; bituminous coal, 3 inches, - - - - 1½
Fire-clay, - - - - - - - - - - 6
Cannel coal, as above, - - - - - - - - ½
Fire-clay, - - - - - - - - - - 2½
Sandstone cliff rocks 8 feet, over sandy shales 11 feet, - - 25
Cannel coal, or jet black slate; sometimes growing compact like cannel, but nowhere seen as a true coal, but rather a black fire-clay, one inch thick, with a few inches of black slate above and below it; plenty of fish scales, but no ferns, - - - - ½
Fire-clay 3; sandy clay 3, shaly 3, pure clay 4, blackish shales 8½, soft clay 2,—clay full of balls passing an eight inch plate of iron ore,—sandy shales 6, soft 3½, dark soft 1½, soft gray 8, - - 45
Sandstone shales 2, gray shales 5, blackish 1, gray 6, massive weathering flaky 2, sandy flaky 9, sandy cliff shales 11, blackish 10, sandy cliff shales 5, sandy shales 20, clay descending into sandstone 21. In this last occurs half an inch of coal, - - 92

Top slate, - - - - - - - - - - - 6
Coal; sometimes black slate with two inches of coal, - - ½
Fire-clay, - - - - - - - - - - - 5
Shales: blackish soft 4, gray 2, with poor sandy ball ore ½, gray 4, sandstone flinty 1, fire-clay, compact below, 6, sandy shales 6, yellow 6, gray 6, soft sandy gray 12, soft shales (nipped out) 5, false-bedded sandy shales, hard at top, soft at bottom, 17, - - 69½

This great mass of sandstone, thrown up at a steep angle, not by any general structural movement, but by original oblique deposition, has here resisted the wearing action of the waves, and left a curious and instructive promontory. The mass begins at the bottom with 3 inches of pure clay, under which is an inch of

Cannel coal which burns well and is full of fish scales.
Shales, soft yellow, concretionary, clay slates 7, harder 1, gray 2½, with iron nodules along its base, gray 4, soft blackish band ½, sandy foliated 3, top clay 1, gray, blackish ½ foot, - - - 19½
Cannel coal, or flaming slate ½, - - - - - - - 1
Hard shale ⅜, coaly matter half an inch, hard sandy shales 3, compact fire-clay 8, - - - - - - - - - - 11

These are the lowest rocks seen before reaching Little Glace Bay entrance, in the low banks, which fall off suddenly into the deep channel of the bay. A slight break in the section takes place here; it cannot be more than a few feet. The section commences again at the summit of the headland projecting from the south side of the bay, and runs thence uninterruptedly to the mouth of Great Glace Bay.

Soft measures under the soil, - - - - - - - - 10
Coaly top slate 4 inches, bituminous coal 4 inches, - - - ½
Sandrock variable 1, green clay with horses of sand ½, fire-clay 2, more compact 2, more sandy, becoming sandstone, 2, compact sandrock with thin flag-courses 7, - - - - - - 14½
Shale fire clay 3, in pencils 4, sandy compact 3½, in pencils 6, sandy 15, crumbling 4, - - - - - - - - - 35½

The profile of this mass is one of singular architectural beauty. See woodcut (p. 179). (*n*).

Sandrock 8, blackish shales and fire-clays 4, sandrock massive 10, sandy fire-clay 2, shaly sandstone with six inch courses 7½, dark shales 7½, flags 3, gray top shales 1½, - - - - - 43½
Bituminous slates with one inch of cannel in the midst, - - 1
Shale fire-clay 1, sandy 1, sandstone 2, sandy 11, fire-clay 5; the whole forming cliffs beetling over the breakers (woodcut *o*), - 20½
Coal. Harbor vein, - - - - - - - - - 5

Wrought by the inhabitants for many years in an entry from the beach. A new opening has been made on the outcrop where it crosses to the northwest side of the harbor below the new bridge.

Shales foliated, under which lies a plate of carbonate of iron three inches thick, sometimes breaking up into balls, - - - 8¼
Coals, with a centre streak of jet, perhaps characteristic of the bed, for it appears again in it at the new bridge, - - - - ⅔
Shales, red, green, yellow 7½, hard clay sandstone 2, clay shale 5, 14½

Coal. Regular bed of bituminous coal, - - - - - -	2
Sandy shales, foliated; then compact; then in half inch layers 26, sandstone then sandy shales 10, - - - - - - -	36
Shales, gray, blackish outside, 5, shaly fire-clay 10, - - -	15
Sandstone, greenish, 6, contorted 8; the local false-bedding has formed the point, and, like one or two other exposures along this coast, would throw a geologist completely off the track, leading him to suppose the country infested with high dips and faults; whereas, careful instrumentation has demonstrated an extraordinarily quiet and regular condition of things, - - - - -	14
Fire-clay 2, shales gray, green, harsh 4, gray, green 5, soft gray 20,	31
Sandrock in three equal layers, - - - - - - -	6
Soft fire-clay: top slate with nodules of ore, - - - -	10
Coal half an inch, black slate six inches, - - - - - -	½
Fire-clay, passing further on into red, green and yellow shales; then sandy 6, false-bedded shales 12, ferruginous fire-clay 2, hard blackish slates 8, - - - - - - - - - -	28
Sandstone, green, rough, shaly, passing into dark shales 12, beautifully false-bedded, scalloped in all directions like the blocks and faces of No. X (Upper Devonian) at the viaduct of the Conemaugh, in Cambria County, Pennsylvania;—thin massive sandstone 6 feet, - - - - - - - - - -	18
Shales, yellow sandstone at top, becoming yellow shales and then at bottom black, - - - - - - - - - -	20
Carbonate of lime and iron, a tight blue bed, - - - -	5

Sometimes 1½ feet thick, but will not average more than 10 or 11 inches. It forms a long reef into the sea, in the exact line of the distant headland. As a solitary specimen of this kind of rock in this section, it is all the more important to have it carefully traced inland. It rests on a green fire-clay full of nodules of ore, as large as filberts and walnuts, oxydized on the surface.

Blackish top slate, under which is a carbonaceous streak, - -	3
Shales (at the top sandstone balls a foot thick), yellow, then green and full of nodules of ore 11, soft fire-clay 1, yellow, then sandy, then clayey, then fire-clay 8, blackish fire-clay, then gray 10, -	30
Coal, - - - - - - - - - - - -	2
Fire-clay 2, with nodules of ore 2, blue shales 6, fire-clay full of nodules as large as chestnuts; the appearance of these fire-clays, crowded with nodules of iron ore, is very striking; their gnarly, knobby outcrops form long reefs visible by lines of breakers far out to sea.—Clays of various shades 12, blue black ½, red, yellow and green, - - - - - - - - - -	25½
Sandstone, false-bedded, then in layers 12, becoming clayey 4, blue fire-clay 5, - - - - - - - - - -	21

These are the last rocks seen at the north side of the mouth of Great Glace Bay. The whole thickness of rocks measured is as follows:—

North of Little Glace Bay, 471 feet }
South " " " " 436 " } in all 907.

Beneath these rocks lie formations of clay (including coal beds, one seven or eight feet in thickness), which form the west end of the long line of sea cliffs running out eastward from the Great Glace Bay bar. A measured section was made of these cliff-rocks also, which I propose to give at another time. It repeats a certain portion of the section given above, with interesting variations.

Our section of 907 feet of rock, commences at the headland in the centre of the synclinal and runs along the coast southward. Commencing at the same headland and running along the coast westward, a similar section may be obtained of the same rocks as they rise from the synclinal in that direction at the same low dip. Such a section would be from Cadougan's Creek, which corresponds to Little Glace Bay, to the mouth of Lingan Bay, which in like manner corresponds to Great Glace Bay. Many interesting variations in the metals would appear from such a comparison. While the general regularity and parallelism is remarkable, there are numerous minor irregularities; some fine instances of false bedding and local deposition; lenticular masses of sand separating adjacent mud-rocks; passages of shales into sandstones, and *vice versa;* gradual coalescing of scattered nodules of clay iron-stone into solid plates, or their gradual pervading of a thick bed of fire-clay, hardening it into so refractory a rock, that its outcrop forms a reef far out to sea. Instances occur of of the splitting of coal-beds. The Lingan bed, for example, has, on the sea-shore, a clay parting of half an inch, which in a quarter of a mile inland, thickens to nine inches; and then, in four hundred yards of gangway continued inland, thickens to nine feet, throwing the upper member of the bed entirely beyond the workings.[2] In this we have probably the explanation of the difference between the abandoned Bridgeport bed, on the south shore of Lingan Bay, and the Lingan bed on the north shore, separated by a wide and gentle anticlinal; the Bridgeport bed being but 7 feet thick, while the Lingan bed is 9.

The described section embodies the productive Coal-measures of the east end of Cape Breton, with five workable beds of coal, one of which can hardly be called workable in this area, whatever may be its character in others. In Mr. Brown's section of the North Sydney Coal-measures, there are enumerated, indeed, thirty-four coal-seams; but only four are said to be of workable thickness: Cranberry Head, 3·8 feet; interval (measuring downwards) 280 feet; Lloyd's Cove, 5·0; interval 730 feet; Main

[2] The *Cook Vein*, at Broad Top City in Pennsylvania, has a sandrock parting two feet thick, between two 2 foot beds of coal. At the present heading of the long drift, this rock, after first disappearing, leaving the bed of coal 6 feet thick, has increased to ten feet of tough rock, between two 6 inch beds of coal. This increase of ten feet takes place without crush in a distance of only three or four yards.

Seam, 6·9; interval 450 feet; Indian Cove, 4·8. Mr. Brown's whole section extends to a depth of 1860 feet, or along 5000 yards at a dip of 7° to the N. 60° E.

Mr. Brown "concludes from the best information in his possession that the *productive* Coal-measures exceed 10,000 feet," but I saw nothing in Cape Breton to justify the supposition. He grants that, "owing to several extensive dislocations, it is impossible to ascertion their total thickness with any degree of accuracy." I can only suggest, with deference to his long experience and acknowledged skill, that the structure of the east coast of Cape Breton has not been regarded from a right point of view, inasmuch as the coal-beds have been represented as members of one area, dipping broadside into the waters of the gulf; whereas, in fact, along that coast, they occur with alternate northeast and southeast dips, forming a series of basin-ends, the bodies of which lie side by side submerged beneath the gulf. The same four or five workable beds, inclosed in the same one or two thousand feet of *productive* measures, appear on shore at the west end of each of these basins. As the dip is commonly gentle, viz: from 4° to 8°, the basins sometimes coalesce; but in one instance at least, that of Cow Bay, the *south* dips are 45°, and the basin is sharp and narrow, greatly resembling the end of one of the anthracite basins of Pennsylvania. As at Sydney, and again at Glace Bay, so here at Cow Bay there are but four workable coal-beds in about 1500 feet of *productive* measures, and they are, no doubt, the Glace Bay beds.[3]

Sir William Logan, Sir Charles Lyell, Prof. Dawson, and other geologists who have described the Coal-measures of Nova Scotia and New Brunswick, agree in assigning to them an almost incredible thickness. "The entire section of the Joggins," writes Sir William Logan, "contains 76 beds of coal and 90 distinct Stigmaria underclays," with "24 bituminous limestones," in "a vertical thickness of 14,570 feet."

When we analyze the eight divisions into which this immense mass has been distinguished, we find them thus constituted:

Nos. 1, 2. Sandstones and shales; drift-trees and erect calamites, - - - - - - - - -	2267 feet.
No. 3. Sandstones; coal shales; underclays; 22 coal-beds,	2134 "
No. 4. Sandstones and shales, gray; bituminous limestones; 45 coal-beds; shells and fish-scales, - - -	2539 "
No. 5. Sandstones and shales, red; carbonized plants, -	2082 "

[3] The combined thickness of the Lower, Middle, and Upper Coal-measures, as determined by Mr. Jukes, in South Staffordshire, England, is 1810 feet. The thickness of the productive Coal-measures of Leicestershire does not exceed 2500 feet. In most parts of the deep anthracite basins, 2000 feet would be a fair average. In Western Virginia and Pennsylvania, and in the deepest parts of the Mississippi Valley areas, 1500 feet.

No. 6. Sandstones, ⅝; shales; bituminous limestone; 9 coal-beds; shells and fish-scales, - - - - -	2240 feet.
Nos. 7, 8. Sandstones, conglomerates, shales, nodular limestones, two beds of gypsum; remains of plants, -	2308 "
Interval, - - - - - - - - -	300 "
Massive limestone with *Prod. Lyelli* and other Lower Carboniferous fossils.	

It is very evident that the Sydney, Glace Bay, or Cow Bay section of less than 2000 feet of productive Coal-measures, can represent but barely one of these divisions, and that it must be either No. 3, or No. 4, or No. 6. Sir William Logan adds, in his resumé, that "Nos, 3, 4, 5, and 6,[4] contain the equivalents of the productive Coal-measures of Pictou and Sydney, and, in part, of the sandstones which separate them from the Lower Carboniferous series." Prof. Dawson describes minutely his own section of "2819 feet of the central part of the Coal Formation,"[5] in approaching which, after describing the lower parts,[6] he says: "We have now, after passing over beds amounting altogether to the enormous thickness of 7636 feet, reached the commencement of the true Coal-measures."[7] By the *true Coal-measures* he means, therefore, Division No. 4 and the lower part of Division No. 3, embracing less than 3000 feet of measures and containing but four coal-beds which can be called workable, the rest being from one inch to eighteen inches thick. In descending order we have:

Nine small seams in a thickness of measures of - -	536 feet.	
Main coal seam, 3·6; parting, 1·6; coal, 1·6, - -		5.
Three minute seams in an interval of - - -	75 feet.	
Coal, ·3; clay, ·5; Queen's vein, 1·9; shale, 4·4; coal, 1·0,		3.
Ten small seams (largest 1·2) in an interval of - -	762 feet.	
Coal, with three clay partings, - - - - - -		2¾.
Three small seams in an interval of - - - -	206 feet.	
Coal, - - - - - - - - - -		5.
Three small seams in an interval of - - - -	17 feet.	
Coal, - - - - - - - - - -		4.
Interval of - - - - - - - -	32 feet.	
Coal and bituminous shale, - - - - - -		5.
Eleven small seams in an interval of - - - -	1153 feet.	

The aspect of this section resembles those on the east coast of Cape Breton, where Modiolæ and fish-scales are also abundant.

The Albert or Pictou section is said also to contain but five or six seams of coal, two of which are of unusual thickness, as follows; From the surface, down the Success Pit, 73 feet; Main Coal, 39·11 feet thick; Interval, 157 feet; Deep seam, 24·9. Both these coal-beds, however, are far from presenting solid faces

[4] Dawson's Acadia, p. 178. [5] p. 177. [6] p. 127.
[7] Described in Proc. Geol. Soc., x, 1–42.

of coal. On the contrary, they are built up, like the 30 and 60 foot coal-beds of the Anthracite region of Pennsylvania, of many layers separated by underminings. The peculiarity here is that these separations are plates of ironstone, not more than six inches thick, instead of being layers of fire-clay, coal-slate, or sandstone. The structure is certainly peculiar, and convinces us of the quietness of deposit and of the long-continued stability of the sea-level.

But inasmuch as the 60 foot coal at Mauch Chunk, on the Lehigh, is identifiable with the Low Main or Mammoth bed of the Pottsville Basin to the west, and of the Beaver Meadow, Hazleton, Buck Mountain, and Wyoming Basins to the north of it, and through them with still smaller and separated beds further off in the Mahanoy and Shamokin Basins, and even with the bituminous basins of the Alleghany Mountains,—there can not be, *a priori*, a reasonable ground for doubt, that the 25 and 40 foot beds of Pictou are identifiable with 5 and 6 foot beds of New Brunswick on the one side, and with the 8 and 9 foot beds of Sydney on the other.[8] The paleontological unity of the Low Main coal of the Pittsburg region with the Low Main coal of Eastern Pennsylvania is no longer a matter of discussion. The structural evidence also is coincident and precise. Yet, wider intervals of Devonian and Silurian denudation are to be bridged by the theoretical connection *there*, than are called for between the coal areas of the British Provinces. The general bordering of the sea-coast with coal-beds, and the long and parallel stretches of Carboniferous rocks through the interior, are all cogent arguments for the continuity of the original coal areas, and therefore for the contemporaneity of the remaining portions of the coal-beds. As the same coal-beds which now cap the highest mountains of the Alleghanies in Northern Pennsylvania, and have been swept away over wide intervals of Devonian valleys between them, descend also into the depths beneath the beds of the lowest valleys drained by the Swatara, the Schuylkill, the Lehigh, and the Susquehanna North Branch, so I have no doubt the coal-beds, whose edges we now see only

[8] To illustrate in a still more striking manner this separation of a large bed into several smaller ones, one has only to examine Mr. Jukes's description of the Thick coal of Dudley, in England, "which, forming at that place *one* solid seam ten yards in thickness, becomes split up into *nine* distinct seams by the intercalation of 420 feet of strata over the northern area of the coal-field." The Main coal of the Warwickshire area is split up, according to Mr. Howell, into *five* beds by 120 feet of intervening strata. The Main coal of Moira is noticed by Mr. Hull as a third instance. (See Hull's Paper on the Carboniferous Strata of England, vol. xviii, No. 70, *Quar. Jour. Geol. Soc.*, p. 139.) Mr. Lesquereux, in his Report on the East Kentucky Coal Field, in the fourth volume of Owen's State Reports, p. 360, gives what he considers sufficient evidence of a similar breaking up of the Low Main Coal of the Pittsburg area into three. This is precisely the normal number of large beds into which the great Mauch Chunk or Mammoth Bed separates throughout the Pottsville-Tamaqua Basin.

along the sea-shore of Nova Scotia, or on the sides of the interior low lands, did once ride over the tops of its metamorphic Devonian mountains, whose summits, crowned with cliffs, opposing anticlinal and synclinal dips, remind the Pennsylvanian geologist, at every view he takes of them, of those mountains on which the coal still lies in fragmentary patches in his native State.

What, then, are the thousands of feet of rocks included in Divisions Nos. 5, 6, 7, and 8 of Logan's great section? In other words, the 7630 feet over which Dawson climbed to reach the bottom of his "true Coal-measures?"

What, I ask in reply, are those wide stretches of low, rolling, arable country, with a red shale soil, which the traveller sees spreading around all the productive coal areas of Cape Breton and Nova Scotia, especially the latter? To the geologist from the West they afford familiar scenery. He can hardly persuade himself, sometimes, that he is not riding through Lykens or Locust or Catawissa or Trough Creek Valleys in Pennsylvania, over the chocolate-colored soils of No. XI.[9] This formation, 5000 feet thick around the southern Anthracite coal-fields, becomes, indeed, thinner and thinner northwestward, until it is but 500 in the Alleghany Mountains, and not more than 50 beneath Pittsburg. But along its thickest line it extends from Alabama to New Jersey, a good thousand miles. It would not be surprising, then, to see it stretching another thousand miles further in the same direction, and spreading undiminished around the coal areas of Nova Scotia.

Division No. 5 of Logan's section consists of red shales and sandstones chiefly, 2012 feet thick. There is no reason why this should not be the representative of Formation No. XI, or of its upper part.

If it be objected that Division No. 6 is in fact a coal system with nine beds of coal and numerous bituminous limestones, the objection becomes an additional argument for the identification. For we see in this No. 6 the reproduction, at this immense distance, of the Lower or False Coal-measures of Virginia, where a *productive* coal system underlies the chocolate shales of Formation No. XI, and not only reappears, with workable beds, in Eastern Kentucky and Middle Tennessee, but projects itself, in a recognizable shape, through Western Indiana nearly to Chicago, and through Middle Pennsylvania nearly to the Delaware River. In fact, Lesquereux pronounces the whole coal of Arkansas to belong to this lower system. It may therefore, very well be found in force in Nova Scotia. Throughout Division No. 6 no bed of respectable size is mentioned. It is an early and imperfect system.

[9] The numbers of Formations, used in these pages, are those originally used in the Reports of the Geological surveys of Pennsylvania and Virginia. Prof. Rogers has since then given them.

The chief objections to the hypothesis above sustained will come (1) from the absence of any general representative for the Millstone grit or Great Basal conglomerate of the True Coal-measures; (2) from the sub-position of Divisions 7 and 8, 2308 feet of sands, pebble-rocks, and limestones; and (3) from the presence at a still lower depth of what seems to be the genuine, massive, Subcarboniferous limestone. To break the full force of these objections, I can only remark, (1) that the Pictou coal-basin *has* a massive conglomerate under its productive Coal-measures, while elsewhere no one formation of the whole Palæozoic System is so variable and unreliable and unidentifiable as Formation XII, the Great conglomerate, technically so called; (2) that Nos. 7 and 8 may be identified with Formation X; and (3) that the Subcarboniferous or Archimedes limestones of the Western United States not only have been subdivided into five separate formations in the Valley of the Mississippi, but wholly thin away and disappear before crossing the Schuylkill and Lehigh Rivers on their way to Nova Scotia. Therefore, although the False or Lower Coal-measures of Virginia and Southwestern Pennsylvania are *overlaid* by limestones with Subcarboniferous fossils, the connection, *as to limestone*, is entirely cut away between them and the Nova Scotia deposits, so that the massive gypseous limestones of Nova Scotia may be at any assignable lower level. This argument is rendered all the more forcible by the fact that gypsum is unknown in the United States, except in one or two anomalous positions, apparently connected with the Lower Silurian limestones, and in the closed basin of Michigan.

Beneath the red shale Formation No. XI, we have, in the southeastern ranges of the Appalachians, nearly three miles' thickness of sedimentary deposits, separable everywhere into three great formations: No. X, white sandstone, 2000 feet, No. IX, red sandstone, 5000 feet, No. VIII, green and olive shale, 8000 feet; the white sandstone including rarely a thin bed of conglomerate here and there, and traces of coal-plants and even thin coal-beds; the red sandstone passing downwards into red shale, and often alternating flinty sandrock with massive mud-rocks even in the upper part; and the olive shale becoming near the base of it rocky, and even mountainous in the region of the Juniata, where a system of thin coal-beds was also developed in the midst of the sandstone and shale. The white sandstone of No. X becomes, in the Alleghany Mountain belt, less than 800 feet thick, and is there characterized by thin-bedded and very irregularly cross-bedded sandstones of a peculiar greenish tint and harsh, rough fracture, weathering to a surface sprinkled with small red dots of peroxyd of iron.

It is not too much to say that a geologist well accustomed to these formations, along their great Appalachian belts of mountain and valley, stretching from the Appalachicola and Alabama Rivers in the South, to the Delaware and Hudson in the North, cannot fail to recognize them and distinguish them anywhere. The *tout ensemble* or *facies* of each is *sui generis.* Fossils may come in afterwards as a satisfactory confirmation; but the eye has already determined the respective formations. Even in the West, where Formation IX has dwindled, like Formation XI, to an insignificant one or two hundred feet, and scarcely separates the green sands of X from the green shales of VIII, the characteristic features of the three formations, although modified and harmonized by the preponderance of the argillaceous element, are still in sufficient contrast to be recognized when fairly seen.

To an eye thus trained among the broad outcrops of the Lower, Middle, and Upper Devonian of the Appalachians, it is evident that the mountains of Cape Breton and the hills of Northern Nova Scotia, surrounding or intervening between the already-mentioned red shale borders of the coal areas, are composed of these formations. True, the anticipation of finding these formations has a tendency to warp the judgment and delude the eye, especially when that anticipation is based upon such a probability as this: that a mass, three miles thick and a thousand miles long, will maintain its thickness (and of course its topographical height and geographical breadth) at least as far along the prolongation of its isometric axis (to use Mr. Hull's new and much-needed term), as will such minor formations as the Coal over it or the Upper Silurian limestones under it. In other words, if analogies between the Nova Scotia and the United States coals compel us to consider them synchronic, if not originally conterminous; and if the Clinton fossils of New York, and even the Dyestone[10] iron ore of Pennsylvania, Tennessee, and Wisconsin, be found at Arisaig, and along a well-defined outcrop in the direction of Truro; surely the Second Mountain, Little Mountain, Orwigsburg Mountain, and Summer Hill, upon the Schuylkill River, must be represented by the Antigonish Mountains of Nova Scotia, and by the Sydney and St. Peter's Range in Cape Breton: and this, whether the Nova Scotia Carboniferous rocks or Subcarboniferous limestones be deposited upon the Devonian conformably or unconformably. The Province is in fact a wide belt of mountains partially submerged; and may have been to some extent in the same condition at the beginning of the Coal era. In the Antigonish Hills we may have principally Formation VIII, while in the country south of the Lake Bras d'Or we may have the full series of VIII, IX, and X. The Arisaig formation, with fossils once thought by Hall and Lyell to be Hamilton and Chemung, and now consid-

[10] Described by Dawson, p. 58, supplementary chapter to *Acadian Geology*, August, 1860.

ered by Hall and Dawson to be indisputably Clinton, although overlaid and concealed along most of its extent by apparently nonconformable Coal measures, gives us a fixed lower limit for the so-called metamorphic hill country of the Province, which makes this hill country necessarily Devonian, or Formations VIII, IX, and X. Even if we object to the term Devonian, and permit the paleontologists to carry down the term Carboniferous, or the term Subcarboniferous, step by step, so as to include first, Formation X, perhaps rightly, and then the genuine Old Red IX, and even, as the effort is in the Western States, to include Formation VIII down to its black shale beds with coal, the change of term will not change the lithology,—the mountains of Nova Scotia must still be the representatives of the Catskill, Mohantongo, Terrace, and Alleghany Mountains of New York and Pennsylvania.

The eye can hardly be mistaken in the features of the roadside banks between Antigonish and Merigonish; the road defiles through hills of VIII. Equally certain is it that the outcrops on the road from St. Peter's to Sydney are of the reddish and greenish rocks of IX and X. The road for forty miles winds along the lake shore, and in and out of ravines descending from a group of parallel mountains of these formations, made parallel by a system of parallel anticlinal and synclinal curves which issue from the lake and throw the mountain dips to the north and to the south alternately, at angles from 5° to 45°. Great rib-plates of flinty sandrock rise to the summit and form tablets with broken cliffs upon the outcrop side, fine objects seen thus against the sky. The mountains at the head of the east arm of the lake, and those on its northern side forming the peninsula, come down upon the shore in the same style, and belong to the same system. On the south side of Miré Bay, in the ravines east of the Gabarus road bridge, there is no mistaking the aspect of masses of slates of No. VIII standing at 45°; nor can one be convinced that he is not riding through a forest grown on a soil of IX, as he is whirled over the fine old road from Miré bridge to Louisburg, although the highest elevation of the plateau is but 350 feet.

Whatever impression the Devonian and Subcarboniferous sediments of Nova Scotia and Cape Breton may make upon a geologist from the Middle States, certainly his wonder will be piqued by striking analogies between the exhibitions of the workable Coal-measures at two such distant places as Sydney and Pittsburg. The resemblance is more than general; it has special points.

At Pittsburg there are about a thousand feet of Coal-measures (to the top coal), with a great bed 8 or 10 feet thick near the top, a 6 foot bed half way down, two small workable beds in the lower half of the column, and a large bed (4 to 8 feet) at the bottom.

At Sydney (Glace Bay), in like manner, there are about a thousand feet of Coal-measures, with an 8 or 9 foot bed towards the top, a 6 foot bed half way down, two smaller beds in the lower half of the column, and a 7 or 8 foot bed near the bottom.

At Pittsburg, as at Glace Bay, the upper 18 inches or 2 foot of the high Main coal is rejected.

At Pittsburg, as at Glace Bay, the middle 6 foot coal (Upper Freeport of the Alleghany River and Cook Vein of Six Mile Run) is famous for its solid face and excellent quality.

No one should admit that such coincidences furnish a demonstration of identity. But it must not be overlooked that the beds of the Pittsburg area have been traced and identified from end to end of areas with a diameter, in all, of over a thousand miles, even across the denuded interval of Central Kentucky. The expectation may, therefore, be pardoned, not as an amiable enthusiasm, but as a logical inference, that when the fossil groups of the individual beds of Cape Breton shall have been thoroughly studied by Lesquereux and other competent botanists, their identification with the beds of the West may be made somewhat more than possible. The zone of sediment, when taken along its isometric axis, is equal enough over *a priori* incredible distances. Logan and Hunt and Murchison are finding the Quebec group and the Huronian and Laurentian systems in Scotland and Scandinavia, not by fossils, but by aspect. No one doubts the extension of the Millstone grit and the Mountain limestone of England to Pennsylvania. Why should the remarkably homogeneous and continuous Flora of any one of the immensely outspread beds of the United States not be homogeneously continuous to Rhode Island, New Brunswick, and Cape Breton?

One remarkable feature, however, in this resemblance of the two coal columns at Pittsburg and Sydney, must not be forgotten. I refer to the mass of red shales which cap the Glace Bay section. A similar deposit occurs, at a fixed horizon, widely spread over Western Pennsylvania, but *beneath*, not *above*, the High Main coal.

Note on Mr. Lesley's Paper on the Coal-measures of Cape Breton; by J. W. Dawson, Principal of McGill College, Montreal.[11]

The new facts and general considerations on the Nova Scotia coal-field contained in Mr. Lesley's paper, are of the highest interest to all who have worked at the geology of Nova Scotia. I think itmy duty, however, to take exception to some of the statements, which, I think, a larger collection of facts would have induced

[11] This note was read by Professor Lesley before the American Philosophical Society, and is published in the same number of its Proceedings.

Mr. Lesley himself to modify. My objections may be stated under the following heads.

(1.) It is scarcely safe to institute minute comparisons between the enormously developed Coal-measures of Nova Scotia, and the thinner contemporary deposits of the West, any more than it would be to compare the great marine limestones of the period at the West, with the slender representatives of the part of the group to the eastward.

(2.) There is the best evidence that the Coal-measures of Nova Scotia never mantled over the Devonian and Silurian hills of the Province, but were, on the contrary, deposited in more or less separate areas on their sides.

(3.) Any one, who has carefully compared the Coal-measures of the Joggins with those of Wallace and Pictou, must be convinced of the hopelessness of comparing individual beds, even at this comparatively small distance. *A fortiori,* detailed comparisons with Pennsylvania and more distant localities must fail.

(4.) I do not think that any previous observer has supposed that the coal-measures of Eastern Cape Breton represent the whole of the coal formation of Nova Scotia. The "Upper Coal-measures" of my paper on Nova Scotia are certainly wanting, and probably the Sydney coal-field exhibits no beds higher than the middle of No. 4 of Logan's section at the Joggins.

(5.) The whole of the coal-beds at the Joggins belong to the *Upper* and *Middle* Coal-measures. It is quite incorrect to identify No. 6 of Logan's section with the *Lower* Coal-measures. These do not occur at the Joggins, but are found in Nova Scotia, as in Virginia and Southern Pennsylvania, at the base of the system under the marine limestones. The Albert beds are the equivalents of these Lower measures, and not of the Pictou coal. In my paper on the Lower Carboniferous Coal-measures (*Journal of Geological Society of London*, 1858), will be found a summary of the structure of the Lower Coal-measures, as shown at Horton Bluff, and elsewhere. The term "true-Coal-measures," quoted by Mr. Lesley, does not mean in my description, the Middle Coal-measures, but merely that part of them holding the workable coal-seams.

(6.) Whatever may be the value of Mr. Lesquereux's applications of the fossil flora to the identification of coal-seams in the West, I am prepared to state, as the result of an extensive series of observations, still for the most part unpublished, that in Nova Scotia, the flora is identical throughout the whole enormous thickness of the Middle Coal-measures, and that the differences observable between different seams are attributable rather to difference of station and conditions of preservation, than to lapse of time. It is, indeed, true, as I have elsewhere explained, that the assemblages of species in the Lower, Middle, and Upper

Coal-measures may be distinguished; but within these groups the differences are purely local, and afford no means for the identification of beds in distant places.

(7.) I do not desire to offer any opinion on the questions raised by some American geologists, as to the extension of the term Carboniferous to the Chemung group; but I know as certain facts, that the flora of the Lower Coal-measures, under the marine limestones and gypsums of Nova Scotia, is wholly Carboniferous, and that the *flora*, on which alone I consider myself competent to decide, of the Chemung of New York, as now understood by Professor Hall and others, and also of the groups in Pennsylvania named, by Rogers, Vergent, and Ponent (? IX and X of Mr. Lesley), is as decidedly Devonian, and quite distinct from that of the Carboniferous period.[12]

For Mr. Lesley's ability as a stratigraphical geologist, I have the highest respect; and, with reference to the present subject, would merely desire to point out that he may not have possessed a sufficient number of facts to warrant some of his generalizations, on which in the meantime I would, for the reasons above stated, desire geologists to suspend their judgment.

J. W. DAWSON.

McGill College, Montreal, February 18th, 1863."

Mr. Lesley remarked that he read this communication of his friend, Dr. J. W. Dawson, with great pleasure, as it would prevent any mistake about the nature and importance of the discussion, and any undue weight being attached to his own suggestions; that no one was more convinced than himself that there could be no excuse for dogmatism where so little was known, and, therefore, that he had intended rather to suggest than to defend those opinions expressed in his paper, which had drawn down so earnest and valuable a caveat from so high a source. To defend them would require long and systematic researches on the ground, if, even then, the too easily accepted present standpoint of paleontology would not hide the truth from view behind immovable obstacles. So long as apparent specific identity in organic forms continues to be accepted as the supreme test of stratigraphical horizon, discord is inevitable. When paleontology is prepared to return under the mild dominion of her mother, lithology, which she has at least one-half repudiated, geology will advance more rapidly in her work.

Dr. Dawson's first objection is a begging of the very question, whether the Coal-measures of Nova Scotia *are* "enormously developed." That, in one spot of the earth's surface like Nova

[12] See paper on Devonian Flora of Eastern America, *Quar. Jour. Geol. Soc. Lond.*, November, 1862. Also *this Journal*, May, 1863.

Scotia, and that too midway between the great coal areas of America and those of Europe, wherein the thickness of Coal-measures proper range from 2000 to 5000 feet, if they even attain the latter size, there should be an anomalous deposit of 25,000 feet, is incredible.[13] What the great Bohemian paleontologist, by unerring instinct, said to us after our thirty years' war over the Taconic system, *there must be a mistake somewhere*, I must repeat to those who so "enormously develop" the Nova Scotia Coal-measures. And my intention in the paper on Nova Scotia coal was only to suggest one formula on which the error might be discussed. I distinctly repudiated the safety of instituting "minute comparisons.". My comparison of the Cape Breton coals and the column at Pittsburg was carefully made in the most general manner, and the resemblance called a coincidence. But the value of the comparison remains; for it affords a new argument in favor of the *family likeness* of those parts of the general Coal-measures of different countries, which have a right to the specific title of "productive coals." The argument also remains good, that, if 2000 feet of Coal-measures in Missouri can be recognized in 2000 feet of Coal-measures in Kentucky, Virginia, and Eastern Pennsylvania, the very same system of beds, bed for bed, being demonstrated first by stratigraphy, and then by paleontology (and such is the fact), why not in Nova Scotia? Even granting (3) that sufficient skill and care and opportunity combined have hitherto failed to identify the coals of the Joggins with those of Wallace and Pictou, there is still hope at the bottom of the box. Before Lesquereux undertook the study of the slack-heap at the mine's mouth, our own identification of individual beds was very imperfect, and the search for a complete system of identification had been abandoned with the same sense of hopelessness. But how is it now? There certainly may be special difficulties in Nova Scotia; there are such at Pottsville, and in Michigan; but they are exceptions which prove the rule, instead of affording an *a fortiori* argument against it.

I have no doubt that some of the Coal-measures of the British Provinces may have been "deposited in more or less separated areas on the sides of the Devonian and Silurian hills," as Dr. Dawson says (2). But I confess to a complete scepticism of the great extent which has been assigned to this unconformability of the Coal-measures upon the lower rocks; first, because most of the Island of Cape Breton, and much of the

[13] We have received a note from Dr. Dawson, written after he had seen the above remarks of Prof. Lesley, in which he says he never claimed any such thickness as 25,000 feet for the Coal-measures proper of Nova Scotia, but that the actual measurements of Sir Wm. Logan, carefully revised by himself, gave at one place the truly enormous thickness of nearly 10,000 feet, and that it is to this that his remarks apply. See Dawson's *Acadian Geology*, pp. 117 and 177.—Eds.

surface of Nova Scotia and New Brunswick are confessedly unstudied and almost unknown; secondly, because the incredible thickness assigned to the Coal-measures throws doubt upon the positions assigned to the non-conformable horizons; thirdly, because the coal-beds themselves stand almost vertical in many places round the shores; fourthly, because the mountains of Nova Scotia, with apparently conformable Carboniferous limestones, have apparently an Appalachian structure and aspect, have suffered vast denudation, exhibit cliff outcrops and section ravines, and may just as well have carried coal upon their original backs, as we can prove that our Tussey, Black Log, Nescopec, Mahoning, Buffalo, Tuscarora, Brush, and other Silurian and Devonian mountains did. There is an immense non-conformable chasm in the column west of the Hudson River, and the Catskill Mountains over it have no coal upon their backs; but the coal comes in regularly enough on them at the Lehigh, (a less distance than from Sydney to St. Peters, or from Pictou to Windsor,) and the unconformability in the Upper Silurian and Devonian has already disappeared.

Dr. Dawson's fourth objection would be good, if I had really "supposed the Coal-measures of eastern Cape Breton to represent the whole of the Coal-measures of Nova Scotia." But I only suggested that they may be the equivalents of the system of *productive Coal-measures;* that is all. Between the Monongahela and the Ohio, our column of productive coals is capped by another of barren shales and soft sandstones of unknown height, by one estimate 3000 feet thick; and part of this column may represent the so-called Permian measures, which, in Kansas, cap conformably the Coal-measures. Having no knowledge of the fossils, I have no desire to oppose the conclusions of Professor Dawson, as to the part of the column of the Joggins to which the Glace Bay coals apply, but hope that his accurate handling of them will secure some certainty about it. It was the grouping of the beds, and not the fossils, which I wished to bring into prominent notice; because the doctrine of isolated basins, when unfounded, or overapplied, is as injurious to lithological truth, as the careless identification of surface aspect may at any moment prove to paleontology. I willingly leave to accomplished paleontologists, like Professor Dawson, the discussion of the grand generalization embodied in his sixth objection; but I may be permitted to believe that it has had its birth in the doctrine of isolated basins, and that the two must stand or fall together. It also seems to me to involve radical inconsistencies; for, if I comprehend it, it asserts, 1. That the flora of the whole coal-measures (25,000 feet?) is identical; that is, the vertical distribution of each and all the plants is complete from the bottom to the top. 2. That, nevertheless, there are differences observable between different coal-beds. 3. That these are attributable rather to difference of station and conditions of preservation, than to lapse of time; that is, if we could take the beds, each one in its whole extent, and its fossils in their original condition, there would, after all, be no differences observable between different seams. 4. That groups or assemblages of species in the Lower, Middle, and Upper Coal-measures may nevertheless be distinguished; that is, while each and every species may be found occasionally in all parts of the column from bottom to top, yet this happens in such a manner as to group some of them more abundantly, or in certain peculiar proportions in the Lower, others in the Middle, and others in the Upper portions of it. 5. That, after all, however, these groups are not persistent, but differ at different localities, and are as worthless as the specific forms themselves for the identification of a single bed in more than one place.—Is it possible that all this has been made out, or *can* be made out, except in a country of *horizontal* Coal-measures, well opened for study, where the stratification can be established beforehand, and the range of the fossils be made certain?

In conclusion, I would say, that the want of clearly defined and applied names is a drawback to such a discussion. The discussion is, in fact, *initially* one of names, viz: how far down the name Carboniferous must be carried; what are the Lower Coal-measures, &c. But, *in the end*, it is a question of vital importance to the value of the paleontological *imprimatur* upon stratigraphical and structural deductions from field work. Is the discovery of specific forms to keep all our geological *niveaux* in a perpetual mirage-flicker? Are we never to know, from day to day, whether we are at work in Devonian or Carboniferous, in Trias, or Lias? Why not at once obey the marriage law of the weaker sex, and give up our names for our lords? Let geology forget the virgin nomenclature of her youth, and rewrite her books with such titles for her chapters as these: "The Spiriferiferous formation; The Lepidodendriferous formation; The Lower Thecodont; The Middle Baculite; The Upper Pterodactylian formation. Why has this not already been done? Simply because it cannot be done. No paleontologist has yet been bold enough even to propose it. Yet, as I believe, the 25,000 feet of Coal-measures in the British Provinces will be found to be one of the many unconscious *realizations* of this idea, when no one can be found to *nominate* it openly. The whole Paleozoic system, at its thickest place, in southeast Pennsylvania and middle Virginia, is but 35,000 feet. It is not unreasonable then to *suggest*, if not to affirm, that the vast column of so-called Coal-measures in Nova Scotia will take in all that part of the Paleozoic column which has furnished coal, and that is from the top downwards nearly to the Upper Silurian.

ART. XVIII.—*Hydraulics of the Report of Humphreys and Abbot on the Mississippi River;* by Prof. F. A. P. BARNARD. (Continued from p. 37.)

FOR the solution of most problems in practical hydraulics, it is necessary to establish the relations which exist between the cross-section of the stream, its mean velocity, and the slope of its surface. As a basis of this investigation, it is assumed by the authors of this report, as by other writers on the subject, that the condition of uniform motion is expressed by equating the accelerating force with the resistances. One side of the equation presents no difficulty; it is simply the expression for the force of gravity. The other requires consideration.

The authors of the report reject the idea that the cohesion of the particles of the liquid among themselves enters as an element into the resistance of the liquid to motion. They hold that this cohesion is concerned only in determining the distribution of the resistance through the mass; but that the resistance itself is simply the adhesion of the liquid to its bed. It is unnecessary to stop just here to discuss the question by what name it is most fitting that the resistance to flowing water shall be called. It is quite sufficient, if we agree that were the resistances, irregularities, and obstructions to motion, at the surfaces of contact between the water and the earth or the superincumbent air, to be totally annihilated, the whole body of water would descend with a uniformly accelerated velocity, as a solid descends an inclined plane; and there would be no subsurface curves. The resistances therefore come from the perimeter, and must be proportional to it, and to the length of the channel considered. In the perimeter, the results of this survey go to prove that we must include that part which is in contact with the air, as well as that which bounds the cross-section beneath the water. The question how there happens to be a resistance at the surface, and what is the cause or what are the causes producing it, is a question to be considered by itself. For the moment we accept the fact, as experiment has established it. The resistances must then be proportioned to the perimeter and length of channel; and, also, because, when there is no motion, there is no resistance, to some function of the mean velocity at the surfaces in contact. Now, if we put

a=cross-section of the river, W=width,

p=wetted perimeter, $r=\frac{a}{p}$=mean radius, or hydraulic depth,

l=length of channel considered, h=total head or difference of level,

$h_{,}$=part of head balanced against ordinary resistances of the channel,

$h_{,,}$=part of head neutralized by bends, and irregularities, G=specific gravity of the water, g=measure of the force of gravity,

$s=\frac{h_{\prime}}{l}$=slope of surface expended against ordinary resistances,

$\frac{h_{\prime\prime}}{l}$=slope expended against bends, &c. V, U, v, b=same values as before,

we shall have, for the accelerating force of gravity, the expression, *Ggals*, and, for the resistances at the perimeter, the expression,

$$l(p+\mathrm{W})\varphi\left(\frac{\mathrm{U}_0\mathrm{W}+\mathrm{U}_r p}{\mathrm{W}+p}\right),$$

which two expressions are to be put equal to each other. In a very large river, W and P are very nearly equal, though p of course always exceeds W. The expression may be simplified by putting them equal, and no appreciable error will result. Also, G and g are constants, and l is a factor common to both sides. Dividing by these, and putting p=W in the fraction, there will result the equation,

$$as=(p+\mathrm{W})\varphi\left(\frac{\mathrm{U}_0+\mathrm{U}_r}{2}\right);\quad \text{or}\quad \frac{as}{p+\mathrm{W}}=\varphi\left(\frac{\mathrm{U}_0+\mathrm{U}_r}{2}\right).$$

If we substitute the values of U_0 and U_r given above, the equation simplifies itself immediately to the following:[1]

$$\frac{as}{p+\mathrm{W}}=\varphi(0{\cdot}93v-0{\cdot}167(bv)^{\frac{1}{2}});\ \text{which put}\ =\varphi(z).$$

Let $\frac{a}{p+\mathrm{W}}$ be denoted by $r_{\prime}$=the radius of the entire perimeter: the expression then becomes

$$r_{\prime}s=\varphi({\cdot}93v-167(bv)^{\frac{1}{2}})=\varphi(z).$$

This preliminary equation differs from that which is usually assumed, in two particulars. First, the radius of the entire perimeter, $r_{\prime}$ is employed instead of r, the hydraulic mean depth (to which, however, it is in nearly a determinate ratio); and secondly $\varphi(z)$ is used instead of $\varphi(v)$, or a function of the *mean velocity at the perimeter*, instead of a function of the mean velocity of the river itself.

[1] In stating this expression, the authors have committed a sort of mathematical solecism, in arbitrarily changing the sign of the second term, to guard against the subsequent occurrence of what they conceived to be an inconsistency. We shall see that the apprehension was unfounded. The change of this sign, in fact, contradicts the original hypothesis. For the quantity under the symbol φ is the mean of the velocities at the upper and lower boundaries of the mean vertical plane; and this can never, by any possibility, be greater than the mean velocity in the same plane, which is ${\cdot}93v$; nor even equal to it, except in the case in which the parabola becomes a straight line, and v=0. Therefore, since $(bv)^{\frac{1}{2}}$ has been necessarily taken throughout as essentially positive, the written sign before it must be negative.

In order to determine the nature of the function $\varphi(z)$ and the constants which must enter into it, a collection was made of all the available data which had been furnished by the survey, or which could be gleaned from the publications of other observers; embracing thirty examples of area, width, wetted perimeter, maximum depth, mean velocity and slope of streams varying in magnitude from the dimensions of the Mississippi at high water, down to those of a small canal. In regard to slope, it was to be considered that a portion is expended in overcoming the irregularities of the channel and the changes of cross-section: and a portion, in compensating for the loss of living force at bends. It is only what remains after these effects have been subtracted, which constitutes the equivalent of the resistance of a straight and regular channel. The effect of bends must be provided for in an independent formula; and the amount of slope neutralized by them, deducted from the total slope observed. Irregularities and changes of cross-section in the channel are governed by no law, and therefore cannot directly enter into the formula; but they produce a mean effect, which is provided for in the modification which they introduce into the constants which are derived from observation, on the supposition that, after bends have been allowed for, the channel is straight and regular, and the movement in it uniform. The method pursued by most writers, of putting $\varphi(v)=Av+Bv^2$, and then seeking values for the indeterminate coefficients which shall most nearly represent the observations, was tried by the authors of the report, making

$$r_{,}s=Az+Bz^2, \quad \text{or} \quad \frac{r_{,}s}{z}=A+Bz,$$

in which $\frac{r_{,}s}{z}$ and z are co-ordinates in the equation of a straight line; but they found that a straight line would not represent the observations, and that the involution of z produced expressions of troublesome complexity. They then put

$$r_{,}s=Cz^2, \quad \text{or} \quad C=\frac{r_{,}s}{z^2}$$

and plotted the values of C as ordinates to $r_{,}$, s, and v. The plots with $r_{,}$ and v produced irregular curves following no apparent law. That with s was quite regular. It was inferred therefore that C is some function of the slope. After a very long series of trials, with a view to discover this function, the expression

$$C=\frac{s^{\frac{1}{2}}}{195}$$

was adopted, as most satisfactorily fulfilling the required conditions. Substituting this, therefore, in the formula, it becomes

$$z=(195r_{\prime}s^{\frac{1}{2}})^{\frac{1}{2}}=\left(\frac{195as^{\frac{1}{2}}}{p+\mathrm{W}}\right)^{\frac{1}{2}}.$$

From this are deduced values for each of the variables in terms of the rest (regarding $p+\mathrm{W}$ as a single variable), viz:

$$s=\left(\frac{(p+\mathrm{W})z^2}{195a}\right)^2,$$

$$a=\frac{(p+\mathrm{W})z^2}{195s^{\frac{1}{2}}},$$

$$p+\mathrm{W}=\frac{195as^{\frac{1}{2}}}{z^2}.$$

Instead of $p+\mathrm{W}$, may be put, without appreciable error, for rivers, 2·015 W. Resuming the value of z, viz:

$$z=0{\cdot}93v-0{\cdot}167b^{\frac{1}{2}}v^{\frac{1}{2}}=(195r_{\prime}s^{\frac{1}{2}})^{\frac{1}{2}},$$

and solving with respect to $v^{\frac{1}{2}}$, we obtain

$$v^{\frac{1}{2}}=-\sqrt{0{\cdot}0081b+(225r_{\prime}s^{\frac{1}{2}})^{\frac{1}{2}}}+0{\cdot}09b^{\frac{1}{2}},$$

and $$v=\left(-\sqrt{0{\cdot}0081b+(225r_{\prime}s^{\frac{1}{2}})^{\frac{1}{2}}}+0{\cdot}09b^{\frac{1}{2}}\right)^2$$

The negative value of the radical is that which it is necessary to take, in order to fulfil the condition that v shall become zero when s is zero.

For rivers, the value of b, as heretofore given, is 0·1856. The term containing it under the radical will have only the value ·0015, and may ordinarily be neglected. The expressions for the several variables will then become

$$v=(0{\cdot}0388-(225r_{\prime}s^{\frac{1}{2}})^{\frac{1}{4}})^2=\left(0{\cdot}0388-a^{\frac{1}{4}}\left(\frac{225s^{\frac{1}{2}}}{p+\mathrm{W}}\right)^{\frac{1}{4}}\right)^2$$

$$r_{\prime}=\frac{(v^{\frac{1}{2}}-0{\cdot}0388)^4}{225s^{\frac{1}{2}}} \qquad s=\left(\frac{(v^{\frac{1}{2}}-0{\cdot}0388)^4}{225r_{\prime}}\right)^2 *$$

If Q represent the amount of discharge per second, then

$$v=\frac{\mathrm{Q}}{a}, \text{ and } a=\frac{\mathrm{Q}}{v}.$$

If Q be given, along with any two of the foregoing variables, the rest may be computed by the help of this equation, unless the two given at the same time are v and a.

In estimating the effect of bends, the authors found the

* In the last two formulæ, the second term of the numerator has the negative sign, where the authors of the Report have made it positive. This difference results from our having chosen not to adopt the change of sign in the value of z, introduced by the authors, as explained in the note on p. 198. The two formulæ above are the only ones in which the original difference of proceeding involves any difference of final values; and as neither of these is employed at all in the subsequent test computations, the discrepancy has here no practical importance.

formula of Dubuat, with a modification of the constant, to represent very nearly the effect deduced from observation. This formula is (with the constant divisor reduced to English feet)

$$h_{\prime\prime} = \frac{v^2 \sin^2 \hat{a}}{266 \cdot 3},$$

in which $\sin^2 \hat{a}$ is the sum of the squares of the natural sines of the amount of bending, divided into angles not exceeding 36° or 40°.

Dubuat derived this formula from observation on the flow of water in pipes, in which the cross-section has no such variation as is always observed in the bends of rivers. The value of $h_{\prime\prime}$ is therefore too small, or the constant too large. Observations were made by the survey, to determine the amount of slope required to overcome a given bend. These observations were founded on a principle at once simple and ingenious. A level was run from one point of the river to another, several miles distant, embracing between them a bend and a long straight reach. Simultaneous readings of the stand of the river were made at both ends of the reach, and above the bend. If the bend had not existed, the slope in the reach multiplied into the distance by river between the extreme stations, should give the observed difference of level. The fall is always greater as observed than as computed, by the value of $h_{\prime\prime}$. From the data obtained by means of such observations, it was ascertained that Dubuat's formula, with the constant 134, would accord very closely with the observations. The authors therefore give, as the expression for the effect of bends,

$$h_{\prime\prime} = \frac{v^2 \sin^2 \hat{a}}{134}.$$

A table of the comparative values of $h_{\prime\prime}$ as computed by formula, and as obtained by actual measurement, over distances varying from five to nine miles, is given in the report, in which the differences are all very small, and are proportionally smaller as the distance is greater. In the application of the formula to cases in which an actual examination of every bend had not been made, resort was had to the best maps, and an estimate of the amount of bending made by measurement on the map. As a rough test of the correctness of these determinations, an independent formula was constructed, on the principle that the amount of bending between two points must be approximately proportional to the difference of distance between the points, as measured by an air line, and by the river. Denoting this difference in miles by M, it was found that $\sin^2 \hat{a}$ rarely differed essentially from 0·34 M. A series of comparisons somewhat extended, upon stretches of the river varying from three miles to more than eighty miles in length, gave, for the total of the observed values of $\sin_2 \hat{a}$, 140·81, and for that of the values computed by the last formula, 141·78. The total of the difference, taken, without regard to sign, was 22·83. This, observe the authors, "is given as an illustration that so far from being, as often declared in popular writings, a river without rule or beyond the restraint of law, the Mississippi is in reality controlled by laws which can be expressed in single algebraic formulæ."

We now come to that part of the report which has impressed us most forcibly with the value of the new formulæ, and the merit of the great labor by which they have been wrought out. This consists in an extended series of tests, in which the results of computation according to the formulæ are compared with actual measurements of the quantities computed. As no adequate idea of the severity and thoroughness of these tests can be formed without an inspection of all the data, along with the results deduced from them, we regard it as in justice due to the authors of the report to insert the following tables in full. It is to be observed that, in all cases, the slopes of rivers, as given in the first table, are the slopes as corrected from the measured slopes, by applying the formula for bends. The second table contains *differences* between the values of the computed mean velocity, and the mean velocity actually measured, in each of thirty cases. This table is rendered especially interesting by the comparison which it exhibits between the results given by the new formula for velocity, and those derived from the formulæ laid down by other writers. The following list embaces all these formulæ:

"*Chezy* . . .
- (Young's coefficient) . . . . $v = 84{\cdot}3(rs)^{\frac{1}{2}}$.
- (Eytelwein's coefficient) . . . . $v = 93{\cdot}4(rs)^{\frac{1}{2}}$.
- (Downing's and others' coefficient) $v = 100{\cdot}0(rs)^{\frac{1}{2}}$.

Dubuat . . . $v = \dfrac{88{\cdot}49(r^{\frac{1}{2}} - 0{\cdot}03)}{\left(\frac{1}{s}\right)^{\frac{1}{2}} - \mathrm{L}\left(\frac{1}{s} + 1{\cdot}6\right)^{\frac{1}{2}}} - 0{\cdot}086(r^{\frac{1}{2}} - 0{\cdot}03)$.

In which L = common logarithm multiplied by 2·302585.

Girard . . . $v = (2{\cdot}69 + 26384\, r\, s)^{\frac{1}{2}} - 1{\cdot}64$

De Prony
- (For canals) . . . . $v = (0{\cdot}0556 + 10593\, rs)^{\frac{1}{2}} - 0{\cdot}2357$.
- (For canals and pipes) $v = (0{\cdot}0237 + 9966\, rs)^{\frac{1}{2}} - 0{\cdot}1542$.
- (Eytelwein's coefficient) $v = (0{\cdot}0119 + 8963\, rs)^{\frac{1}{2}} - 0{\cdot}1089$.
- (Weisbach's coefficient) $v = (0{\cdot}00024 + 8675\, rs)^{\frac{1}{2}} - 0{\cdot}0154$.

Young . . . $v = \left(\dfrac{rs}{3\mathrm{A}} + \left(\dfrac{\mathrm{B}}{12\mathrm{A}}\right)^2\right)^{\frac{1}{2}} - \dfrac{\mathrm{B}}{12\mathrm{A}}$.

In which $\mathrm{A} = 0{\cdot}0000001\left(413 + \dfrac{1{\cdot}5625}{r} - \dfrac{90}{3r+8} - \dfrac{15}{4r + 0{\cdot}0296}\right)$,

$$B = 0{\cdot}0000001\left(\frac{900r^2}{r^2+0{\cdot}5}+\frac{1}{(3r)^{\frac{1}{2}}}\left(271{\cdot}25+\frac{6{\cdot}88}{r}+\frac{0{\cdot}0001146}{r^2}\right)\right).$$

Dupuit.... $v=\frac{s\,r\,a}{0{\cdot}08\,W}+(0{\cdot}0067+9114\,rs)^{\frac{1}{2}}-0{\cdot}082.$

St. Venant.. $v=106{\cdot}068\,(rs)^{\frac{11}{21}}.$

Ellet...... $v=0{\cdot}64\,(\Delta H)^{\frac{1}{2}}+0{\cdot}04\,\Delta H.$

In which Δ denotes the maximum depth of the stream, and H the fall in water surface in 1 English mile."

Measurements of cross-section, slope, and resulting mean velocity of rivers

No. of observation.	Stream.	Locality.	Date.	Dimensions of cross-section. Area.	Width	Perimeter.	Max. depth	Mean veloc'y	Slope.	Authority.
				Sq. ft.	Feet.	Feet.	Feet.	Feet.		
1	Mississippi river,	Carrolton.	High w. 1851	193,968	2653	2693	136	5.9288	0.00002051	DeltaSurv'y
2	" "	"	" "	195,349	2656	2696	136	5·8869	0.00001713	" "
3	" "	"	May 31, 1851	180,968	2421	2461	131	4·0338	0.00000342	" "
4	" "	"	June 3, 1851	183,663	2429	2469	132	3·9775	0.00000384	" "
5	" "	Columbus.	May 15, 1858	148,042	2214	2247	88	6·9575	0·00006800	" "
6	" "	Vicksburg.	June 7, 1858	178,137	2729	2779	100	6·9496	0·00006379	" "
7	" "	"	H. w. 1858	179,502	2732	2782	101	6·8245	0·00004365	" "
8	" "	"	Nov. 6, 1858	78,828	2507	2530	63	3·5234	0·00002227	" "
9	" "	"	Dec. 18, 1858	134,942	2556	2589	83	5·5580	0·00003029	" "
10	" "	"	Dec. 24, 1858	150,354	2580	2621	90	6·3186	0·00004811	" "
11	Bayou Plaquemine,	Near Upper mouth.	Mar. 12, 1851	5,560	292	303	28	5.1979	0·00020644	Mr. C. Ellet.
12	" "	" " "	Jan. 16, 1859	4,259	268	278	24	3·9589	0·00014372	DeltaSurv'y
13	" La Fourche,	" " "	H. w. 1851	3,738	223	238	27	3·0765	0·00004468	" "
14	" "	" " "	May 6, 1851	3,025	223	232	24	2·8430	0·00003731	" "
15	" "	" " "	May 7, 1851	2,957	223	231	24	2·8069	0·00003655	" "
16	" "	" " "	May 8, 1851	2,868	223	230	23	2·7894	0·00004384	" "
17	C. & O. canal feeder	Near Georget'n, D.C.	Nov. 26, 1859	121	23	32·7	7·6	3·0323	0·00069851	" "
18	" "	" " "	Nov. 28, 1859	119	23	32.5	7·5	2·7227	0·00069851	" "
19	Ohio River,	Point Pleasant.	Nov. 20, 1858	7,218	1073	1074	8	2·5152	0·00009334	Mr. C. Ellet.
20	River Haine,	France.	—— 1782	248·5	48	50·5	8 ?	2·4947	0·00016534	M. Dubuat.
21	" "	"	—— 1782	306·4	50·5	53·4	9 ?	2·5579	0·00015593	" "
22	Canal,	England.		50	18	20·6	4	1·1336	0·00006313	Mr. Watt.
23	River Rhine,	Byland.	June— 1812	19,135	1155	1163	20	3·5749	0·00009769	Krayenhoff.
24	" "	Pannerden.	" "	6,304	557	563	17 ?	3·2766	0·00009986	"
25	" Waal,	Upper mouth.	" "	14,782	1328	1334	17 ?	3·1648	0·00010438	"
26	" Rhine,	Below the Yssel.	" "	5,341	700	704	12 ?	2·9167	0·00011744	"
27	" Yssel,	Upper mouth.	" "	1,930	321	324	9 ?	2·7727	0·00011657	"
28	" Tiber,	Rome.	June— 1821	2,355	243	249	15	3·4132	0·00013061	M. Buffon.
29	" Neva,	Russia.	June— 18—	43,461	1218	1227	50	3·2296	0·00001389	M. Destrem.
30	" Great Nevka,	"	June— 18—	15,554	881	893	21	2·0486	0·00001487	" "

Tests of the several formulæ for mean velocity.

Number of observations.	Chezy's formula with coefficient of Young.	Chezy's formula with coefficient of Eytelwein	Chezy's formula with coefficient of Downing and others.	Dubuat's formula.	Girard's formula.	De Prony's formula with coefficients. For canals.	De Prony's formula with coefficients. For pipes and canals.	De Prony's formula with coefficients. By Eytelwein.	De Prony's formula with coefficients. By Weisbach.	Young's formula.	Dupuit's formula.	St. Venant's formula.	Ellet's formula.	New formula.
	Feet.	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*
1 - - -	+2·6888	+2·3390	+2·0854	+3·1820	+1·1140	+2·2017	+2·2430	+2·3974	+2·3644	+2·6547	+1·0536	+2·4381	+2 8837	+0·0385
2 - - -	+2·9167	+2·5961	+2·3636	+3·4374	+1·5736	+2·4887	+2·5204	+2·6584	+2·6206	+2·9000	+1·4629	+2·7002	+3·1500	+0·2425
3 - - -	+2·6973	+2·5530	+2·4484	+3·3542	+2·6207	+2·6208	+2·5978	+2·5378	+2·5725	+2·7822	+2·3651	+2·6534	+2·9552	+0·2593
4 - - -	+2·5522	+2·3984	+2·2868	+3·2073	+2·4188	+2·4572	+2·4369	+2·4820	+2·4182	+2·6320	+2·1732	+2·5009	+2·8230	+0·0658
5 - - -	+1·3152	+0·7061	+0·2643	+1·5090	−2·3975	+0·3004	+0·4281	+0·7288	+0·7388	+1·1238	−3·0950	+0·7159	+2·0962	−0·8093
6 - - -	+1·5591	+0·9772	+0·5552	+2·7722	−1·9246	+0·5998	+0·7184	+1·0038	+1·0091	+1·3844	−0·0904	+0·9996	+1·8883	−0·4602
7 - - -	+2·3506	+1·8677	+1·5174	+2·6733	−0·3106	+1·5929	+1·6784	+1·9078	+1·8968	+2·2359	−0·4739	+1·9298	+2·8054	+0·0713
8 - - -	+1·3028	+1·0631	+0·8893	+1·6224	+0·5812	+1·0377	+1·0434	+1·1361	+1·0853	+1·3343	+0·8166	+1·1737	+1·4852	−0·3978
9 - - -	+2·2085	+1·8469	+1·5847	+2·5718	+0·5389	+1·6975	+1·7427	+1·9037	+1·8726	+2·1690	+0·8041	+1·9437	+2·6953	+0·0430
10 - - -	+1·8902	+1·4121	+1·0654	+2·1721	−0·7304	+1·1425	+1·2263	+1·4530	+1·4411	+1·7785	−0·6254	+2·0143	+2·3442	−0·1982
11 - - -	+0·0094	−0·5507	−0·9569	−0·1965	−3·2932	−0·9055	−0·7942	−0·5211	−0·5194	−0·1556	−1·4981	−0·5187	+0·4408	−0·0448
12 - - -	+0·0033	−0·4238	−0·7334	−0·0583	−2·1974	−0·6407	−0·5738	−0·3760	−0·3963	−0·0801	−0·8769	−0·3435	+0·4993	−0·3871
13 - - -	+0·8434	+0·6023	+0·4275	+1·0077	+0·1116	+0·5756	+0·5817	+0·6751	+0·6245	+0·8751	+0·4812	+0·7129	+1·2065	−0·2076
14 - - -	+0·9837	+0·7829	+0·6374	+1·1533	+0·5428	+0·7964	+0·7900	+0·8069	+0·8040	+1·0392	+0·7353	+0·8921	+1·2623	+0·0213
15 - - -	+0·9835	+0·7866	+0·6439	+1·1535	+0·5695	+0·8039	+0·7963	+0 8651	+0·8076	+1·0613	+0·7447	+0·8954	+1·2442	+0·0304
16 - - -	+0·8184	+0·6056	+0·4513	+0·9686	+0·2926	+0·6072	+0·6044	+0·6821	+0·7270	+0·8657	+0·5499	+0·7156	+1·0997	−0 0768
17 - - -	−1·2535	−1·7162	−2·0517	−1·7040	−3·7470	−1·9699	−1·8912	−1·6733	−1·6876	−1·3746	−1·9100	−1·6470	−1·4773	−0·0709
18 - - -	−1·5406	−2·0008	−2·3346	−1·9857	−4·0141	−2·2520	−2·1741	−1·9576	−1·9723	−1·6603	−2·1895	−1·8953	−1·7497	−0·3594
19 - - -	+0·4038	+0·1759	+0·0106	+0·6998	−0·2313	+0·1623	+0·1643	+0·2504	+0·1977	+0·4406	+0·1520	+0·2864	+1·0867	+0·0298
20 - - -	+0·0901	−0·1694	−0·3577	+0·0453	−0·7802	−0·2148	−0·2028	−0·0990	−0·1467	+0·1054	−0·2003	−0·0593	+0·5240	+0·0257
21 - - -	+0·0364	−0·2358	−0·4333	−0·0038	−0·9301	−0·2940	−0·2779	−0·1671	−0·2128	+0·0447	−0·2847	−0·1265	+0·5194	−0·0886
22 - - -	+0·0901	−0 0226	−0·1043	+0·1478	+0·1788	+0·0737	+0·0425	+0·0655	−0·0040	+0·1871	+0·0257	+0·0684	+0·3413	−0·1965
23 - - -	+0·1952	−0·1696	−0·4342	+0·2315	−1·5005	−0·3224	−0·2761	−0·1133	−0·1439	+0·1506	−0·5043	−0 0736	+1·1067	−0·4809
24 - - -	+0·4577	+0·1534	−0·0673	+0·4877	−0·7571	+0·0627	+0·0891	+0·2179	+0·1774	+0·4492	+0·0070	+0·2597	+1·0020	−0·0544
25 - - -	+0·2978	−0·0117	−0·2361	−0·2478	−0·9577	−0·1078	−0·0797	−0·0520	+0·0125	+0·2862	−0·1620	+0·2895	+0·8310	−0·1808
26 - - -	+0·4004	+0·1288	−0 0681	+0·4037	−0·5617	+0·0712	+0·0871	+0·1976	+0·1518	+0·4107	+0·0629	+0·2382	+0·8733	+0·0973
27 - - -	+0·5513	+0·3115	+0·1376	+0·5584	−0·1711	+0·2860	+0·2918	+0·3845	+0·3337	+0·5803	+0·2855	+0·4221	+1·0048	+0·2993
28 - - -	+0·4503	+0·1305	−0 1015	+0·4294	−0·8867	+0·0238	+0·0553	+0·1928	+0·1587	+0·4315	−0·0107	+0·2347	+0·9410	+0·1378
29 - - -	+1·3598	+1·1579	+0·0115	+1·4974	+0·9109	+1·1702	+1·1641	+1·2357	+1·1790	+1·4133	+0·9731	+1·2671	+1·8573	−0 4976
30 - - -	+0·6919	+0·5455	+0·4393	+0·9601	+0·6026	+0·6112	+0·5888	+0·6300	+0·5649	+0·7771	+0·5348	+0·6463	+1·1609	−0·5191
Sum.	32·9420	28·4411	26·6988	40·4417	37·4472	28·0905	28·1506	29·5258	28·8412	33·3834	25·1488	33.6619	45·3547	6·3920

In order to understand the signs prefixed to the numbers in the foregoing table, it must be observed that the authors have tabulated the differences as *corrections*, not as *errors*. That is, each number must be applied to the result of the particular computation to which it relates, with the sign as written. Thus, the first number in the Chezy-Young formula, viz: +2·6888, indicates that this amount must be added to the value of v which the formula gives, in order to make it equal to the observed velocity, 5·9288. The formula gives 3·2400, and 3·2400+2·6888 =5·9288. This mode of exhibiting results, though it makes the comparative error striking, fails to convey an adequate impression of the *comparative approach to truth*, which is a different, and practically more important thing. Let us take, for illustration, the first four examples, with the results by several of the old formulæ and the new.

	1.	2.	3.	4.
Vel. observed,	5·9288	5·8869	4·0338	3·9775
Chezy-Young,	3·2400	2·9702	1·3365	1·4253
Dubuat,	2·7468	2·4495	0·6796	0·7702
Girard,	4·8148	4·3133	1·4131	1·5587
Prony-Eytelwein,	3·5314	3·2285	1·3960	1·4955
Prony-Weisbach,	3·5644	3·2663	1·4613	1·5593
Young,	3·2741	2·9869	1·2516	1·3455
St. Venant,	3·5907	3·1867	1·3804	1·4766
Ellet,	3·0451	2·7369	1·0786	1·1545
Humphreys and Abbot,	5·8903	5·6444	3·7745	3·9117

Thus, by comparing the actual velocities obtained by the different methods, it will be seen that most of the results are so far from the truth as to make them of little practical value; while the approach by the new formula is so near, that the difference is as likely to be due to errors of observation as to those of method.

There is another particular in which the table will not fail to attract notice. It is, that the old formulæ give better results upon rivers of moderate size, as upon the bayous, the Haine, the Rhine, the Tiber, &c., than upon the Mississippi; though upon the Mississippi itself, their results show great discrepancies. There is, however, one curious exception in the case of small streams. Numbers 17 and 18 are examples upon the feeder of the Chesapeake and Ohio Canal near Washington. The following are the results:

	1.	2.		1.	2.
Vel. observed,	3·0323	2·7227	Prony-Weisbach,	4·7199	4·7050
Chezy-Young,	4·2858	4·2633	Young,	4·4069	4·3830
Dubuat,	4·7363	4·7084	St. Venant,	4·6793	4·6180
Girard,	6·7793	6·7368	Ellet,	4·5096	4·4724
Prony-Eytelwein,	4·7056	4·6803	Humphreys and Abbot,	3·1032	3·0821

The old formulæ all give here a velocity largely in excess; whereas in large streams they are almost invariably in deficiency. The new formula represents these cases with as close an approach to observation as any others. The explanation of the anomaly is not obvious. The example of nearest general agreement of results, appears to be the small river Haine, No. 20, which gives the following:—

Vel. observed,	2·4947	Prony-Weisbach,	2·6414
Chezy-Young,	2·4046	Young,	2·3893
Dubuat,	2·4494	St. Venant,	2·5540
Girard,	3·2749	Ellet,	1·9707
Prony-Eytelwein,	2·5937	Humphreys and Abbot,	2·4690

If we examine the numerical ratio between the sums of the errors of the several formulæ in these thirty cases taken without regard to sign, as given in the table, to the sum of the observed velocities (115·4847), we shall find it to vary from twenty-two per cent for the formula of Dupuit, to thirty-nine per cent for that of Ellet. The formula of Humphreys and Abbot gives five and a half per cent. If we take the algebraic sum of these errors, this last ratio is reduced to three per cent; which is the tendency, as shown by this table toward excess. Examining the other formulæ in the same way, we shall see that they are all in deficiency, with the exception of Girard, who leans on the side of excess to the extent of eleven and a half per cent. The Chezy-Eytelwein formula gives a ratio of twenty-five per cent when the arithmetical sum of the errors is compared with the sum of the velocities; the Chezy-Downing formula gives twenty-three per cent on the same comparison. In these cases the algebraic sum of the errors shows a tendency to deficiency of fifteen and a half per cent for the first, and nine and a half for the second. Of all the old formulæ, the Dupuit appears to be the best; for the arithmetical sum of its errors bears the least ratio of all of them to the sum of the velocities; and the opposite errors, in these examples at least, almost exactly balance.

The second method employed by the authors of the report, to test the accuracy of their formulæ, consisted in computing the differences of level between points of the river distant from each other, in regard to which this difference had been ascertained by measurement. The same computation was made by Mr. Ellet's formula also, the results being introduced into the table along with those derived from the new formulæ, for the purpose of comparison. No computations were made in this case from the other formulæ, their large errors already showing their inapplicability to natural streams. An exception was made in favor of Mr. Ellet's, because it had been expressly designed for rivers. The present test applies equally to the bend formula, and to that for mean velocity. The following table embraces both data and

Tests of the formulæ for slope.

Stream.	Level-stations.	Date.	Area.	Width.	Perimeter.	Maximum depth.	Discharge.	Measured between level-stations			Ellet's formula.		New formula.			
								Sin 2d.	Distance.	Fall.	Computed fall.	Error.	$h_{,}$	$h_{,,}$	Computed fall, or $h_{,}+h_{,,}$	Error.
			Sq. ft.	Ft.	Ft.	Ft.	Cu. Ft.	Ft.	Miles.	Ft.	Ft.	Ft.	Ft.	Ft.	Ft.	Ft.
Mississippi R.	Ft. St. Philip and B. La Fourche.	H. w. 1851	199,000	2,470	2,510	129	1,150,000	21·60	156·00	20·7	50·9	-20·2	12·9	5·4	18·3	+ 2·4
" "	" " " " "	L. w. "	163,000	2,250	2,290	114	250,000	21·60	156·00	0·9	6·2	- 5·3	0·1	0·4	0·5	+ 0·4
" "	B. La Fourche and Red river.	H. w. "	200,000	3,000	3,035	113	1,200,000	15·39	122·60	23·7	47·7	-24·0	17·0	4·1	21·1	+ 2·6
" "	" " " " "	L. w. "	100,000	2,750	2,770	78	250,000	15·39	122·60	3·7	16·6	-12·9	1·8	0·7	5·5	+ 1·2
" "	Red river and Arkansas river.	H. w. 1858	199,000	4,080	4,115	96	1,200,000	56·50	373·00	112·0	172·1	-60·1	98·4	15·3	113·7	- 1·7
" "	" " " " "	L. w. "	54,000	3,060	3,070	56	200,000	56·50	373·00	114·0	92·4	+21·6	117·7	5·8	123·5	- 9·5
" "	Arkansas river and Ohio river.	H. w. "	191,000	4,470	4,510	87	1,175,000	47·33	408·00	160·0	214·2	-54·2	151·7	13·4	165·1	- 5·1
" "	" " " " "	L. w. "	45,000	3,400	3,410	49	150,000	47·33	408·00	159·0	121.2	+37·8	141·1	3·8	144·9	+14·1
B. Plaquemine.	Plaquemine and Indian Village.	H. w. 1850	5,500	300	318	31	33,500	8·63	8·33	20·0	12·1	+ 7·9	18·0	2·4	20·4	- ·04
" "	" " " "	March 12, '51	5,120	300	315	30	29,000	8·63	8·00	17·9	10·7	+ 7·2	14·9	2·1	17·0	+ 0·9
" "	" " " "	H. w. "	5,700	300	320	32	35,000	8·63	8·33	20·0	11·9	+ 8·1	17·4	2·4	19·8	+ 0·2
B. La Fourche.	Donaldsonville and Lockport.	" "	3,640	230	245	25	11,500	11·77	55·50	11·9	27·7	-15·8	12.6	0·9	13·5	- 1·6
" "	" " "	April 11, '58	3,567	221	237	26	9,700	11·77	55·50	8·0	25·7	-17·7	6·8	0·7	7·5	+ 0·5
Sum,										671·8	800·4	302·8			367·8	40·6

results. The data were not in all the cases known with equal degrees of exactness; but the small ratio of the errors to the distances on which the computations severally depend is not only satisfactory, but even surprising. In the last example but one, the error is regarded by the authors, as having been probably in great measure occasioned by the occurrence of crevasses between the points observed. The error which is largest in absolute amount is that between the Arkansas and Ohio rivers at low water, which is regarded as possibly due to sand bars.

The final test, and, as it seems to us, the most satisfactory of all, consists in the application of the new formula to the solution of the important question, how much will the level of a river be raised at a given locality, at which the cross-section and discharge are known, by any given definite increase of the discharge? In investigating this question, it is commonly assumed that the slope of the river is unaltered by the increased volume of discharge. But, as this assumption is not true, the results which are deduced from it are equally erroneous. In order to introduce the variation of slope, or the new slope produced by the addition to the volume, as an element in the computation, it was necessary to ascertain, if possible, by observation, the law which regulates the change.

The level of the water at the mouth of a river is not sensibly affected by a flood. For a certain distance up the course of the stream, the effect upon the slope produced by a rise in the river of a definite amount, will be equal to the total rise divided by the distance to the mouth. But, in general, an addition to the volume of waters produces a swell which passes down the stream like a great wave, so that the level may be actually falling near the head of the valley, when, at points lower down, it has not yet begun to rise. It is therefore evident that the same stand of the river is not always accompanied by the same slope, at any given point of observation, unless it be near the mouth.

From gauge observations, it appears that the form of the wave is tolerably regular, and that the daily *change of slope* is nearly the same for the same stand of the river in rising and falling. It is evident that observations on the passage of the great flood waves may be best conducted in the upper parts of the valley; inasmuch as the wave in its progress down the river tends, from the greater slope on the lower side, to spread itself over a wider and wider base, and loses therefore in the degree of its convexity. Columbus, Kentuky, was, on this account, first selected for study. The cross-section, perimeter, width, gauge-level and discharge of the river were determined for different dates during the progress of each of six marked rises of the river, and the corresponding slopes computed from the formula for those dates. The *differences of slope* measured, of course, the change produced by the increased volume of discharge.

In the endeavor to ascertain the law of change, the slopes were first plotted as abscissæ, to the gauge-readings as ordinates; and straight lines were drawn connecting the points representing the top and bottom of each rise. These lines were not parallel, showing that the rate of increase of slope varies for different rises. In the further study of their relations, it was discovered that the difference of slope divided by the rise is the abscissa of a curve sensibly parabolic, in which the gauge-reading at the top of the rise, measured from low water mark, is the corresponding ordinate. Or, if x denote the rise, e the primitive gauge-reading, and $e+x$ the gauge-reading at flood; also, if $s_{,}$ and $s_{,,}$ represent the primitive slope and the slope at flood, then the following equation will be true:—

$$\frac{s_{,,}-s_{,}}{x}=\frac{1}{2P}(e+x)^2, \quad \text{or} \quad s_{,,}-s_{,}=\frac{1}{2}P(e+x)^2x.$$

The value of $\frac{1}{2P}$ is to be determined by dividing $s_{,,}-s_{,}$ (of both which slopes the values are deduced, as just stated, by the formula, after the observations have determined the cross-section, discharge, perimeter, and rise of the river) by $(e+x)^2x$. For the same locality it is found to be constant; but it is different at different points in the length of the river.

If now we put $a_{,}$ $Q_{,}$ $p_{,}$ $W_{,}$ $v_{,}$ for the cross-section, discharge, perimeter, width, and mean velocity of the river in the primitive stage, and $a_{,,}$ $Q_{,,}$ $p_{,,}$ $W_{,,}$ and $v_{,,}$ for the same quantities after the rise; and if, in estimating the increased perimeter of the river occasioned by the rise, we neglect, as we may safely do for a large stream, the inclination of the banks, the new perimeter will be equal to the primitive perimeter increased by $2x$, and we shall have

$$\frac{a_{,,}}{p_{,,}+W_{,,}}=\frac{a_{,}+W_{,}x}{p_{,}+W_{,}+2x}$$

Also, as these denominators are equal and numerators also, we shall have

$$a_{,,}=a_{,}+W_{,}x; \quad \text{or} \quad a_{,,}v_{,,}=Q_{,,}=a_{,}v_{,,}+W_{,}v_{,,}x$$

and

$$x=\frac{Q_{,,}-a_{,}v_{,,}}{W_{,}v_{,,}}$$

Now, if the quantities $a_{,}$ $Q_{,}$ $p_{,}$ $W_{,}$ $v_{,}$ (with $z_{,}$ which is its function) be given, and it be required to know how much the river will rise if $Q_{,}$ be made $Q_{,,}$, the problem may be solved, and higher equations avoided, by an easy process of trial and error. Let $s_{,}$ be first computed from the formula

$$s_{,}=\left(\frac{(p_{,}+W_{,})z_{,}^2}{195a_{,}}\right)^2.$$

Then, assuming some definite value for x, obtain the numerical values of $\frac{a_{,,}}{p_{,,}+W_{,,}}$ and $s_{,,}$; the former from the equation given just above, and the latter from the equation,

$$s_{,,}=s_{,}+\frac{1}{2P}(e+x)^2x,$$

in which the reciprocal of the parameter has the value belonging to the locality. This being done, $v_{,,}$ may be obtained from the equation for mean velocity already given, viz:

$$v_{,,}=\left(0{\cdot}0388-\left(225s^{\frac{1}{2}}\frac{a_{,,}}{p_{,,}+W_{,,}}\right)^{\frac{1}{4}}\right)^2;$$

and with this value of $v_{,,}$ a value of x may be formed from the equation just found;

$$x=\frac{Q_{,,}-a_{,}v_{,,}}{W_{,}v_{,,}}.$$

If this last value agrees with the assumed value, the problem is solved. If not, a new supposition must be made. But, as the true value always lies between the two erroneous values—that is, between the assumed one and the computed one—the approximation will be rapid. This method has been applied by the authors of the report to the calculation of many rises in the river, of which the particulars are given in the following table. The results are compared with calculations for the same rises from the formula of Mr. Ellet. The symbols $\triangle_{,}$ and L in the table belong to Mr. Ellet's formula, the manner of employing which it is not necessary here to explain.

The only criterion by which it is possible to judge of the value of hydraulic formulæ, is the degree of their accordance with direct observation. We have no principles of positive science, to which, in forming such estimates, we can confidently or safely trust. Were it otherwise, we should long since have had formulæ, concerning the truth of which there would be no room for doubt. But science is not in possession of the material for the construction of such expressions. It can only indicate certain variables which must enter into them; as to the manner in which they shall enter, or whether they are all that affect the case, it is silent. We do not know the physical law of resistance opposed to the movements of a fluid by the surfaces which confine it, nor does it yet appear how we can know it. And so long as it is a fact that all the postulates of theory, and all the resources of analysis, are powerless to tell us what amount of force will be consumed in driving a liquid, with a given velocity, into the mouth of a tube, or through the simplest orifice that can be made in the side of the containing vessel, we may well regard a problem affected by all the complex conditions which

Tests for the formulæ for oscillation caused by variation in discharge.

Locality.	Date.	e	$\Delta_{,}$	L	$a_{,}$	$W_{,}$	$p_{,}$	$Q_{,}$	$Q_{,,}-Q_{,}$	True oscillation. Kind.	True oscillation. Am't.	Computed oscillation. Ellet's formula. x	Ellet's formula. Error.	New formula. x	New formula. Error
		Feet.	*Feet.*	*Miles.*	*Sq. feet.*	*Feet.*	*Feet.*	*Cubic feet.*	*Cubic feet.*		*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*
Columbus - - -	Dec. 3 to Dec. 21, 1857	12·7	62	1076	92,730	2102	2115	220,000	+940,000	Rise.	25·3	72·9	−47·6	27·0	−1·7
	Dec. 11 to Dec. 16, "	25·8	75	1076	120,660	2160	2204	691,630	+445,970	Rise.	12·2	20·8	− 8·6	13·8	−1·6
	Mar. 17 to Mar. 28, 1858	25·4	75	1076	119,780	2157	2200	590,000	+530,000	Rise.	15·0	27·6	−12·6	15·0	0·0
	April 11 to April 26, "	24·4	74	1076	117,630	2157	2200	570,000	+690,000	Rise.	18·7	35·1	−16·4	18·7	0·0
	May 7 to June 22, "	32·0	81	1076	134,180	2199	2242	776,550	+606,530	Rise.	14·6	26·6	−12·0	14·7	−0.1
	June 3 to June 22, "	42·3	92	1076	156,500	2218	2252	1,160,970	+222,110	Rise.	4·3	8·2	− 3·9	5·2	−0.9
	July 17 to July 27, "	25·3	75	1076	119,580	2157	2200	424,530	+240,900	Rise.	6·6	17·9	−11·3	6·6	0·0
	Oct. 30 to Nov. 8, "	9·8	59	1076	86,670	2073	2086	143,710	+298,280	Rise.	11·9	37.8	−25·9	11·6	+0·3
Vicksburg - - -	March 6 to March 30, "	29·1	82	487	127,630	2522	2545	670,550	+438,880	Rise.	13·8	18·8	− 5·0	13·3	+0·5
	Mar. 10 to Mar. 23, "	30·9	84	487	132,200	2535	2560	748,200	+199,260	Rise.	6·1	8·5	− 2·4	6·3	−0·2
	Mar. 20 to Mar. 30, "	34·5	87	487	141,350	2558	2585	841,570	+267,860	Rise.	8·4	10·5	− 2·1	7·4	+1·0
	April 19 to June 26, "	45·4	98	487	169,500	2660	2698	1,105,000	+125,900	Rise.	2·9	4·4	− 1·5	2·8	+0·1
	Aug. 6 to Aug. 26, "	45·0	98	487	168,600	2658	2698	1,086,400	−372,340	Fall.	11·6	15·0	− 3·4	11·2	+0·4
	Aug. 26 to Sept. 1, "	33·4	86	487	138,530	2550	2575	714,060	−173,140	Fall.	6·7	8·9	− 2·2	6·7	0·0
	Nov. 3 to Nov. 15, "	8·7	61	487	77,360	2420	2430	236,000	+359,000	Rise.	16·8	25·7	− 8·9	17·6	−0·8
	Dec. 10 to Dec. 13, "	17·0	70	487	97,580	2455	2475	399,920	+117,700	Rise.	5·6	7·3	− 1·7	5·9	−0·3
Carrollton - - -	Feb. 17 to Feb. 23, 1851	6·3	128	121	164,170	2324	2355	534,780	+335,220	Rise.	5·2	9·8	− 4·6	4·5	+0·7
	Feb. 19 to March 1, "	8·2	130	121	168,840	2338	2368	630,000	+382,570	Rise.	5.2	11·0	− 5·8	4·7	+0·5
	Feb. 20 to Feb. 25, "	9·0	131	121	170,800	2344	2374	670,770	+239,130	Rise.	3·0	7·0	− 4·0	3·1	−0·1
	Feb. 25 to March 21, "	12·0	134	121	177,900	2364	2398	909,900	+229,800	Rise.	3·1	6·2	− 3·1	3·0	+0·1
	April 17 to May 9, "	14·8	136	121	183,800	2378	2416	1,065,000	−181,000	Fall.	1·8	5·1	− 3·3	3·2	−1·4
	May 27 to July 25, "	9·9	132	121	173,000	2350	2380	652,330	+222,670	Rise.	3·0	6·5	− 3·5	2·5	+0·5
	July 31 to Aug. 15, "	12·4	134	121	178,810	2367	2401	845,000	−175,610	Fall.	2·6	5·3	− 2·7	2·9	−0·3
	Jan. 7 to Jan. 17, 1852	0·8	122	121	152,000	2287	2312	310,000	+220,000	Rise.	4·3	7·2	− 2·9	4·4	−0·1
										Sum.	208·7	404·1	195·4	212·1	11·6

modify the flow of water in a natural channel, as practically beyond their reach. Hydraulic formulæ must, accordingly, from the nature of the case, be to a great extent empirical; and the highest degree of theoretic plausibility which such a formula may bring to recommend it, can at best only serve as an encouragement to us to try it, in order that we may ascertain how far it may truly represent nature. The experience gathered in such past trials has not, however, been of a nature to render the encouragement a very solid ground of hope for a favorable result.

The test then of actual trial is that to which we must bring at last all theorems in hydraulics; and our judgments of their merits will be regulated by the manner in which they stand this test. This is a principle which the authors of the report before us seem to have fully recognized; and the thoroughness with which they have applied it to their own formulæ is without any past example in the history of such investigations. We think them, therefore, fully justified in the modest claim with which they conclude this part of their labor, viz., that these formulæ are "entitled to the confidence of practical men."

ART. XIX.—*On Inhalation of Nitroglycerine;* by JOHN M. MERRICK, Jr.

VARIOUS experiments have been made by different observers[1] upon the action of nitroglycerine or glonoine upon the animal economy—the nitroglycerine, or its solution in alcohol, being administered by dropping it upon the tongue—the effects which have been noticed being generally acceleration of the pulse, headache and prostration, and in peculiarly susceptible persons, these symptoms greatly aggravated.

These experiments, though somewhat contradictory, are very interesting, both from a chemical and a toxological point of view, but do not touch upon one matter, viz: the effects of the inhalation of the vapor of glonoine—a subject to which considerable interest must attach itself when we consider the rapidity with which the symptoms develop themselves when only a fraction of a drop is placed on the tip of the tongue.

In preparing a quantity of nitroglycerine in 1859, I met with an accident, the result of which exhibits in a very marked and satisfactory manner the toxical properties of this curious substance, and shows the necessity for extreme caution in handling it, especially when mixed with a volatile and inflammable solvent, as alcohol or ether.

[1] Vide Braithwaite's Retrospect of Practical Medicine, part xxxvii, p. 294.

The nitroglycerine was prepared by allowing pure glycerine to drop from a pipette with a glass stop-cock, so adjusted as to allow from fifteen to twenty drops to fall in a minute into a mixture of equal volumes of the strongest nitric and sulphuric acids cooled by very cold water.

In repeated experiments I have found that, in spite of the precautions taken to cool the acids, it is impossible to avoid an accident now and then, since, when the action reaches a certain intensity, just as in the oxydation of uric acid or cotton, the experiment ends in an explosion or a violent evolution of nitrous fumes. When such a result occurs in making glonoine, the bystander seldom escapes a severe headache, even though the experiment be conducted in the open air.

After glycerine equal to half the bulk of the mixed acids had dropped in, the whole was thrown into a large volume of cold water, thoroughly washed, drawn off with a pipette, dissolved in ether, and the ethereal solution evaporated on a water-bath. It was in this part of the preparation that the accident occurred which enables me to speak of the consequences which follow the inhalation of the vapor. The glass dish in which the evaporation was being conducted, by some mishap tipped over, spilling half its contents on the hot copper bath, and in a moment the room was full of the mixed vapor of nitroglycerine and ether. Although I stood directly over the water-bath to adjust it, and must have inhaled a large volume of the mixed vapor, no instant bad result followed, but in less than fifteen minutes a headache set in, slight at first, but increasing in intensity by degrees, until in an hour and a half it became almost intolerable. It was accompanied by a good deal of faintness and exhaustion, intolerance of light, and a feeling of great general distress and alarm, in addition to the racking pain. Relief was only obtained at length by the inhalation of a large quantity of ether, the insensibility produced by which was followed by broken and disturbed sleep lasting until the following day, which was marked by weakness, exhaustion, and slight headache. These unpleasant symptoms did not finally disappear for three or four days.

It may be remarked that, during all the time that the severe pain and distress lasted, consciousness was never lost for an instant. In Mr. Field's case,[2] *two drops* of a solution containing only one drop of glonoine to ninety-nine of rectified spirit produced loss of consciousness and other very alarming symptoms of narcotic poisoning.

The effects of glonoine upon different individuals are exceedingly different and contradictory. Two drops of a diluted solution containing only one drop of nitroglycerine in ninety-nine of

[2] Vide Braithwaite ut supra.

alcohol produces alarming symptoms of poisoning in one person, while another swallows two hundred drops of a similar solution with no other ill effects than a slightly "muddled" feeling in the head. I have experienced unpleasant feelings from tasting exceedingly minute quantities of *pure* nitroglycerine, such as headache, buzzing in the ears, with a feeling of nervousness and depression, although the action of the drug does not seem to be nearly so powerful or so rapid as when it is given in the form of alcoholic solution. Pure nitroglycerine is volatile at ordinary temperatures—a fact which was accidentally discovered in drawing off with a mouth pipette some nitroglycerine which had just been washed with water. Headache and the usual symptoms immediately set in, though not a particle of the liquid touched my mouth or tongue.

The following experiment, which shows that some constitutions are susceptible to the action of one-fortieth of a drop of glonoine, was made with a solution of nitroglycerine containing two and one-half drops of the pure substance to ninety-seven and one-half of alcohol. The solution was dropped upon sugar, and the sugar allowed to dissolve on the tongue.

My general health being good, and my pulse being seventy-nine, about two and one-half hours after a full meal, I took *one* drop of the solution. In two minutes my pulse was ninety-four, with dull, throbbing headache; in five minutes the pulse was one hundred, the headache changing from the back to the front of the head; in ten minutes the pulse was down to eighty-eight, and in fourteen minutes back to its normal rate, seventy-nine, although the headache did not wholly pass off for fifteen minutes more. It will be noticed that a quantity of the solution was taken, equal to only one-fortieth of a drop of pure nitroglycerine.

Walpole, Mass., July, 1863.

ART. XX.—*On the Chemical and Mineralogical Relations of Metamorphic Rocks;* by T. STERRY HUNT, F.R.S., of the Geological Survey of Canada.[1]

[Read before the Geological Society of Dublin, March 12, 1863.]

AT a time not very remote in the history of geology, when all crystalline stratified rocks were included under the common designation of primitive, and were supposed to belong to a period anterior to the fossiliferous formations, the lithologist confined his descriptions to the various species of rocks, without reference to their stratigraphical or geological distribution. But,

[1] **From the *Dublin Quarterly Journal of Science,* for 1863.**

with the progress of geological science, a new problem is presented to his investigation. While paleontology has shown that the fossils of each formation furnish a guide to its age and stratigraphical position, it has been found that sedimentary strata of all ages up to the Tertiary, inclusive, may undergo such changes as to obliterate the direct evidences of organic life; and to give to the sediments the mineralogical characters once assigned to primitive rocks. The question here arises, whether, in the absence of organic remains, or of stratigraphical evidence, there exists any means of determining, even approximately, the geological age of a given series of crystalline stratified rocks;—in other words, whether the chemical conditions, which have presided over the formation of sedimentary rocks, have so far varied, in the course of ages, as to impress upon these rocks marked chemical and mineralogical differences. In the case of unaltered sediments, it would be difficult to arrive at any solution of this question without greatly multiplied analyses; but in the same rocks, when altered, the crystalline minerals which are formed, being definite in their composition, and varying with the chemical constitution of the sediments, may, perhaps, to a certain extent become to the geologist what organic remains are in the unaltered rocks, a guide to the geological age and succession.

It was while engaged in the investigation of metamorphic rocks of various ages in North America, that this problem suggested itself, and I have endeavored from chemical considerations, conjoined with multiplied observations, to attempt its solution. In this *Journal* for 1858, and in the *Quarterly Journal of the Geological Society of London* for 1859, (p. 488), will be found the germ of the ideas on this subject which I shall endeavor to explain in the present paper.

It cannot be doubted that, in the earlier periods of the world's history, chemical forces of certain kinds were much more active than at the present day. Thus, the decomposition of earthy and alkaline silicates, under the combined influence of water and carbonic acid, would be greater when this acid gas was more abundant in the atmosphere, and the temperature probably higher. The larger amounts of alkaline and earthy carbonates then carried to the sea, from the decomposition of these silicates, would furnish a greater amount of calcareous matter to the sediments; and the chemical effects of vegetation, both on the soil and on the atmosphere, must have been greater during the Carboniferous period, for example, than at present. In the spontaneous decomposition of feldspars, which may be described as silicates of alumina, combined with silicates of potash, soda and lime, these latter bases are removed, together with a portion of silica; and there remains, as the final result of the process, a hydrous silicate of alumina, which constitutes kaolin, or clay. This change is favored by mechanical division; and Daubrée has shown that by prolonged attrition of fragments of granite under water, the softer and readily cleavable feldspar is in great part reduced to an impalpable powder, while the uncleavable grains of quartz are only rounded, and form a readily subsiding sand—the water at the same time dissolving from the feldspar a certain portion of silica, and of alkali. It has been repeatedly observed, where potash and soda feldspar are associated, that the latter is much the more readily decomposed, becoming friable, and finally being reduced to clay, while the orthoclase is unaltered. The result of combined chemical and mechanical agencies acting upon rocks which contain quartz, and orthoclase, and a soda feldspar, such as albite or oligoclase, would thus be a sand, made up chiefly of quartz and potash feldspar, and a finely divided and suspended clay, consisting for the most part of kaolin and of partially decomposed soda feldspar, mingled with some of the smaller particles of orthoclase and of quartz. With this sediment will also be included the oxyd of iron, and the earthy carbonates set free by the sub-aerial decomposition of silicates like pyroxene and the lime (anorthic) feldspars, or formed by the action of the carbonate of soda derived from the latter upon the lime and magnesia salts of sea water. The debris of hornblende and pyroxene will also be found in this finer sediment. This process is evidently the one which must go on in the wearing away of rocks by aqueous agency, and explains the fact that while quartz, or an excess of combined silica, is for the most part wanting in rocks which contain a large proportion of alumina, it is generally abundant in those in which potash feldspar predominates.

So long as this decomposition of alkaliferous silicates is subaerial, the silica and alkali are both removed in a soluble form. The process is often, however, submarine, or subterranean, taking place in buried sediments which are mingled with carbonates of lime and magnesia. In such cases the silicate of soda set free, reacts either with these earthy carbonates, or with the corresponding chlorids of the sea water, and forms in either event a soluble soda salt, and an insoluble silicate of lime and magnesia, which takes the place of the removed silicate of soda. The evidence of such a continued reaction between alkaliferous silicates and earthy carbonates is seen in the large amounts of carbonate of soda, with but little silica, which infiltrating waters constantly remove from argillaceous strata; thus giving rise to alkaline springs, and to natron lakes. In these waters it will be found that soda greatly predominates, sometimes almost to the exclusion of potash. This is due not only to the fact that soda feldspars are more readily decomposed than orthoclase, but to the well known power of argillaceous sediments to abstract from water the potash salts which it already holds in solution. Thus, when a solution of silicate, carbonate, sulphate, or chlorid of potassium is filtered through common earth, the potash is taken up, and replaced by lime, magnesia, or soda, by a double decomposition between the soluble potash salt and the insoluble silicates or carbonates of the latter bases. Soils in like manner remove, from infiltrating waters, ammonia, and phosphoric and silicic acids, the bases which were in combination with these being converted into carbonates. The drainage water of soils, like that of most mineral springs, contains only carbonates, chlorids, and sulphates of lime, magnesia and soda—the ammonia, potash, phosphoric and silicic acids being retained by the soil.

The elements which the earth retains or extracts from waters are precisely those which are removed from it by growing plants. These, by their decomposition under ordinary conditions, yield their mineral matters again to the soil; but, when decay takes place in water, these elements become dissolved, and hence the waters from peat bogs and marshes contain large amounts of potash and of silica in solution, which are carried to the sea, there to be separated—the silica by protophytes, and the potash by algæ, which latter, decaying on the shore, or in the ooze at the bottom, restore the alkali to the earth. The conditions under which the vegetation of the coal formation grew and was preserved, being similar to those of peat, the soils became exhausted of potash, and are seen in the fire-clays of that period.

Another effect of vegetation on sediments is due to the reducing or deoxydizing agency of the organic matters from its decay. These, as is well known, reduce the peroxyd of iron to a soluble protoxyd, and remove it from the soil, to be afterwards deposited in the forms of iron ochre and ores, which by subsequent alteration, become hard, crystalline, and insoluble. Thus, through the agency of vegetation, the iron-oxyd of the sediments is withdrawn from the terrestrial circulation; and it is evident that the proportion of this element diffused in the more recent sediments must be much less than in those of ancient times. The reducing power of organic matter is further shown in the formation of metallic sulphurets; the reduction of sulphates having precipitated in this insoluble form the heavy metals, copper, lead, and zinc, which, with iron, appear to have been in solution in the waters of early times, but are now by this means also abstracted from the circulation, and accumulated in beds and fahlbands, or by a subsequent process have been redissolved and deposited in veins. All analogies lead us to the conclusion that the primeval condition of the metals, and of sulphur, was, like that of carbon, one of oxydation, and that vegetable life has been the sole medium of their reduction.

The source of the carbonates of lime and magnesia in sedimentary strata is two-fold: first, the decomposition of silicates containing these bases, such as lime (anorthic) feldspar and pyroxene; and, secondly, the action of the alkaline carbonate, formed by the decomposition of feldspars, upon the chlorids of calcium and magnesium originally present in sea-water, which have thus, in the course of ages, been in great part replaced by chlorid of sodium. The clay or aluminous silicate which has been deprived of its alkali is thus a measure of the carbonic acid removed from the air of the carbonates of lime and magnesia precepitated, and of the amount of chlorid of sodium added to the waters of the primeval ocean.

The coarser sediments, in which quartz and orthoclase prevail, are readily permeable to infiltrating waters, which gradually remove from them the soda, lime and magnesia which they contain, and, if organic matters intervene, the oxyd of iron; leaving at last little more than silica, alumina and potash; the elements of granite, trachyte, gneiss, and mica schist. On the other hand, the finer marls and clays, resisting the penetration of water, will retain all their soda, lime, magnesia and oxyd of iron; and, containing an excess of alumina with a small amount of silica, will, by their metamorphism, give rise to basic lime and soda feldspars, and to pyroxene and hornblende—the elements of diorites and dolerites. In this way, the operation of the chemical and mechanical causes which we have traced naturally divides all the crystalline silico-aluminous rocks of the earth's crust into two types. These correspond to the two classes of igneous rocks, distinguished first by Professor Phillips, and subsequently by Durocher and by Bunsen, as derived from two distinct magmas; which these geologists imagine to exist beneath the solid crust, and which the latter denominates the trachytic and pyroxenic types. I have, however, elsewhere endeavored to show that all intrusive or exotic rocks are probably nothing more than altered and displaced sediments, and have thus their source within the lower portions of the stratified crust, not beneath it.

It may be well in this place to make a few observations on the chemical conditions of rock metamorphism. I accept in its widest sense the view of Hutton and of Boué, that all the crystalline stratified rocks have been produced by the alteration of mechanical and chemical sediments. The conversion of these into definite mineral species has been effected in two ways: first, by molecular changes, that is to say, by crystallization, and a rearrangement of the particles; and, secondly, by chemical reactions between the elements of the sediments. Pseudomorphism, which is the change of one mineral species into another by the introduction, or the elimination, of some element or elements, presupposes metamorphism; since only definite mineral species can be the subjects of this process. To confound metamorphism with pseudomorphism, as Bischoff, and others after him, have done, is therefore an error. It may be further remarked, that, although certain pseudomorphic changes may occur in some mineral species, in veins, and near to the surface, the alteration of great masses of silicated rocks by such a process is as yet an unproved hypothesis.

The cases of local metamorphism in proximity to intrusive rocks go far to show, in opposition to the views of certain geologists, that heat has been one of the necessary conditions of the change. The source of this has been generally supposed to be from below; but to the hypothesis of alteration by ascending heat Naumann has objected that the inferior strata in some cases escape change; and that, in descending, a certain plane limits the metamorphism, separating the altered strata above from the unaltered ones beneath: there being no apparent transition between the two. This, taken in connection with the well-known fact that in many cases the intrusion of igneous rocks causes no apparent change in the adjacent unaltered sediments, shows that heat and moisture are not the only conditions of metamorphism. In 1857, I showed by experiments, that, in addition to these conditions, certain chemical reagents might be necessary, and that water, impregnated with alkaline carbonates and silicates, would, at a temperature not above that of 212° F., produce chemical reactions among the elements of many sedimentary rocks, dissolving silica, and generating various silicates.[2] Some months subsequently, Daubrée found that, in the presence of alkaline solutions, at temperatures above 700° F., various silicious minerals, such as quartz, feldspar, and pyroxene, could be made to assume a crystalline form; and that alkaline silicates in solution at this temperature might combine with clay to form felspar and mica.[3] These observations were the complement of my own, and both together showed the agency of heated alkaline waters to be sufficient to effect the metamorphism of sediments by the two modes already mentioned—namely, by molecular changes, and by chemical reactions. Following upon this, Daubrée observed that the thermal alkaline spring of Plombières, with a temperature of 160° F., had, in the course of centuries, given rise to the formation of zeolites, and other crystalline silicated minerals, among the bricks and cement of the old Roman baths. From this he was led to suppose that the metamorphism of great regions might have been effected by hot springs, which, rising along certain lines of dislocation, and thence spreading laterally, might produce alteration in strata

[2] "Proc. Royal Soc. London," May 7, 1857, and "Phil. Mag." (4), xv, 68; also "Am. Jour. Science" [2], xxii, 437, and xxv, 435.

[3] "Comptes Rendus de l'Acad," Nov. 16, 1857; also "Bull. Soc. Geol. France," (2), xv, 103.

near the surface, while those beneath would in some cases escape change.[4] This ingenious hypothesis may serve in some cases to meet the difficulty pointed out by Naumann; but, while it is undoubtedly true in certain instances of local metamorphism, it seems to be utterly inadequate to explain the complete and universal alteration of areas of sedimentary rocks, embracing many hundred thousands of square miles. On the other hand, the study of the origin and distribution of mineral springs shows that alkaline waters (whose action in metamorphism I first pointed out, and whose efficient agency Daubrée has since so well shown) are confined to certain sedimentary deposits, and to definite stratigraphical horizons; above and below which, saline waters wholly different in character are found impregnating the strata. This fact seems to offer a simple solution of the difficulty advanced by Naumann, and a complete explanation of the theory of the metamorphism of deeply buried strata by the agency of ascending heat — which is operative in producing chemical changes only in those strata in which soluble alkaline salts are present.[5]

When the sedimentary strata have been rendered crystalline by metamorphism, their permeability to water, and their alterability, become greatly diminished; and it is only when again broken down by mechanical agencies, to the condition of soils and sediments, that they once more become subject to the chemical changes which have just been described. Hence, the mean composition of the argillaceous sediments of any geological epoch, or, in other words, the proportion between the alkalies and the alumina, will depend not only upon the age of the formation, but upon the number of times which its materials have been broken up, and the periods during which they have remained unmetamorphosed and exposed to the action of infiltra-

[4] It should be remembered that normal or regional metamorphism is in no way dependent upon the proximity of unstratified or igneous rocks, which are rarely present in metamorphic districts. The ophiolites, amphibolites, euphotides, diorites, and granites of such regions, which it has been customary to regard as exotic or intrusive rocks, are in most cases indigenous, and are altered sediments. I have elsewhere shown that the great outbursts of intrusive dolerites, diorites and trachytes, in southeastern Canada, are found, not among the metamorphic rocks, but among the unaltered strata along their margin, or at some distance removed; and I have endeavored to explain this by the consideration that the great volume of overlying sediments, which, by retaining the central heat, aided in the alteration of the strata now exposed by denudation, produced a depression of the earth's surface, and forced out the still lower and softened strata along the lines of fracture which took place in the regions beyond. See my paper "On some Points in American Geology," *American Jour. Science*, [2], xxxi, 414.

[5] See *Report of the Geological Survey of Canada*, 1853–6, pp 479, 480; also *Canadian Naturalist*, vii, 262. For a consideration of the relations of mineral waters to geological formations, see *General Report on the Geology of Canada* (now in press), p. 61; also chap. xix, on "Sedimentary and Metamorphic Rocks," where most of the points touched in the present paper are discussed at greater length.

ting waters. Thus, for example, that portion of the Lower Silurian rocks in Canada which became metamorphosed before the close of the Paleozoic period will have lost less of its soluble bases than the portion of the same age which still remains in the form of unaltered shales and sandstones. Of these, again, such portions as remain undisturbed by folds and dislocations will retain a larger portion of bases than those strata in which such disturbances have favored the formation of mineral springs; which, even now, are active in removing soluble matters from these rocks. The crystalline Lower Silurian rocks in Canada may be compared with those of the older Laurentian series on the one hand, and with the Upper Silurian or Devonian on the other; but, when these are to be compared with the crystalline strata of Secondary or Tertiary age in the Alps, it cannot be determined whether the sediments of which these were formed (and which may be supposed, for illustration, to have been directly derived from Paleozoic strata) existed up to the time of their translation, in a condition similar to that of the altered or of the unaltered Lower Silurian rocks of Canada. The proportion between the alkalies and the alumina in the argillaceous sediments of any given formation is not, therefore, in direct relation to its age, but indicates the extent to which these sediments have been subjected to the influences of water, carbonic acid, and vegetation. If, however, it may be assumed that this action, other things being equal, has, on the whole, been proportionate to the newness of the formation, it is evident that the chemical and mineralogical composition of different systems of rocks must vary with their antiquity, and it now remains to find in their comparative study a guide to their respective ages.

It will be evident that silicious deposits, and chemical precipitates, like the carbonates and silicates of lime and magnesia, may exist with similar characters in the geological formations of any age; not only forming beds apart, but mingled with the impermeable silico-aluminous sediments of mechanical origin. Inasmuch as the chemical agencies giving rise to these compounds were then most active, they may be expected in greatest abundance in the rocks of the earlier periods. In the case of the permeable and more highly silicious class of sediments already noticed, whose chief elements are silica, alumina, and alkalies, the deposits of different ages will be marked chiefly by a progressive diminution in the amount of potash, and the disappearance of the soda which these contain. In the oldest rocks, the proportion of alkali will be nearly or quite sufficient to form orthoclase and albite with the whole of the alumina present; but, as the alkali diminishes, a portion of the alumina will crystallize, on the metamorphism of the sediments, in the form of a potash-mica, muscovite, or margarodite. While the oxygen ratio between the alumina and the alkali in the feldspar just named is 3 : 1, it becomes 6 : 1 in margarodite, and 12 : 1 in muscovite. The appearance of these micas in a rock, then, denotes a diminution in the amount of alkali, until in some strata the feldspar almost entirely disappears, and the rock becomes a quartzose mica schist. In sediments still farther deprived of alkali, metamorphism gives rise to schists filled with crystals of kyanite, or andalusite, simple silicates of alumina, into whose composition alkalies do not enter; or, in case the sediment still retains oxyd of iron, staurotide and iron-alumina garnet take their places. The matrix of these minerals is generally a quartzose mica schist. The last term in this exhaustive process appears to be represented by the disthene and pyrophyllite rocks, which occur in some regions of crystalline schists.

In the second class of sediments, we have alumina in excess, with a small proportion of silica, and a deficiency of alkali, besides a variable proportion of silicates or carbonates of lime, magnesia, and oxyd of iron. The result of the processes already described will produce a gradual diminution in the amount of alkali, which is chiefly soda. So long as this predominates, the metamorphism of these sediments will give rise to feldspars like oligoclase, labradorite, or scapolite (a dimetric feldspar); but in sediments where lime replaces a great proportion of the soda, there appears a tendency to the production of denser silicates, like lime-alumina garnet and epidote, which replace the soda-lime feldspars. Minerals like the chlorites, and chloritoid are formed when magnesia and iron replace lime. In all these cases, the excess of the silicates of earthy protoxyds over the silicate of alumina is represented in the altered strata by hornblende, pyroxene, olivine, and similar species; which give rise by their admixture with the double aluminous silicates, to diorite, dolerite, diabase, euphotide, eclogite, and similar compound rocks.

In eastern North America, the crystalline strata, so far as yet studied, may be conveniently classed in five groups, corresponding to as many different geological series, four of which will be considered in the present paper.

1. The Laurentian system represents the oldest known rocks of the globe, and is supposed to be the equivalent of the Primitive Gneiss formation of Scandinavia, and that of the Western Islands of Scotland to which also the name of Laurentian is now applied. It has been investigated in Canada along a continuous outcrop from the coast of Labrador to Lake Superior, and also over a considerable area in Northern New York.

2. Associated with this system is a series of strata characterized by a great development of anorthosites, of which the hypersthenite, or opalescent feldspar-rock of Labrador, may be taken as a type. These strata overlie the Laurentian gneiss, and are

regarded as constituting a second and more recent group of crystalline rocks, to which the name of the Labrador series may be provisionally given. From evidence recently obtained, Sir William Logan conceives it probable that this series is unconformable with the older Laurentian system, and is separated from it by a long interval of time.

3. In the third place, there is a great series of crystalline schists, which are in Canada referred to the Quebec group, an inferior part of the Lower Silurian system. They appear to correspond, both lithologically and stratigraphically, with the Schistose group of the Primitive slate formation of Norway, as recognized by Naumann and Keilhau, and to be there represented by the strata in the vicinity of Drontheim, and those of the Dofrefeld. The Huronian series of Canada in like manner appears to correspond to the Quartzose group of the same Primitive Slate formation. It consists of sandstones, imperfect varieties of gneiss, diorites, silicious and feldspathic schists, passing into argillites, with limestones, and great beds of hematite. Though more recent than the Laurentian and Labrador series, these strata are older than the Quebec group; yet, from their position to the westward of the greatest accumulation of sediments, they have been subjected to a less complete metamorphism than the Paleozoic strata of the East. The Huronian series is as yet but imperfectly studied, and for the present will not be further considered.

4. In the fourth place are to be noticed the metamorphosed strata of Upper Silurian and Devonian age, with which may also be included those of the Carboniferous system in eastern New England. This group has as yet been imperfectly studied, but presents interesting peculiarities.

In the oldest of these, the Laurentian system, the first class of aluminous rocks takes the form of granitoid gneiss, which is often coarse grained and porphyritic. Its feldspar is frequently a nearly pure potash-orthoclase, but sometimes contains a considerable proportion of soda. Mica is often almost entirely wanting, and is never abundant in any large mass of this gneiss, although small bands of mica schist are occasionally met with. Argillites, which, from their general predominance of potash and of silica, are related to the first class of sediments, are, so far as known, wanting throughout the Laurentian series; nor is any rock here met with, which can be regarded as derived from the metamorphism of sediments like the argillites of more modern series. Chloritic and chiastolite schists and kyanite are, if not altogether wanting, extremely rare in the Laurentian system. The aluminous sediments of the second class are, however, represented in this system by a diabase made up of dark green pyroxene and bluish labradorite, often associated with a red alumino-ferrous garnet. This latter mineral also sometimes constitutes small beds, often with quartz, and occasionally with a little pyroxene. These basic aluminous minerals form, however, but an insignificant part of the mass of strata. This system is further remarkable for the small amount of ferruginous matter diffused through the strata, from which the greater part of the iron seems to have been removed, and accumulated in the form of immense beds of hematite and magnetic iron. Beds of pure crystalline plumbago also characterize this series, and are generally found with the limestones. These are here developed to an extent unknown in more recent formations; and are associated with beds of crystalline apatite, which sometimes attain a thickness of several feet. The serpentines of this series, so far as yet studied in Canada, are generally pale colored, and contain an unusual amount of water, a small proportion of oxyd of iron, and neither chrome nor nickel, both of the latter being almost always present in the serpentines of the third series.

The second, or Labrador series is characterized, as already remarked, by the predominance of great beds of anorthosite, composed chiefly of triclinic feldspars, which vary in composition from anorthite to andesine. These feldspars sometimes form mountain masses, almost without any admixture, but at other times include portions of pyroxene, the latter passing into hypersthene. Beds of nearly pure pyroxenite are met with in this series, and others which would be called hyperite and diabase. These anorthosite rocks are often compact, but more frequently granitoid in structure. They are generally grayish, greenish, or bluish in color, and become white on the weathered surfaces. The opalescent labradorite-rock of Labrador is a characteristic variety of these anorthosites, which often contain small portions of red garnet and brown mica, and, more rarely, epidote and a little quartz. They are sometimes slightly calcareous. Magnetic iron and ilmenite are often disseminated in these rocks, and occasionally form masses or beds of considerable size. These anorthosites constitute the predominant part of the Labrador series, so far as yet examined. They are, however, associated with beds of quartzose orthoclase gneiss, which represent the first class of aluminous sediments, and with crystalline limestones; and they will probably be found, when further studied, to offer a complete lithological series. These rocks have been observed in several areas among the Laurentide Mountains, from the coast of Labrador to Lake Huron, and are also met with among the Laurentian rocks of the Adirondack Mountains; of which, according to Emmons, they form the highest summits.

In the third series, which we have referred to the Lower Silurian age, the gneiss is sometimes granitoid, but less markedly so than in the first; and it is much more frequently micaceous, often passing into micaceous schist, a common variety of which contains disseminated a large quantity of chloritoid. Argillites abound, and, under the influence of metamorphism, sometimes develop crystalline orthoclase. At other times, they are converted into a soft micaceous mineral and form a kind of mica-schist. Chiastolite and staurotide are never met with in the schists of this series, at least in its northern portions, throughout Canada and New England. The anorthosites of the Labrador series are represented by fine grained diorites, in which the feldspar varies from albite to very basic varieties, which are sometimes associated with an aluminous mineral allied to chlorite in composition. Chloritic schists, frequently accompanied by epidote, abound in this series. The great predominance of magnesia in the forms of dolomite, magnesite, steatite and serpentine, is also characteristic of portions of this series. The latter, which forms great beds (ophiolites), is marked by the almost constant presence of small portions of the oxyds of chrome and nickel. These metals are also common in the other magnesian rocks of the series; green chrome-garnets, and chrome-mica occur; and beds of chrome-iron ore are found in the ophiolites of the series. It is also the gold-bearing formation of eastern North America, and contains large quantities of copper ores in interstratified beds resembling those of the Permian schists of Mansfeld and Hesse. In some parts of this series, pure limestones occur, which contain various crystalline minerals common also to the Laurentian limestones, and to those of the fourth series. The only graphite which has been found in the third series is in the form of impure plumbaginous shales.

The metamorphic rocks of the fourth series, as seen in southeastern Canada, are, for the greater part, quartzose and micaceous schists, more or less feldspathic; which, in the neighboring States, become remarkable for a great development of crystals of staurotide and of red garnet. A large amount of argillite occurs in this series; and, when altered, whether locally by the proximity of intrusive rock, or by normal metamorphism, exhibits a micaceous mineral and crystals of andalusite: so that it becomes known as chiastolite slate in its southern extension. Granitoid gneiss is still associated with these crystalline schists. Gold is not confined to the third series, but is also met with in veins cutting the argillites of Upper Silurian age. The crystalline limestones and ophiolites of eastern Massachusetts, which are probably of this series, resemble those of the Laurentian system; and the coal beds in that region are, in some parts, changed into graphite. It is to be remarked that the metamorphic strata of the third and fourth series are contiguous throughout their extent, so far as examined, but are everywhere separated from the Laurentian and Labrador series by a zone of unaltered Paleozoic rocks.

Large masses of intrusive granite occur among the crystalline strata of the fourth series, but are rare or unknown among the older metamorphic rocks in Canada. The so-called granites of the Laurentian and Lower Silurian appear to be in every case indigenous rocks; that is to say, strata altered *in situ*, and still retaining evidences of stratification. The same thing is true with regard to the ophiolites and the anorthosites of both series; in all of which the general absence of great masses of unstratified rock is especially noticeable. No evidences of the hypothetical granitic substratum are met with in the Laurentian system, although this is, in one district, penetrated by great masses of syenite, orthophyre, and dolerite. Granitic veins, with minerals containing the rarer elements, such as boron, fluorine, lithium, zirconium, and glucinum, are met with alike in the oldest and newest gneiss in North America. These, however, I regard as having formed, like metalliferous veins, by aqueous deposition in fissures in the strata.

The above observations upon the metamorphic strata of a wide region seem to be in conformity with the chemical principles already laid down in this paper; which it remains for geologists to apply to the rocks of other regions, and thus determine whether they are susceptible of a general application. I have found that the blue crystalline labradorite of the Labrador series of Canada is exactly represented by specimens from Scarvig, in Skye; and the ophiolites of Iona resemble those of the Laurentian series in Canada. Many of the rocks of Donegal appear to me lithologically identical with those of the Laurentian period; while the serpentines of Aghadoey, containing chrome and nickel, and the andalusite and kyanite schists of other parts of Donegal cannot be distinguished from those which characterize the altered Paleozoic strata of Canada. It is to be remarked that chrome- and nickel-bearing serpentines are met with in the same geological horizon in Canada and Norway; and that those of the Scottish Highlands, which contain the same elements, belong to the newer gneiss formation; which, according to Sir Roderick Murchison, would be of similar age. The serpentines of Cornwall, the Vosges, Mount Rosa, and many other regions, agree in containing chrome and nickel; which, on the other hand, seem to be absent from the serpentines of the primitive gneiss-formation of Scandinavia. It remains to be determined how far chemical and mineralogical differences, such as those which have been here indicated, are geological constants. Meanwhile, it is greatly to be desired that future chemical and mineralogical investigations of crystalline rocks should be made with this question in view; and that the metamorphic strata of the British Isles, and the more modern ones of southern and central Europe, be studied with reference to the important problem which it has been my endeavor, in the present paper, to lay before the society.

Montreal, Canada, Jan. 25, 1863.

ART. XXI.—*On the Appalachians and Rocky Mountains as Time-boundaries in Geological History;* by JAMES D. DANA.

THE Appalachian mountains, extending from Labrador to Alabama, and the Rocky chain, facing the Pacific from the Arctic to the Isthmus of Darien, are the two great mountain chains of the North American continent. They are the heights which determine its features—one constituting the Atlantic border-chain, the other, the Pacific, and the two forming the limits of the vast interior continental basin. All other lines of heights are small in comparison.

If the elevation of mountains has ever made epochs in geological history, or time-boundaries between its ages, we should look to the elevation of these chains for the profoundest of all divisions in the chronology of the North American continent. And, corresponding with this expectation, these two cases of mountain-raising stand out as boldly between the grander eras of time, as the mountains themselves do geographically between the oceans and the continental interior. The three eras, after the Azoic, recognized by geologists, are the *Paleozoic*, or ancient time, the *Mesozoic*, or medieval time, and the *Cenozoic*, or recent time; the first and second having their intervening limit between the Carboniferous and Reptilian ages, and the second and third, between the Cretaceous period closing the Reptilian age and the Tertiary commencing the age of Mammals.[1] Now, the elevations of the two mountain chains, referred to, date from the limits of these eras. At the first of these limits, or as the closing act in Paleozoic history, the rocks of the Appalachian region were flexed into numerous folds, in part crystallized, and, over a country more than a thousand miles in length, lifted into mountain ranges. And at the *second*, or as the introduction of Cenozoic time, the mass of the Rocky Mountains began to rise above the ocean.

The fact that the formation of the main portion of the Appalachians took place *after* the close of the Carboniferous age is fixed, beyond all question, as the Professors Rogers and others have shown, by the occurrence of the coal-beds of Pennsylvania, Rhode Island and Nova Scotia among the uplifted folded rocks. The coal-beds are part of the material bent and lifted in the grand process of mountain-making, and, of course, must have been laid down before the disturbance began. The evidence has been abundantly presented elsewhere and need not be here repeated.

[1] Prof. Agassiz, in a recent paper in the Atlantic Monthly, places the close of the Paleozoic after the Devonian. In the writer's view, the whole bearing of the science is against any such new arrangement of the Geological ages.

As the uppermost strata of the coal formation, together with the Permian beds, are wanting in Pennsylvania, although occurring in the Mississippi basin, it is probable, as suggested elsewhere by the writer, that the epoch of uplift and disturbance had its commencement even before the Permian period began; and that from this time it continued its progress, reaching its climax when the Carboniferous age had closed.

Again, it is proved, decisively, that the origin of these mountains preceded the Triassic or earliest period of the Reptilian age (or, at least, the closing part of that period) by the position and nature of the Triassic or Triassico-Jurassic beds. For they lie in valleys or depressions that were made in the formation of the Appalachians; they rest unconformably on rocks that were crystallized or consolidated in the course of the Appalachian revolution; and they are largely made of debris from these crystalline rocks. In addition, the species of fossil plants and of Thecodont and Labyrinthodont Reptiles, whose remains or traces occur in the beds, indicate that at least the older part of the formation is Triassic.

With regard to the Rocky Mountains, it is so well known that the mass of the chain was to a large extent under the sea in the Cretaceous period, and has since been raised 5000 to 6000 feet, and that this elevation commenced before the Tertiary period, or Cenozoic time, opened, that a recital in this place of facts bearing on the point is unnecessary.

The importance of the Appalachian revolution as a time-boundary is greatly enhanced by the history of the Paleozoic era preceding it. No raising of mountains is known to have occurred in North America between the Devonian and Silurian ages; and only some limited uplifts and disturbances between the Devonian and Carboniferous. The only elevations of prominent importance during these ages, of which we have evidence, occurred either at the close of the Lower Silurian or earlier. The Green Mountains, one portion of the Appalachians, date their first emergence, probably, from the close of the Lower Silurian. With a few small exceptions, therefore, the long era from the Azoic to the termination of the Carboniferous age was, comparatively, one of prolonged quiet, in which oscillations of level were in progress over continental areas, but no profound and extensive disturbances. These oscillations throughout the Paleozoic, had been, moreover, most profound along the Appalachian region, and the formations in progress had increased there to ten times the thickness acquired in the interior region—the whole directly preparatory for that making of the mountains which was to commence when Paleozoic time should draw to a close.[1]

[1] See the writer's article on American Geological History, this Journal, xxii, 305, 1856.

With no great epochs of revolution to fix limits to the Silurian, and none to give bounds to the Devonian, the heights of the Appalachians loom up majestically as a time-boundary to the Paleozoic.

It is fitting that the raising of one of the two *border-chains* of the continent—the eastern—should thus mark one of the grandest of the transitions in geological history. The transition was as abrupt in the life of the continent and globe as in its formations; for it was the time when its *ancient* features were to a great extent lost:—when *Trilobites*, *Cyathophylloid* and other old styles of *corals*, and the *Sigillariæ* and *Lepidodendra* of the old forests came to an end; when *Brachiopods* lost their preëminence among Mollusks, and *Crinoids* among Radiates, and *heterocercal Ganoids* among Fishes, and the *Lycopodium* tribe and *Calamites* among Acrogens. The transition from the Devonian to the Carboniferous presents no such abrupt change in living tribes. More than 70 species of *coal-plants*, according to Dawson, have already been identified from the Devonian rocks of North America alone —including species of *Ferns*, *Calamites* and *Lepidodendra* among Acrogens, and of *Sigillariæ* and *Conifers* among Gymnosperms; and some of these Devonian species, both of Acrogens and Gymnosperms, occur also, as this author has observed, among the fossil plants of the Carboniferous age.

The Reptiles of the Carboniferous age are the prominent Medieval type begun in Paleozoic time; and these were precursors of the age of Reptiles which was to follow, just as the Jurassic Mammals were precursors of the succeeding age of Mammals. It would be as right to throw the Jurassic and Cretaceous periods (or half of the Reptilian age) into the Age of Mammals, on account of these precursor Mammals, as the Carboniferous age into the Reptilian, because of its Reptiles. In all history, human as well as geological, each age has its beginning, or the initiation of its great characteristic, in the age preceding.

The second of the two grandest transitions in geological history has its appropriate monument in the Rocky Mountains, the *western* border-chain of the continent. The Rocky-Mountain region had been undergoing changes through all previous time, like the Appalachian anterior to its elevation; for ridges of Azoic origin stand on its slopes or upper plateaus—as the Black Hills of Dacotah, and the Laramie range; and others date their origin probably from epochs in the course of the Paleozoic, and from that of the Appalachian revolution at its close:—we say *probably*, because the precise ages of the ranges along the chain have not yet been determined. But there is no doubt that the *mass* of the chain, through a large part of its area, commenced its rise, as has been stated, just before Cenozoic time began. The elevation was not completed at once, but continued in progress, as the investigations of Hayden have shown, through much of the Tertiary period.

On the eastern border of the continent, only one epoch of profound disturbance *during the progress* of Mesozoic time (or the Reptilian age)—has been distinguished: namely, that when the Triassico-Jurassic formation underwent displacement, and the trap ridges and dykes that are associated with it were formed in Nova Scotia, the Connecticut valley, the Palisade region of New York and New Jersey, Pennsylvania, Virginia and North Carolina. This subordinate epoch of disturbance divides off the period of these Triassico-Jurassic beds from that of the Cretaceous formation.

At the close of the Mesozoic, there was some elevation of the continent on this same *eastern* border, but it was small in amount, compared with that on the *western*.[3]

The destruction of life closing Mesozoic time was as comprehensive and complete in North America, according to present knowledge, as that closing the Paleozoic. Investigation with reference to this point has already extended over so wide a region, that the fact may be regarded as quite well established. The exceptions that we have most reason to look for are those of oceanic fishes; for these species might have escaped the destroying agency (whether of climate and change of level, or the latter alone) which was in action over the continents and along the ocean's shallow borders.[4]

It is, then, evident that in North America the *two* boldest transitions in the course of the Zoic ages correspond with the raising of the mountain chains of the *two* oceanic borders. Thus time and geography are brought into direct parallelism.

Looking now abroad, we find evidence that the fact, here established as regards North America, has the universality of a fundamental truth or principle.

The epoch of the Appalachian revolution was not only a grand epoch in American history, but also in European. For the greatest disturbances over the continent, and the most extensive metamorphic changes, after those which preceded the Upper Silurian, appear to date from the time between the Carboniferous and Triassic periods, either at the beginning or close

[3] The essential conformability of the Cretaceous and Tertiary beds along part of the Atlantic and Gulf borders shows that even the mos tabrupt epochal transitions in geological history are not accompanied everywhere by disturbances of stratification and cases of unconformability. It is hence no objection to closing the Carboniferous age with the Permian, that the Permian beds and the Triassic in some parts of the world are conformable.

[4] Dr. J. Leidy has questioned, in conversation with the writer, whether the teeth of sharks from the American Cretaceous, that are undistinguishable from some of the Tertiary teeth, belong to distinct species or not. The point is not easily settled, since the teeth in these species often afford unsatisfactory specific characters.

of the Permian period. Murchison remarks, concerning the epoch following the Carboniferous, that it was then "that the coal-strata and their antecedent formations were very generally broken up, and thrown by grand upheavals into separate basins, which were fractured by numberless powerful dislocations." The formation of the main part of the Ural chain—the mountains on the *eastern* border of Europe (dividing the Orient into its eastern and western portion)—has been referred to this time.

Again, the epoch of the elevation of the Rocky Mountains was similarly eminent in European history. From the Triassic onward to the middle or later Cretaceous, there had been in Europe only oscillations of level, and relatively small uplifts or disturbances. The elevation of the range of the Côte d'Or and Cévennes in France, and of the Erzgebirge in Saxony, all north-easterly in course, has been referred to the interval between the Jurassic and Cretaceous periods. But when Mesozoic time was drawing to its close, then commenced the elevation of the Alps, Appenines and other heights of this western border of the Orient (for these mountains belong to the *border-region* of the Orient just as the Rocky Mountains do to that of the Occident, and are not as far distant as the latter from the adjoining ocean). The raising of these mountains, like that of the mountains of western America, was completed in the course of the Tertiary period.

Some of the loftiest ranges of Europe, and also of Asia, were lifted to their places after the Eocene had begun—as if the close of the Cretaceous period were less important as a mountain-making epoch than a later era, and as if Mesozoic time, in order to terminate against the grandest mountain elevations, should be continued to the middle or later Eocene. But the transition in kinds of life which accompanied the transition in time from the Cretaceous to the Tertiary shows that the close of the former was, in fact, the prominent epoch of physical change over the globe, notwithstanding the changes of level which subsequently took place. An early step in those changes that were introductory to Cenozoic time appears to have been that which, on both continents, was attended with the most universal effects. Mountains, as is now well known, have not been made by single heavings of the earth's crust, as waves may be thrown up on the ocean, but are results of a slow, long-continued, and often intermitted, action. And, as the Appalachians were in preparation during the Carboniferous age, and probably occupied in their formation the Permian period, so the Rocky Mountains, Alps, and other heights, while initiated long before, finally commenced their grand movements upward as the Reptilian age was terminating, to end them only with the lapse of the Tertiary.

There are thus two specially prominent periods of mountain-making in Europe, as in America, and they are directly connected with the two grand transitions in the life of the world, that of the Paleozoic to the Mesozoic, and that of the Mesozoic to the Cenozoic.[5]

Asia probably affords similar facts. The two opposing mountain chains of most prominence are the Altai on the north, and the Himalayas on the south. Jurassic rocks occur in the Himalayas, on the northern or Thibet side, to a height of from 14,000 to 18,000 feet, according to Strachey, and extend probably through a length of 400 miles. The elevation of the mountains, according to this author, must have taken place in mass, and subsequently to the Jurassic period. The absence of Cretaceous rocks appears to indicate that some slight emergence, at least, existed during the Cretaceous period. With regard to the exact time of the main part of the elevation, the evidence is not yet satisfactory. It is, however, certain that the western portion, in which Cashmeer lies, was still 15,000 feet below its present level in the early Eocene; and the elevation, whenever commenced, was completed throughout the chain, like that of the Alps and Appenines, only after the Tertiary period had begun. Thus the progress was gradual; and it covered the same part of geological time as that of the loftier mountains of America and Europe. As above remarked, the great transition in the life of the globe which took place at the close of the Cretaceous, shows that, notwithstanding this prolonging of the era of elevation, there was a *crisis* in the movement, climate and otherwise, at the close of Mesozoic time. The great physical changes in progress *then* made their profoundest mark on the world's history.

In South America, there is proof, as Darwin has shown, that the Andes were, to a large extent, raised from the ocean after the close of the Mesozoic. The elevation was not completed at once, any more than that of the Rocky Mountains or Alps, but continued afterwards to increase at intervals, while undergoing oscillations, during the subsequent Tertiary period.[6] The Rocky Mountains and Andes were one, apparently, in time of origin, as they are one in position along the American continent.

Is it not then probable, that over *all* the continents the making of the *border-mountains*—the chains which give the land its dominant features, or rather which are its features—corresponds as in America, with the two grandest epochs in the geological past, or, in other words, gives bounds to Paleozoic and Mesozoic time?

[5] The only other epoch or epochs of like eminence indicated in the North American rocks pertain to Azoic time: at its close, and perhaps at distant epochs preceding, there were mountains made and sedimentary strata thousands of feet in thickness folded and crystallized, the latter on a scale not afterwards equalled unless at the close of the Paleozoic or Mesozoic eras.

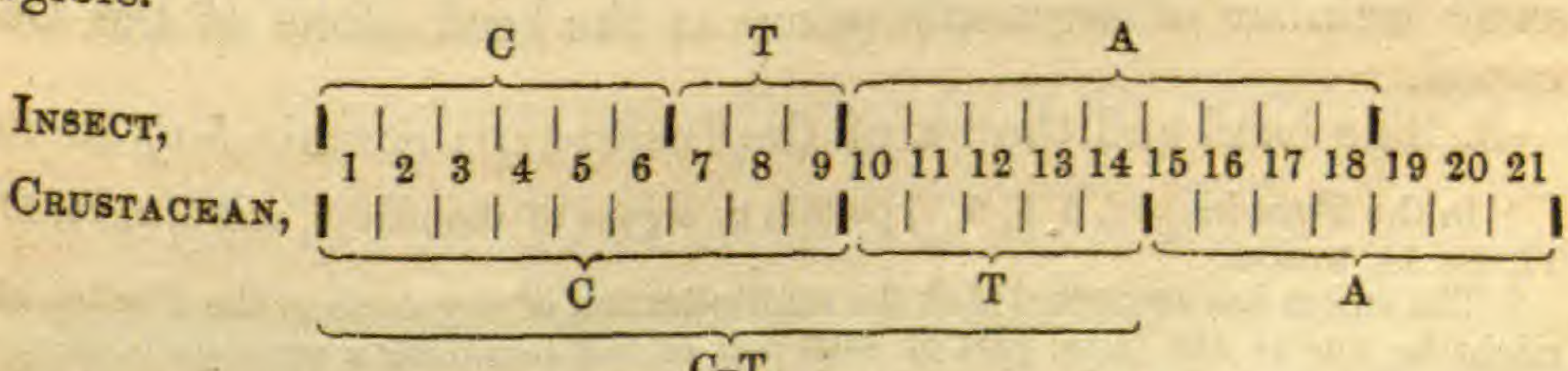
[6] See, on the extensive distribution of Cretaceous or Cretaceo-oolitic beds in the Andes, and on the elevation of these Mountains, Darwin "on South America," pp. 238, 239, and elsewhere; also D. Forbes, Q. J. Geol. Soc. 1861, p. 7.

The uplifting of these mountain regions was produced, as the writer has illustrated elsewhere, by lateral movements in the earth's crust, and mainly in those parts of it that make the bed of the ocean. And as the Atlantic bed stretches from America to Europe, and the Pacific, from America to Asia, there is no violence to reason in supposing that the profound movements which originated the lofty border-chains of one continent should have acted simultaneously (although it may have been very unequally) at the two sides of the oceanic basins, and thus have produced *world-wide* results. If so, we have a universal cause for simultaneous universal effects. There is evidence that in the case of some of the *minor* oscillations there were synchronous parallel movements in the North American and European continents;—as in the formation of marine limestones alike on the two continents in the Subcarboniferous period; in the accumulation of the strata of Millstone grit or coarse sandstone over these limestones; in the slight emergence of the continents and their oscillations below and above the sea-level, during the Carboniferous period, resulting in successive great marshes for coal-making vegetation; and, again, in the simultaneous northern change of level of the Glacial epoch. If distant lands, as these examples prove, moved in sympathy during some of the inferior vibrations of the crust, surely we may look for synchronous action during the raising of the greatest of its mountains. The earth has moved as a unit in all its grander steps of progress.

In view of such facts it is nothing suprising or improbable that the subdivison of time into Paleozoic, Mesozoic and Cenozoic should be registered in the strongest lineaments of the earth's surface.

ART. XXII.—*On the Homologies of the Insectean and Crustacean Types*; by JAMES D. DANA.

IN a note to the article on cephalization, at page 6 of this volume, a brief statement is made by the writer on the relations between the structures of Insects and Crustaceans. The following diagram and explanations will make the subject more intelligible.



The diagram presents to the eye the succession of normal segments in the two types, that of the Insect or highest Insectean, and that of the Decapod or highest Crustacean (including Crabs, Lobsters, &c.). The spaces between the vertical lines stand for the segments, which are numbered from 1 to 21. C stands for the cephalic portion or head; T, for the thorax; A, for the abdomen; C-T, for the cephalothorax.

The number of normal segments in a Crustacean has been so clearly and conclusively demonstrated by Milne Edwards, that it is unnecessary to add here to what has already been said on the subject. The series and its subdivisions are illustrated in the line above, opposite CRUSTACEAN: *fourteen* segments are shown to belong to the cephalothorax and *seven* to the abdomen. It is established beyond all doubt, that each segment corresponds to a single pair of members, as follows: number 1, to the eyes; 2, 3, to the two pairs of antennæ; then, in the *Decapod*, 4, 5, 6, 7, 8, 9, to organs of the mouth (or mandibles, maxillæ and maxillipeds); 10, 11, 12, 13, 14, to feet; and 15 to 21, to the abdomen.[1]

The abdominal members in all Decapods which have them, and four or more posterior pairs of thoracic members or feet in degradational forms of Decapods (as in Gastrurans or the Squilla group, and in Schizopods), are *two-branched*, or have two jointed terminations proceeding from the second segment: and this is the nearest approach in Decapods to that duplication of the pairs of legs to each segment which occurs in the Iuli and some other related Myriapods.[2]

As the true normal limit of the head in an animal is determined by the fact that this part includes the senses, mouth, and mouth appendages, (for this is demonstrated by the principles of cephalization already explained, if not established on other grounds,) the *head* in the Decapod includes *nine* segments, and the *thorax*, *five*, although there is no constriction of the body to make the division obvious to the eye.

The relation of the Insect-type to the Decapod is at once apparent from a comparison of the two lines in the preceding diagram. Supposing the parallelism rightly presented, the following facts are to be noted.

1. The Insect-type wants the 3 posterior segments of the Crustacean.

2. The head and thorax together of the Insect-type have the same number of segments (nine) as the head alone of the Decapod.

3. The head and thorax of the Insect-type contain *half* of its

[1] In the *Tetradecapod*, 4, 5, 6, 7, pertain to organs of the mouth, and 8, 9, 10, 11, 12, 13, 14, to feet.

[2] The writer has suspected that the multiplication of segments in the Phyllopods might be due to the basal part of each pair of feet becoming a separate body-segment, and that the branches corresponded to the double feet of the Iuli; but as the members in these multiplicative types appear often (if not always) to have the full number of basal joints, this view does not appear to be tenable.

total number of segments (eighteen); the same of the Decapod-type contains *two-thirds* of its total (twenty-one).

4. The head of an Insect contains six segments, which is *one-third* of the total in the Insect-type; that of a Decapod, nine segments, or *three-sevenths* of the total in the Crustacean type. [The head of a Tetradecapod, it may be added, contains seven, or *one-third* the total.]

5. The visceral segments (or those containing the viscera connected with digestion) are the 10th, 11th, 12th, 13th, 14th, in both the Insect-type and the Decapod-type. But in the Insect, the 10th is the first behind the thorax; and in the Crustacean, it is the first behind the head (or the mouth-organs.)[3] The last 2 or 3 normal segments in Insects (that is the 16th, 17th and 18th) are frequently wanting.

In the above homological comparisons, it is assumed that the three anterior normal segments present in a Crustacean are normally and potentially present in an Insect. This will be considered by many as the doubtful point in the above comparisons. But it is proved to be correct by the fact that these three segments are sense-bearing segments in Crustaceans, and the Insect fails in no sense belonging to the Crab. As stated on page 2 of this volume, the absence of a jointed organ is no proof of the absence of the segments, unless it be true, also, that the corresponding sense is wanting.

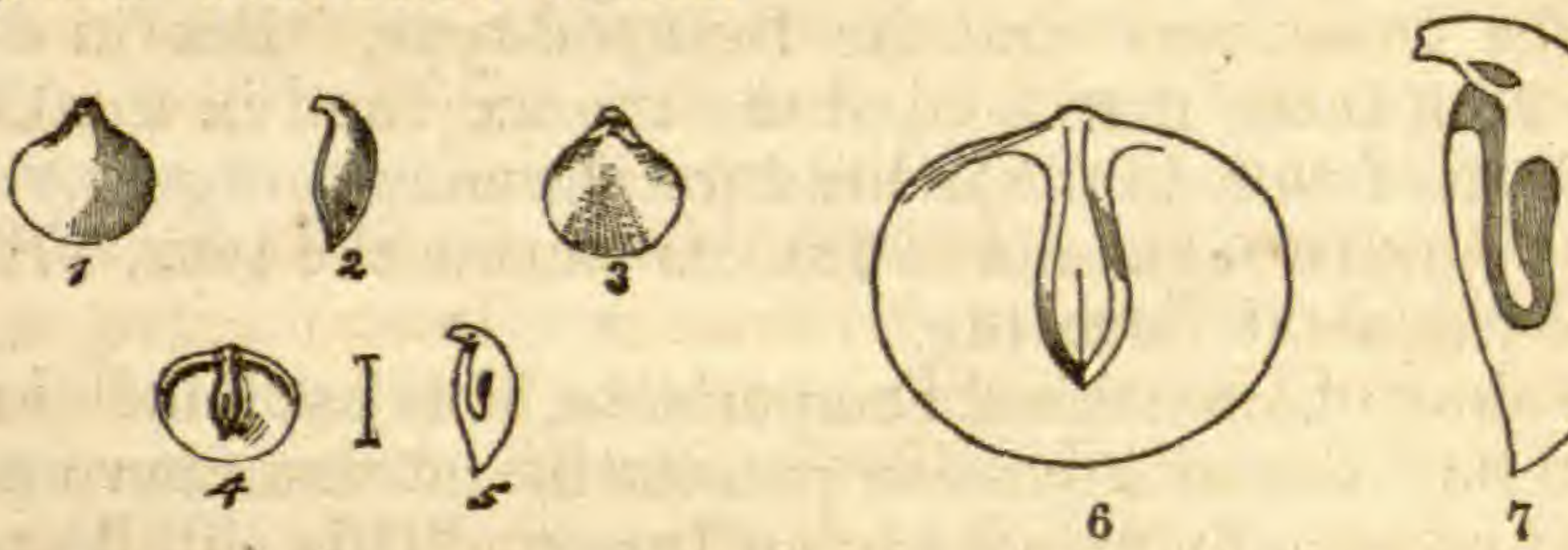
If the constitution of the anterior part of the head in the Insect be still questioned, there is nevertheless good reason for making the mandibular segment in the Articulate type—as it adjoins the centre in embryonic development from which progress goes on forward and backward—normally identical in all groups under that type; and, hence, from this segment, or No. 4 in the Crustacean series, on to No. 18, the parallelism between the Insect and Crustacean must be rightly given; consequently, if there is any doubt, it holds only with regard to Nos. 1, 2 and 3. The law of unity of structure under a type seems, however, to preclude even this chance for doubt.

Comparing the higher Decapods among Crustaceans and the higher Insects, the mean size or mass is about as 50 to 1. This ratio indicates approximately the amount of condensation in the Articulate structure connected with the elevation of grade from the typical Crustacean to the typical Insectean.

[3] Only in a degradational group of Decapods, that of the Gastrurans, do the viscera reach into the abdominal segments, or those following the 14th. The abdomen is very much elongated in these species, the cephalothoracic portion of the body is comparatively small, and the whole structure is lax and low in grade. The species thus stand apart from the Macrurans, as a separate tribe, equivalent with those of Brachyurans and Macrurans; while the Schizopods are only degradational Macrurans. See *this Journal*, [2], xxv, 338. In the fact that the viscera of the Squilloids or Gastrurans are contained in the *abdominal* portion of the animal, this group appears to approach the order of Insects. But this seeming approximation comes, as observed, through degradation, and is analogous to that between a Limulus and an Insect, as explained on page 6 of this volume.

ART. XXIII.—*On the genus Centronella, with remarks on some other genera of Brachiopoda;* by E. BILLINGS.—In a letter addressed to the Editors of this Journal.

Gentlemen:—As I fear that some confusion may arise with regard to the characters of the genus *Centronella*, I shall feel obliged by your permitting me to publish the original figures illustrating it, together with some others.



Figs. 1, 2, 3. Ventral, lateral, and dorsal views of *Centronella glans-fagea.*—Fig. 4. Interior of dorsal valve, showing the loop.—Fig. 5. Longitudinal section, showing the position of the loop in the interior.—Figs. 6, 7. Figures 4 and 5 as copied by Prof. Hall.[1]

The straight line, between figs. 4 and 5, indicates the total length of the ventral valve of the original specimen, and these figures are, therefore, nearly twice the natural size. Although not very perfect, they represent the form and position of the loop, as seen in the original specimens, correctly, with the exception of one part, (the small triangular process mentioned in the description) which is placed too near the beak. It is not shown at all in Professor Hall's copy of the figure. (Compare figures 5 and 7 above.) The specimen was silicified, and imbedded in a piece of limestone, and it was while dissolving it out, by the application of hydrochloric acid, that I first discovered the characters of the genus. The loop was reduced to a mere skeleton and being in a very fragile condition it fell off and was lost the very day my paper was read. I believe it was the first loop of the kind ever seen. The reflected portion of the loop, or the part represented in fig. 5 as being bent upwards towards the beak, was (in the original specimen), a single flat thin plate. The only difference between it and the plate of *Centronella Julia* (as figured by Prof. Winchell) is in the proportional breadth and extension forwards and backwards.[2] This at the most would be only a specific difference, but I have now some evidence that in *C. glans-fagea* the plate, when perfect, is as large as in *C. Julia*. I have lately succeeded in dissolving out four other specimens, showing the loop. Two of these are

[1] For Professor Hall's paper, referred to here and elsewhere in this article, see this Journal, [2], xxxv, 396, and xxxvi, 11.
[2] See Prof. Winchell's figure 2, this Jour., [2], xxxv, 400.

destitute of the vertical plate. The third showed it imperfectly, but before the loop was fully exposed, it (the plate) disappeared altogether. The fourth, of which I shall give a figure below, retains about half the plate, and is important, as it proves that in the original specimen the whole of the organ was not preserved, and that, when entire, there is little difference between it and that of *C. Julia.*

In publishing the figure of this specimen I beg to place alongside of it the important figure furnished by Dr. Rominger for Prof. Hall's paper, and also my original description of the loop. By comparing these it will be seen that the first view taken of the structure of this genus is the correct one.

"GENUS CENTRONELLA *Billings.*[3] Generic characters.—Shells, having the general form of *Terebratula.* Dorsal valve, with a loop consisting of two delicate ribbon-like lamellæ, which extend about one half the length. These lamellæ at first curve gently outwards and then approach each other gradually, until at their lower extremities they meet at an acute angle; then becoming united they are reflected backwards towards the beak in what appears to be a thin flat vertical plate. Near their origin, each bears upon the ventral side a single triangular crural process."

8. 9.

Fig. 8. A specimen showing the loop with part of the plate, enlarged two diameters.

Fig. 9. Dr. Rominger's figure showing the loop with the plate, broken off.

The remainder of the description compares *Centronella* with other genera and need not be repeated here. Prof. Hall says (p. 405 of his article) that Dr. Rominger's figure "is quite different from that given by Mr. Billings for *Centronella glans-fagea,* and shows essentially the same character as *Cryptonella.*" This expression was used by the distinguished author, to induce the idea that my figures indicate one genus and Dr. Rominger's another and a different genus. In reply, I shall only say that any naturalist can see that the agreement between Dr. Rominger's figure (so far as the latter goes) and my description is absolutely perfect. This figure differs from mine in representing a larger specimen with the plate broken off, in being drawn from a different point of view, and in being more artistically drawn; but it does not indicate a loop constructed upon a different generic plan. In studying the works of a naturalist, we should endeavor, earnestly, to arrive at his meaning by examining and comparing both his figures and his descriptions. The truth must be evolved out of the whole, not from a part. Prof. Hall, I regret to say, does not appear to have followed this course, as he makes no comment upon my description, but confines himself altogether

[3] Canadian Naturalist and Geologist, iv, 131, April, 1859.

to the figures which he misinterprets. The only question (in this connection) that can be of any interest to men of science is this.—*Is there in nature a genus of fossil Brachiopods having the general structure assigned to Centronella by me in* 1854? The discoveries of Prof. Winchell, Dr. Rominger and myself prove very clearly that there is, and I think I have a right to point out that Prof. Hall's recently published observations have added nothing but words to these discoveries.

I shall now offer a few remarks on the genus *Cryptonella*. This genus was first published by Prof. Hall in 1861, at which time, it was said to be founded on the species previously described by him under the names of *Terebratula Lincklœni*, *T. lens*, *T. rectirostra* and *T. planirostra*. Another species was said to be known to him, but it was not named, figured nor described. His description was confined almost altogether to the external characters. The muscular impressions were mentioned, but these give no clue to the structure of the internal organs (such as spires or loops). The substance of the description may be thus expressed.

GENUS CRYPTONELLA, *Hall*.—Generic Characters.—Shells, having the general form of *Terebratula*. Internal organs unknown.

I think this genus must stand or fall upon the structure of the internal organs of the species above mentioned, upon which it was originally founded by the author. Should it turn out that in their organization they possess nothing generically new, then, the name *Cryptonella* must become obsolete. It cannot be preserved by shifting it to another genus.

It will be observed that the new edition of the genus in Prof. Hall's paper (p. 401) is in fact founded on the loop of *Centronella Julia*. We have no evidence that this species is congeneric with those which were made the typical forms in 1861. There is no connection yet shown between the *Cryptonella* of 1861 and the *Cryptonella* of 1863.

I repeat that the genus *Cryptonella* can be sustained only by showing that the internal organs of the species upon which it was originally founded are different from those of all previously established genera. As these organs have never been seen, all that is known of the genus is expressed in the short description which I have given above in two lines. I do not say that it is not a new genus, but only, that we have as yet no published proof that it is.

The question whether *C. Julia* belongs to the genus *Centronella* is one of some importance, as its solution depends upon certain principles of classification much discussed of late. That our genera are founded on the modifications of the ultimate parts of animals, there can be no doubt; but how great an amount of modification is required to constitute a generic character is a matter of opinion. I have reason to believe that the internal organs of the fossil Brachiopoda are much more variable than is generally supposed. Some of these variations I shall mention presently. I shall first make some remarks on the punctate structure of the shell. Prof. Hall says (p. 397) that this character afforded him the principal means of distinguishing the typical species of *Cryptonella*, (as described in 1861) from *Meristella Haskinsi*, *M. Barrisii* and *M. Doris*, afterwards placed by me in *Charionella*. Now this statement astonished me not a little, as it was on account of his having described the shell of *M. Haskinsi* and *M. Doris* as being punctate that I was led to regard these two species as congeneric with *T. Linckleni*, *T. lens*, *T. planirostra* and *T. rectirostra*.[4] And when afterwards he figured the muscular impressions of *C. eximia*, I concluded that all of the species belonged to *Charionella*. I do not now think the punctate structure of the shell a good guide in classification, as it is a character which pervades the Brachiopoda widely and irregularly, without regard to the affinities of the groups of species in which it occurs.

The grounds upon which *C. Julia* is said to be separable generically from *C. glans-fagea* are the following.

1. The species of *Centronella* heretofore described have the "ventral valve highly convex or subangular in the middle, with the dorsal valve flattened or concave in the middle, or with a median depression, and convex at the sides." (Prof. Hall, p. 402.) In *C. Julia* both valves have a "regular lens-like convexity." In answer to this I have only to state that in almost all genera of Brachiopoda, where the species are numerous, similar differences in the form occur. Let us refer to *Waldheimia* the genus which seems to be nearest to *Centronella*. *W. resupinata* has almost exactly the form of *C. glans-fagea*. *W. carinata* has the form of *C. Hecate*. *W. numismalis* has the form of *C. Julia*. *W. Waltoni*, *W. lagenalis*, *W. ornithocephala* and *W. Celtica* are examples of elongate ovate forms like the typical species of Prof. Hall's genus *Cryptonella*. In *Terebratula* proper, numerous examples of similar and even greater differences might be cited.

2. The hinge, socket, and dental plates are also liable to small variations in structure in different species of the same genus. Thus, most species of *Orthis* have a well developed divaricator process in the dorsal valve. But in *O. Electra* and *O. Tritonia* there is not a vestige of this organ to be seen, the umbo being simply hollowed out into a triangular cavity, to the bottom of which the muscles for opening the valve were no doubt attached. In *O. porambonites*, this process appears in a rudimentary form,

[4] See the 13th *Regents' Report*, where, on p. 84, *M. Haskinsi* is described as having the "*Interior substance of the shell fibrous, with an exterior covering which appears to be punctate*," and on p. 85, *M. Doris* as having the "*shell-structure punctate*."

being represented by a narrow thread-like ridge. In others, it is larger, and, in many, strongly developed. In *Leptæna sordida* and *L. decipiens*, there is no divaricator process. In *Strophomena*, all the Lower Silurian species have a wide foramen; in the Middle and Upper Silurian rocks, species make their appearance with a much narrower aperture, and, in the Devonian, we find many with this opening reduced to a mere line, and some with it obsolete altogether. In this same series, we find also a gradual increase in the extent to which the area is striated; it being smooth in the Lower Silurian, partly striated in the Middle and Upper Silurian, and, in the Devonian, sometimes, as in *S. demissa*, ornamented the whole length with transverse lines.

We have here a gradual transition from *S. alternata* to *S. demissa*, in which two species the characters of the hinge and area are so different that they have been placed in different genera.[*] *Orthisina Verneuilii* has large dental plates, but *O. festinata* none at all. *Spirifera Mosquensis* has these plates extending more than half the length of the valve, but *S. mucronata* is destitute of them. Almost precisely the same differences exist between the internal characters of *Terebratula vitrea* and *T. elongata* as those relied upon for the separation of *C. Julia* from *C. glans-fagea*. [Thus (as described by Davidson), in *T. elongata* there are "*well-defined dental or rostral plates, leaving slits in the beak of the casts.*" Compare Prof. Hall's figure 4 of *C. Julia* on page 400, and his remarks on page 399, of his paper.]

[In *T. vitrea*, Davidson says, "*No prominent rostral plates, only a simple thickening of the shell, at the dental projections, which leave no slits in the beak of internal casts.*" Compare Prof. Hall's remarks and figures (p. 403), *C. glans-fagea.*]

Mr. Davidson does not consider this character of generic importance (see his *British Permian Brachiopoda*, p. 7, and the *Carboniferous Brachiopoda*, p. 11).

I think, therefore, that Prof. Winchell has rightly placed *C. Julia* in the genus *Centronella*, and that Prof. Hall's endeavors to separate it from that genus and make it the foundation of his *Cryptonella* will not be successful.

[*] There is one species, *S. rhomboidalis* in the Devonian and Carboniferous rocks, which retains the wide foramen and non-striated area of *S. alternata*. But this is a true Lower Silurian form, which appears to have sprung from the stock of *S. alternata*, and lived on through the Middle Silurian, Upper Silurian, and Devonian without change. It may be regarded as a remarkable instance of Darwin's theory of divergence. In the Lower Silurian period this species had numerous closely allied congeners. But during the interval to the Devonian the genus as a whole became gradually changed, *S. rhomboidalis* alone retaining the original aspect. In this comparison, all the species of *Streptorhynchus* are of course excepted.

Montreal, Canada, July, 1863.

ART. XXIV.—*On the Explosive Force of Gunpowder;* by Prof. F. A. P. BARNARD.

AUTHORITIES very widely differ as to the degree of strain to which heavy guns are subjected in experimental or in service firing; and still more widely in their estimate of the expansive force which gunpowder would be capable of exerting, could it be exploded in a space incapable of enlargement, which it exactly fills. The magnitude of the differences may be illustrated by the following examples.

In the work published in 1742 by Benjamin Robins, entitled "New Principles of Gunnery," the absolute expansive force of gunpowder exploded within its own bulk, is set down at one thousand atmospheres. This estimate was founded on certain experiments which may briefly be described as follows: First, by actually collecting the gases generated by the combustion of a given weight of powder, Mr. Robins inferred that these gases, reduced to the actual temperature previous to explosion, exceed, under the ordinary atmospheric pressure, the bulk of the powder by which they are produced in the ratio of 244 to 1. In order to ascertain the effect upon elasticity produced by the heat of combustion, he drew out a portion of a musket barrel into a conical form, leaving an orifice at that end of only one-eighth of an inch. The other end being closed, he subjected the apparatus to the highest heat of a furnace, and then immersed the conical end (which he first stopped with an iron plug) in water. After the tube had sufficiently cooled, he removed the plug and allowed the water to enter. The amount of the fluid found in the tube after the complete restoration of the original temperature, compared with the entire capacity of the tube itself, furnished the data for computing the expansion which the air had undergone in the furnace. The relative volumes of the air in the tube before and after expansion, when reduced to a common temperature, were determined, by a series of experiments of this kind, to be as 796 to 194½. Combining the data, Mr. Robins computed the maximum possible elasticity of the gases generated in the firing of gunpowder, at 999½ atmospheres; or, in round numbers, at 1000. It is here assumed that the heat of burning gunpowder is no greater than that of an ordinary furnace.

At a later period this subject was investigated by Gay Lussac. According to his determination, the bulk of the gases generated in the combustion exceeds the original bulk of the powder, in the ratio of 450 to 1. He estimates the temperature of combustion at 1000° C.; and computes the resulting elastic pressure at more than 2100 atmospheres.

Dr. Hutton, relying upon the approximate correctness of a formula which he had constructed for computing the velocities of projectiles fired from a gun, and taking as data the velocities actually observed, as ascertained by Robins' pendulum, concluded the maximum pressure to be somewhere between 1700 and 2300, thus substantially agreeing with Gay Lussac. The results of Dr. Gregory are not materially different from this, the maximum pressure being put by him at 2250.

In the year 1797, Count Rumford communicated to the Royal Society of London the results of an elaborate series of experiments upon the force of gunpowder, in which the estimates of pressure had been deduced among other methods from the observed effect of small charges of powder in lifting heavy weights. He puts the greatest force actually observed at about 55,000 atmospheres; but, as the charges filled but a portion of the cavity beneath the weight, he infers that the maximum pressure in a space entirely filled with the powder ought to be as high as 101,000 atmospheres.

The processes and results of Rumford are criticised by Piobert (*Traité d'Artillerie*, Paris, 1847), who regards them as unsatisfactory. According to his own determination, the maximum pressure should be about 7500 atmospheres.

Dr. Young, in his lectures on natural philosophy, quotes Euler, Lombard and D. Bernoulli, as giving for the same pressure the value of 10,000 atmospheres. He himself seems to favor a higher estimate, between 30,000 and 50,000 atmospheres.

In *Nichol's Cyclopedia of the Physical Sciences*, under the article "Gunnery," we find this statement: "Various experiments indicate the expansive force [of gunpowder] to be between 25,000 and 32,000 atmospheres." No authorities are cited.

In vol. xxii, 2nd Series of this Journal, is contained an article on the "Pressure of Fired Gunpowder," by W. E. Woodbridge, M.D., in which are detailed the results of some interesting experiments made by the writer, in connection with Maj. Mordecai, U. S. A., at the Washington Navy Yard, upon the pressure which guns actually endure in firing round shot. These experiments are deserving of study and will be examined hereafter; but they are not to the point immediately before us. Mr. Woodbridge however states that he exploded twenty grains of rifle powder in a cast-steel cylinder capable of enduring a pressure of 6200 atmospheres at the maximum—the powder entirely filling the cylinder, and the explosion being effected without escape of gas—without bursting the cylinder.

Mr. Woodbridge also quotes Gen. Antoni, of the Sardinian army, as authority for the statement that fine military powder fired in a cylinder of half an inch diameter and height, with no opening but the vent through which it is fired, exerts a pressure of 1400 to 1900 atmospheres.

In the *Encyclopedia Britannica*, last edition, article Gun-powder, Mr. Tomlinson, assuming that the gaseous products of the combustion of gunpowder are carbonic oxyd, sulphurous acid and nitrogen exclusively, computes a theoretic enlargement of volume as 1 : 787·3. Assuming further that the elevation of temperature is such as to treple this volume, he make the maximum pressure 2360 atmospheres.

The interesting Reports of Capt. Rodman, upon metals for heavy guns, and upon the qualities of cannon-powder, published in 1861 by authority of the Secretary of War of the United States, contain statements of experiments in which powder was actually exploded in a shell which it could not burst, but which it entirely filled, and of the force actually developed under these circumstances. In these experiments there was an orifice, one-tenth of an inch in diameter, through which the powder was fired, and through which the gases might of course more or less rapidly escape. The highest pressure registered by the gauge employed for the purpose, contrived by Capt. Rodman himself and described in the volume, was 185,000 lbs. per sq. in.—equivalent to more than 12,000 atmospheres. For certain reasons which he mentions, Capt. Rodman thinks that this is below the true pressure, "so that," he concludes, "I should feel perfectly safe in fixing the inferior limit of the pressure per square inch due to the combustion of gunpowder in its own volume, at, in round numbers, 200,000 pounds."

On the other hand, Mr. Norman Wiard, of New York City, a practical gun founder of large experience, who has given a great deal of attention to this subject experimentally, in a recently published essay, expresses a very strong conviction that all the estimates of the maximum possible pressure of gunpowder heretofore made are greatly in excess; and that this maximum pressure cannot exceed 743 atmospheres, or about eleven thousand pounds to the square inch.

These examples are cited not with any intention to exhaust the list of authorities, but simply for the purpose of illustrating the wide differences between them. None of the results presented can be said to rest upon entirely unexceptionable data; and among those which most largely differ, are some which seem to possess almost equal claims to acceptance. In the year 1857, however, there was published in Poggendorf's *Annalen* for November, a paper by Messrs. Bunsen and Schischkoff, of Heidelberg, entitled "*Chemische Theorie des Schiesspulver*," in which this subject is investigated with a thoroughness never before attempted, and the data are presented for determining the maximum force of gunpowder in a form which seems to leave nothing to desire.[1] This force is computed by them to be equal to

[1] This Journal, [2], xxxvi, 106.

4373·6 atmospheres. The objects successively aimed at by these investigators were to ascertain, first, by the most rigorous methods of analysis, the nature of the several products resulting from the combustion of gunpowder, and their relative quantities; secondly, the volume of the gaseous products reduced to 0° C., as compared with the original volume of the powder; thirdly, the volume of the fixed products, both at the temperature of experiment and at that of combustion; and finally, the absolute amount of heat evolved in the combustion, and (considering the capacities for heat of the several substances present, and their respective weights) the actual temperature of the whole mass in the instant in which the combustion is complete.

As it appears, in this investigation, that the fixed products occupy no inconsiderable portion of the space which the powder originally filled, it follows that the conclusions of Robins and Gay Lussac, had they been in other respects exact, would have materially underrated the maximum theoretic pressure; since the gaseous products, in an absolutely closed space, are by so much the more compressed as the cavity is practically diminished by the presence of the portions which are not gaseous.

It is impossible to read the article of Messrs. Bunsen and Schischkoff, without being strongly inclined to believe that their conclusion is as near the truth as it is possible, in an inquiry of so difficult a nature, to arrive. At the same time, one cannot fail to observe, in reading it, that they have furnished the means of testing the correctness of their result, by taking the velocities which projectiles of given weight, fired from guns of given length and calibre, are observed to have acquired at the moment of leaving the gun; and computing, according to recognized principles of physics, the initial pressures which would be necessary to produce such velocities.

In making such a computation, one assumption must be made (at least in the first instance) which is not true; and which, in so far as it is not true, will have the effect to make the computed maximum less than the real maximum pressure. This assumption is, that the powder is completely fired before the projectile begins to move. The same assumption was made by Robins and by Hutton, and it is implicitly involved in all the velocity formulæ which are found in treatises on artillery or on ballistics, at the present time. It being assumed, then, that all the gas which the powder is capable of producing is set free before the ball begins to move, we require to know, in order to determine the velocity it will generate in the projectile, the following particulars, viz: the original bulk of the gas, its initial temperature, its bulk at the atmospheric temperature and pressure, its capacity for heat both at constant pressure and at constant volume, the length of the bore of the gun, the part of this length occupied by the cartridge, and the weight of the projectile. All these data, except those which relate to the gun and projectile, are furnished by the investigation just cited; and the remainder may be deduced or directly taken from any table showing the initial velocities obtained by experiment, and the dimensions of the guns by means of which they were obtained.

Without such an investigation, no determination of the probable maximum pressure of gunpowder which could be deduced from the observed velocities of projectiles thrown by it could be entitled to any confidence. The rate of diminishing pressure of the mixed gases during expansion depends on the ratio of their capacities for heat at constant pressure and at constant volume: and of this nothing had been previously known. The formula of Hutton and the formulæ in present use, for calculating the initial velocities of cannon balls, are founded on the law of Mariotte for the relation of the pressure of a gaseous body to its density. This law furnishes a curve of pressures in which the ordinates diminish as the bulk increases much less rapidly than the real pressures; and accordingly, for the production of a given effect, it makes the higher pressures too low, to compensate for the excess of the lower.

The United States Ordnance Manual furnishes a variety of examples of the initial velocities observed in firing round shot from smooth bore guns of different calibres. The calculations which follow are founded on a selection from these examples. In order to obtain a formula suitable for the purpose, we suppose a to represent the length of the space, measured along the bore, which the liberated gases fill, provided they are entirely set free before the shot begins to move; x, the variable length, measured in like manner, which they fill at any time after the motion has commenced; F, the initial force by which the shot is urged; v, the velocity acquired, and γ the ratio between the capacities for heat of the gases as taken at constant pressure and at constant volume. This ratio requires to be so often referred to, that it seems to be desirable to have some mode of indicating it without circumlocution. The term *thermo-dynamic index* appears to be sufficiently significant, and is believed not to be preoccupied. It is therefore employed in the following discussion to denote the ratio in question. The conditions of the problem give us immediately the following, which is founded on Poisson's well known law for the pressure of expanding gases:

$$dv=\mathrm{F}\left(\frac{a}{x}\right)^{\gamma}dt. \quad \text{And we have also } dx=vdt.$$

$$\text{Hence } vdv=\mathrm{F}\left(\frac{a}{x}\right)^{\gamma}dx. \quad \text{And } v^2=\frac{2\mathrm{F}a^{\gamma}}{1-\gamma}x^{1-\gamma}+\mathrm{C}.$$

$$\text{But when } x=a,\ v=0\text{; and } \mathrm{C}=-\frac{2\mathrm{F}a^{\gamma}}{1-\gamma}a^{1-\gamma}=\frac{2\mathrm{F}a}{\gamma-1}.$$

Whence, finally,

$$v^2=\frac{2\mathrm{F}a}{\gamma-1}-\frac{2\mathrm{F}a^\gamma}{(\gamma-1)x^{\gamma-1}}=\frac{2\mathrm{F}a}{\gamma-1}\left(1-\frac{a^{\gamma-1}}{x^{\gamma-1}}\right)=\frac{2\mathrm{F}a}{\gamma-1}\left(\frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}\right).$$

In order to find the value of F, as compared with gravity, put p for the initial pressure of the gases in atmospheres, estimated at 14·72 lbs. per square inch, W_s for the weight of the shot, S_s for its specific gravity, and b for its diameter in a fraction of a foot, W_p for the weight of the powder and S_p for its specific gravity, 62·5 lbs. for the weight of a cubic foot of water, g for the force of gravity, represented by a velocity of $32\frac{1}{6}$ feet per second, c for the calibre of the gun in a fraction of a foot, L its length of bore in calibres, l the length of the cartridge, and n for the ratio of the weight of the powder to the weight of the shot, or $n=\frac{\mathrm{W}_p}{\mathrm{W}_s}$. Then the pressure per square inch of the section through the centre of the shot which gravity would produce is equivalent to $\frac{\mathrm{W}_s}{144b^2\times\frac{1}{4}\pi}$.

And we have the proportion

$$\frac{\mathrm{W}_s}{144b^2\times\frac{1}{4}\pi}:g::14{\cdot}72p:\mathrm{F}=\frac{14{\cdot}72\times144b^2gp\times\frac{1}{4}\pi}{\mathrm{W}_s}.$$

For W_s, substitute its equivalent, viz:

$$\mathrm{W}_s=62{\cdot}5b^3\mathrm{S}_s\times\tfrac{1}{6}\pi,$$

and we obtain the following:

$$\mathrm{F}=\frac{14{\cdot}72\times144b^2gp\times\frac{1}{4}\pi}{62{\cdot}5b^3\mathrm{S}_s\times\frac{1}{6}\pi}=50{\cdot}872\,\frac{gp}{b\mathrm{S}_s}.$$

Inasmuch as the whole mass of the powder (or of the products of its combustion) is moved as well as the ball, it is common to make some allowance for this circumstance by considering the weight of the ball to be effectively increased by a certain fraction of the weight of the charge. The fraction fixed on by Hutton as giving the most consistent results was one-third; and in this he has been generally followed. If we adopt the same value, we must substitute for W_s in the foregoing, $\mathrm{W}_s+\frac{1}{3}\mathrm{W}_p=\mathrm{W}_s+\frac{1}{3}n\mathrm{W}_s=\mathrm{W}_s(1+\frac{1}{3}n)=\mathrm{W}_s\left(\frac{3+n}{3}\right)$. Whence

$$\mathrm{F}=152{\cdot}616\,\frac{gp}{(3+n)b\mathrm{S}_s}.$$

The value of a is a certain fraction (to be presently determined) of the length (l) of the charge. To find l, we have

$$\mathrm{W}_p=c^2\times\tfrac{1}{4}\pi\times62{\cdot}5l\mathrm{S}_p.$$

Divide this by the value of W_s before given, and we have

$$\frac{\mathrm{W}_p}{\mathrm{W}_s}=n=\frac{c^2\times\frac{1}{4}\pi\times62{\cdot}5l\mathrm{S}_p}{62{\cdot}5b^3\mathrm{S}_s\times\frac{1}{6}\pi}=\frac{3c^2l\mathrm{S}_p}{2b^3\mathrm{S}_s}.$$

From which we deduce

$$l=\frac{2b^3nS_s}{3c^2S_p}.$$

The specific gravity of cannon powder, compacted as it is in the gun, is 1·039. That of cannon shot is 7. Substituting these numbers,

$$l=4{\cdot}491\,\frac{b^3}{c^2}\,n.$$

And if m represent the fractional part of l occupied by a, we obtain

$$a=4{\cdot}491\,\frac{b^3}{c^2}\,mn.$$

Substituting then, in the equation for velocity, the value of F; and also that of a in the factor without the bracket, there will result,

$$v^2=2\times152{\cdot}616\times4{\cdot}491\,\frac{b^3gmnp}{(\gamma-1)(3+n)bc^2S_s}\cdot\frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}$$
$$=8538{\cdot}27\,\frac{b^2}{c^2}\cdot\frac{np}{3+n}\cdot\frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}},$$

in which reduction we put $\gamma=1{\cdot}39$, as deduced by Messrs. Bunsen and Schischkoff, and employ for m the value ·52856, which will presently be shown to be just. We have therefore, finally,

$$v=92{\cdot}4029\,\frac{b}{c}\sqrt{\frac{np}{3+n}\cdot\frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}}.$$

And

$$p=\frac{v^2}{8538{\cdot}27}\cdot\frac{c^2}{b^2}\cdot\frac{3+n}{n}\cdot\frac{x^{\gamma-1}}{x^{\gamma-1}-a^{\gamma-1}}.$$

The increase of the weight of the shot in the foregoing formula, by one-third of the weight of the powder, first introduced by Hutton, and since generally adopted, is empirical entirely. A more just view of the case would be the following. Supposing the gases to be wholly liberated before motion begins, it is evident that, during motion, the stratum of gas next the projectile will be lower in temperature and less in density than the stratum next the bottom of the bore; since the expansion of the former will be opposed by the inertia of the projectile only, and that of the latter by the inertia of both the projectile and the charge. Considering, however, that the fire is communicated next the bottom of the bore, it is evident that the first effect of expansion will be to throw the powder, of which the combustion is yet incomplete, forward against the projectile. This effect may balance, or more than balance, the former, so that there can be no great error in assuming that the centre of gravity of the charge is always in the middle of its length. Before the explosion, the position of the common centre of gravity of the charge and the projectile may be found by the proportion,

$$W_s+W_p : W_s :: \tfrac{1}{2}(l+b) : k = \frac{\frac{1}{2}W_s(l+b)}{W_s+W_p} = \frac{\frac{1}{2}(l+b)}{1+n},$$

k being the distance of the common centre of gravity from the centre of the charge. The entire distance of this common centre from the bottom of the bore (which distance we will represent by q) is therefore

$$q = k + \tfrac{1}{2}l = \frac{l+\frac{1}{2}(b+nl)}{1+n}.$$

If, in any stage of the expansion, we represent the length of the charge by l', and the entire distance from the bottom of the bore of the common centre, by q', we shall in like manner obtain

$$q' = k' + \tfrac{1}{2}l' = \frac{l'+\frac{1}{2}(b+nl')}{1+n}.$$

And the movement of the common centre in the mean time, which is $q'-q$, will be

$$q'-q = \frac{2+n}{2+2n}(l'-l).$$

But $l'=x-a+l$; whence

$$q'-q = \frac{2+n}{2+2n}(x-a); \text{ and } d(q'-q) = \frac{2+n}{2+2n}\,dx.$$

If, therefore, we represent by v' the velocity which will be acquired by the common centre, we shall obtain the equation,

$$v'^2 = \frac{2Fa}{\gamma-1} \cdot \frac{2+n}{2+2n} \cdot \frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}};$$

in which F must have the value, $F=50{\cdot}872\,\frac{gp}{(1+n)bS_s}$, because the mass moved is now the entire weight of the powder added to that of the projectile. Substituting and reducing as before, there results the equation,

$$v'^2 = 2846{\cdot}09\,\frac{b^2}{c^2} \cdot \frac{pn}{1+n} \cdot \frac{2+n}{2+2n} \cdot \frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}.$$

Now the velocities of the common centre and of the projectile will be to each other as the spaces simultaneously passed over by them; and the squares of the velocities will be as the squares of those spaces. The space passed over by the common centre is $q'-q$; and that passed over in the same time by the projectile is $x-a$. Hence

$$(q'-q)^2\left(=\left(\frac{2+n}{2+2n}\right)^2(x-a)^2\right) : (x-a)^2 :: v'^2 : v^2 = \frac{(2+2n)^2}{(2+n)^2}v'^2.$$

And $$v^2 = 2846{\cdot}09\,\frac{b^2}{c^2} \cdot \frac{pn}{1+n} \cdot \frac{2+n}{2+2n} \cdot \frac{(2+2n)^2}{(2+n)^2} \cdot \frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}$$

$$= 5692{\cdot}18\,\frac{b^2}{c^2} \cdot \frac{pn}{2+n} \cdot \frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}.$$

And $v = 75{\cdot}4465\,\frac{b}{c}\sqrt{\frac{pn}{2+n}\cdot\frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}}.$

Also $p = \frac{v^2}{5692{\cdot}18}\cdot\frac{c^2}{b^2}\cdot\frac{2+n}{n}\cdot\frac{x^{\gamma-1}}{x^{\gamma-1}-a^{\gamma-1}}.$

If the value of v last found be divided by that obtained previously, the ratio is $\sqrt{\frac{6+2n}{6+3n}}$; which, when the powder is half the weight of the shot, is $=0{\cdot}9661$; showing that the velocity obtained by the method we have just been considering, is not, even with so excessive a charge, three and a half per cent less than that deduced from the empirical allowance of one-third the weight of the charge to the weight of the projectile. That allowance is therefore nearly correct. When n is put $=0$, or the weight of the powder disregarded, the two determinations agree, as they ought.

We will now attend for a moment to the method of deducing the value of p, *a priori.*

The materials employed in the manufacture of gunpowder, are mixed nearly in the proportion of one equivalent of saltpetre, one equivalent of sulphur, and three equivalents of carbon. If, in the combustion, the sulphur and potassium be supposed to combine, we may assume the results to consist of one equivalent of sulphid of potassium, one equivalent of free nitrogen, and three equivalents of carbonic acid. One gramme of powder will thus furnish, at 0° C. of temperature, and 0mm·760 of pressure, 82·50 c. c. of nitrogen and 248·40 c. c. of carbonic acid; in all 330·92 c. c. in volume of gaseous products. Any other *probable* combination of the gaseous elements will not increase this volume. The investigations of Messrs. Bunsen & Schischkoff demonstrate, however, that the sulphid of potassium forms but a small portion of the fixed residuum. The sulphur is to a great extent oxydized, and forms sulphate and sulphite of potassa, and a portion of the carbonic acid unites with the same base. A small portion of the nitrate appears also to be undecomposed. One gramme of powder, accordingly, furnishes but 193·1 cubic centimetres of gas.

The gunpowder employed by these experimenters contained an excess of saltpetre above the theoretic proportion, and a deficiency of carbon. The sulphur was also somewhat deficient, and there was about three and a half per cent of oxygen and hydrogen nearly in the proportions to form water. The gaseous products constituted 0·3138 of the total weight, and the solid residuum 0·6804.

In the gunpowder employed in the American military service, the saltpetre is slightly in excess, and also the carbon, the sulphur being in deficiency. It is probable that the results of its combustion would vary somewhat from those here detailed, but not to such an extent as very materially to affect the calculations which follow.

In the experiments of the Heidelberg investigators, the gunpowder was burned under the ordinary pressure of the atmosphere. It may be objected that, in the chamber of a gun, other forms of combination of the elements may take place; but, though this is possible, it is hardly supposable that greater power would in such a case be developed. A combustion which should produce a greater volume of gas would probably be attended with a less development of heat; so that as much as would be gained in elastic force by one of these circumstances would be lost by the other.

The specific gravity of the fixed residuum at 18° C. was found to be 2·35. By a method of determination devised by the experimenters, which is not described but in which they have full confidence, the specific gravity at the temperature of combustion is ascertained to be 1·5. The temperature of combustion itself was determined by very careful experiment. The details of the method pursued are interesting, but it is unnecessary to present them here. It was found that the heat developed by burning a given weight of powder would be sufficient to raise the temperature of an equal weight of water 619°·5 C. The specific heat of the mixed products of combustion was found, at constant pressure and at constant volume, by multiplying the specific heat of each ingredient by the amount per cent of it present, and taking the sum of the products. The elevation of temperature produced by the combustion, on supposition of no enlargement of the space occupied by the powder, is then obviously found by dividing 619°·5 C. by the specific heat at constant volume. This specific heat being 0·18547, the elevation of temperature is equal to 3340° C.

The specific heat at constant pressure is at the same time 0·20698.

In order to obtain the thermo-dynamic index of the gaseous portions of the mixture, the sum of the products formed, as just described, by the several capacities at constant pressure of those portions, must be divided by the sum of the products similarly formed, by the capacities at constant volume. These sums are, respectively, 0·07672 and 0·05520. Their quotient is 1·39, which is the value of the thermo-dynamic index.

In order to find the original bulk of the gases, or the magnitude of the space within which they are compressed if liberated without expansion, we consider that the bulk of the powder before combustion will be expressed in cubic centimetres by $\frac{W_p}{S_p}$; a cubic centimetre of pure water at maximum density weighing one gramme, and W_p being expressed in grammes. In like manner, if W_r be the weight of the fixed residuum, expressed in grammes, the bulk of the residuum will be $\frac{W_r}{S_r}$. The space occupied by the gases will therefore be $\frac{W_p}{S_p} - \frac{W_r}{S_r} = 0{\cdot}50872$ c. c. As, at 0° C. and $0^{mm}{\cdot}760$ of pressure, the same gases occupy 193·1 c. c. per gramme weight of powder, the elastic force due to difference of volume only would be expressed by

$$\frac{193{\cdot}1\ W_p}{\frac{W_p}{S_p} - \frac{W_r}{S_r}} = (\text{if } W_p = 1 \text{ gramme})\ \frac{193{\cdot}1}{\frac{1}{1{\cdot}039} - \frac{0{\cdot}6806}{1{\cdot}5}} = \frac{193{\cdot}1}{{\cdot}50872} = 379{\cdot}58.$$

Thus, the pressure in atmospheres would be 379·58, if there were no elevation of temperature produced by the combustion. The original bulk of one gramme of powder being $\frac{1}{1{\cdot}039} = {\cdot}96246$ c. c., and the space originally filled by the gases being 0·50872 c. c., the relative original bulk of gas and powder will be $\frac{{\cdot}50872}{{\cdot}96246} = 0{\cdot}52856$, which is therefore the numerical value of a in the foregoing formulæ, when l is unity.

The volume of the gases having been reduced to zero of temperature, the effect of an elevation by combustion of 3340° C., may be computed by assuming the absolute zero at −274° C., which is the latest determination as given by Rankine. Putting then p for the pressure, we shall have

$$p = 379{\cdot}58\left(1 + \frac{3340}{274}\right) = 5006{\cdot}5 \text{ atmospheres.}$$

This value exceeds that found by the experimenters themselves by 632·9 atmospheres. The difference is owing almost entirely to the difference in the assumed specific gravities of the powder: the experimenters having taken this at ·964, while we have employed the value 1·039. When gunpowder is not shaken down, its specific gravity is always less than 1, and when well shaken, is always more than 1. The U. S. Ordnance Manual (edition of 1850) gives ·929 for the specific gravity of loose powder, and 1·039 for that of powder well shaken down. It is obvious that, in a gun, we must adopt the higher value. There is also a slight difference between the determinations, owing to a difference in the assumed place of the absolute zero. The coefficient of expansion employed by the experimenters is $(1 + 0{\cdot}00366\,t)$, which corresponds to a zero at −273°·225 C. Adopting their specific gravity with the zero at −274°, the pressure would be 4364, or about ten atmospheres less than the determination of the experimenters.

We are now in condition to apply the formulæ above given, to the computation of the velocities which the initial pressure just assigned ought to be capable of generating in projectiles of given weight, fired with given charges of powder, from guns of given calibre and length; and also the initial pressures which would be necessary, in similar cases, to produce the velocities actually observed. The examples which follow, twenty-five in number, are taken from the U. S. Ordnance Manual, and exhibit the results actually obtained in experimental firing at the Washington Navy Yard. As the guns used were all smooth-bores, and the projectiles round shot, the observed velocity is corrected for the loss by windage. The formula for this correction which experiment has suggested, is,

$$C = A\frac{c-b}{c},$$

in which C is the correction, *c* and *b* have the values assigned them in the foregoing formulæ, and A is a constant determined by observation, and is usually put =6400 ft.

The particulars which enter into the calculation for each form of gun are the following:—

Kind of gun.	Calibre in inches.	Windage in inches.	L'gth of bore in calibres	Weight of projectile in pounds.	Charges, in pounds.
6 pdr. field,	3·67	0·09	15·67	6·15	1·25, 1·50, 2·00.
12 pdr. field,	4·62	0·10	16·00	12·3	2, 2·5, 3, 4.
12 pdr. siege,	4·62	0·10	22·38	12·3	2, 3, 4.
12 pdr. 25 cal.,	4·62	0·10	25·00	12·3	2, 3, 4, 5, 6, 7, 8.
24 pdr. siege,	5·82	0·14	18·56	24·25	3, 4, 6, 8.
32 pdr. sea-coast,	6·40	0·15	16·78	32·3	4, 5·33, 8, 10·67.

In the table which succeeds, are given the values of v which result from the formula when x is made equal to $L-l+a$; that is, when it has the value which belongs to it at the moment the shot leaves the muzzle. The columns "approximate values of n" and "No. of volumes expansion," are introduced for convenient comparison. The second consists of the values of $\frac{x}{a}$ at the moment of the expulsion of the shot. These numbers are approximate, like the values of n. In the calculation, the exact values are in all cases employed. The column of pressures contains the computed initial pressures which would be necessary to produce the velocities corrected for windage.

The results presented in the following table are certainly surprising. While anything like a close agreement between computation and observation was hardly to be expected, every reason for anticipating a discrepancy would indicate that the computed velocities should be in excess and not in deficiency; and the computed pressures in deficiency and not in excess. The formula assumes that the gases are fully developed before the shot begins to move. In point of fact we know that the combustion of cannon powder is far from complete even when the shot

Kind of gun.	No.	Approx val. of *n*.	No. of vols. expansion.	Velocity observed.	Velocity cor. for Windage	Velocity computed.	Dif. obs. and comp. Velocity.	Dif. cor. and comp. Velocity.	Pres. in atm. which would produce cor. veloc.
6 pdr. field.	1	$\frac{1}{5}$	34	1439	1596	1368	− 71	−228	6820
	2	$\frac{1}{4}$	28+	1563	1720	1465	− 98	−255	6899
	3	$\frac{1}{3}$	21−	1741	1898	1623	−118	−275	6842
12 pdr. field.	4	$\frac{1}{6}$	43+	1370	1508	1257	−113	−251	7208
	5	$\frac{1}{5}$	34+	1486	1624	1373	−113	−251	7008
	6	$\frac{1}{4}$	29−	1635	1773	1471	−164	−302	7274
	7	$\frac{1}{3}$	21+	1834	1972	1630	−204	−342	7329
12 pdr. siege.	8	$\frac{1}{6}$	61	1378	1516	1280	− 98	−236	7023
	9	$\frac{1}{4}$	40+	1674	1812	1505	−169	−307	7259
	10	$\frac{1}{3}$	30	1906	2044	1674	−232	−370	7461
Long 12 pdr.	11	$\frac{1}{6}$	68	1444	1582	1287	−157	−295	7567
	12	$\frac{1}{4}$	45	1742	1880	1515	−227	−365	7713
	13	$\frac{1}{3}$	34−	1951	2089	1688	−263	−401	7673
	14	$\frac{5}{12}$	27−	2098	2236	1825	−273	−411	7518
	15	$\frac{1}{2}$	22	2239	2377	1937	−302	−440	7539
	16	$\frac{7}{12}$	19−	2300	2438	2030	−270	−408	7222
	17	$\frac{2}{3}$	16+	2324	2462	2107	−217	−355	6833
24 pdr. siege.	18	$\frac{1}{8}$	68−	1240	1394	1127	−113	−267	7665
	19	$\frac{1}{6}$	50+	1440	1594	1270	−170	−324	7892
	20	$\frac{1}{4}$	33+	1723	1877	1489	−234	−388	7959
	21	$\frac{1}{3}$	25	1870	2024	1653	−217	−371	7511
32 pdr. sea-coast.	22	$\frac{1}{8}$	60+	1271	1421	1124	−147	−297	7994
	23	$\frac{1}{6}$	45	1430	1580	1266	−164	−314	7799
	24	$\frac{1}{4}$	30−	1640	1790	1482	−158	−308	7302
	25	$\frac{1}{3}$	22	1780	1930	1642	−138	−288	6913
							Mean of pressures,		7369

leaves the muzzle. A medium sized cannon powder has grains three-tenths of an inch in diameter—the largest sized, from six to nine-tenths. Piobert gives, as the conclusion arrived at after a very long and elaborate series of experiments on the rapidity of combustion of powder in lumps, that the combustion advances at the rate of half an inch per second. His conclusion is, moreover, positive, that neither heat nor pressure affect to any sensible degree this rate. A grain of powder $0^{\cdot in}.3$ in diameter would therefore be nearly a third of a second in burning, while Rodman's experiments prove that the shot of a 42 pdr. with 10 lbs. powder is but little more than five thousandths of a second in the gun after the fire takes the cartridge. We may, also, by a very simple process of calculation, show that Rodman's experimental determination cannot be far from correct. The actually observed velocity with which a round shot leaves a 12 pdr. gun of 16 calibres length, when the charge is $\frac{1}{4}$ of the weight of the shot, is 1635 ft. The length of the bore is 6 ft., which is therefore about the space through which the pressure acts on the projectile. If this pressure were *constant*, it would expel the ball in the time which it would take the ball to move *twelve* feet after leaving the gun—that is to say, in $\frac{12}{1635} = \frac{1}{140}$ of a second nearly.

But as the pressures near the muzzle are very far below the mean, those near the breech must be greatly above; or the time must be much less than this calculation gives. Since therefore the powder is not by any means completely burned—not even probably on supposition that Piobert's conclusions in regard to the effect of heat and pressure on rapidity of combustion are erroneous—before the elastic force of the developed gases ceases to act on the projectile, we have reason to suppose that, if our determination of maximum theoretic pressure is correct, the velocities computed by our formula will be much in excess of those obtained in actual experiment.

Again, all the circumstances of the actual experiment of which the calculation takes no account are in favor of excess on the side of the calculation. The weight of the sabot, the envelopes of the cartridge, the large amount of heat absorbed by the metal of the gun, the resistance of the air, &c.—all these particulars, if allowed for, would reduce the computed velocity. It is evident that there is error somewhere—either in the determination of the volume of gas, or in that of the heat developed, or in the assumption that the relation of elasticity to volume is the same under all pressures. What is known of carbonic acid, however, under high condensation at ordinary or at low temperatures, does not encourage the belief that its elasticity can, under any circumstances, increase more rapidly than in the inverse ratio of its bulk (temperature remaining constant), but makes it nearly certain that any change which should occur in this respect would be in the opposite direction.

There is one particular in which it is probable that our formula ought to be corrected. We have employed the thermodynamic index of the expanding gases, 1·39, as determined by the Heidelberg experimenters and by ourselves. In doing this, we have disregarded the fact that the fixed products of combustion, which have, originally, the same high temperature as the gaseous, are intimately mingled with the gases, in a state of minute division—that is, in the form of smoke. A true thermodynamic index for the time being must take account of the capacities for heat of the entire mixture; and will therefore be the quotient of the joint capacity at constant pressure, which is 0·20698, by the joint capacity at constant volume 0·18547. This quotient is 1·116 nearly. By substituting this value for γ instead of the former, we obtain results in which the differences are generally in the right direction, though by no means so great as we should be led to anticipate. These results are exhibited in the following table.

The mean of the computed pressures, in this table, is somewhat below the assumed pressure:—that is to say, the difference is in the right direction, but it is not a sufficient difference. It

Kind of Gun.	No.	Approx. val. of n.	No. of vols Expansion.	Velocity observed.	Velocity cor. for Windage.	Velocity computed.	Diff. obs. and comp. velocity.	Diff. cor. and comp. velocity.	Pres. in atm. which would produce cor. veloc.
6 pdr. field.	1	$\frac{1}{5}$	34	1439	1596	1687	248	91	4483
	2	$\frac{1}{4}$	28+	1563	1720	1790	227	70	4621
	3	$\frac{1}{3}$	21−	1741	1898	1954	213	56	4722
12 pdr. field.	4	$\frac{1}{6}$	43+	1370	1508	1563	193	55	4659
	5	$\frac{1}{5}$	34+	1486	1624	1689	203	65	4629
	6	$\frac{1}{4}$	29−	1635	1773	1793	158	20	4896
	7	$\frac{1}{3}$	21+	1834	1972	1958	124	−14	5081
12 pdr. siege.	8	$\frac{1}{6}$	61	1378	1516	1618	240	102	4396
	9	$\frac{1}{4}$	40+	1674	1812	1865	191	53	4724
	10	$\frac{1}{3}$	30	1906	2044	2046	140	2	4999
Long 12 pdr.	11	$\frac{1}{6}$	68	1444	1582	1635	191	53	4688
	12	$\frac{1}{4}$	45	1742	1880	1888	146	8	4965
	13	$\frac{1}{3}$	34−	1951	2089	2073	122	−16	5081
	14	$\frac{5}{12}$	27−	2098	2236	2217	119	−19	5092
	15	$\frac{1}{2}$	22	2239	2377	2331	92	−46	5206
	16	$\frac{7}{12}$	19−	2300	2438	2422	122	−16	5074
	17	$\frac{2}{3}$	16+	2324	2462	2498	174	+36	4863
24 pdr. siege.	18	$\frac{1}{8}$	68−	1240	1394	1431	191	37	4754
	19	$\frac{1}{6}$	50+	1440	1594	1591	151	− 3	5027
	20	$\frac{1}{4}$	33+	1723	1877	1829	106	−48	5274
	21	$\frac{1}{3}$	25	1870	2024	2000	130	−24	5127
32 pdr. sea-coast.	22	$\frac{1}{8}$	60+	1271	1421	1421	150	0	5007
	23	$\frac{1}{6}$	45	1430	1580	1577	147	− 3	5023
	24	$\frac{1}{4}$	30−	1640	1790	1811	171	21	4891
	25	$\frac{1}{3}$	22	1780	1930	1976	196	46	4774
						Mean of pressures,			4882

would produce a mean effect upon velocity equal to $\sqrt{\frac{4882}{5007}} =$ about one-eightieth of the whole. But, if we take the time in the gun at $0^s{\cdot}006$, and admit that the conclusions of Piobert in regard to the rapidity of combustion are not absurdly in error, we shall be forced to allow that only a fraction of the possible elastic energy of the generated gases comes into play; and that therefore, the computed velocities ought (*if we have correctly determined the maximum pressure*) are very greatly in excess of the velocities actually observed; and should probably exceed them a number of times.

It is probable that further experiment would prove, that, contrary to Piobert's opinion, the combustion of gunpowder must go on more rapidly in the chamber of a gun, than under the circumstances in which it is most easy directly to observe it. But it is not by any means probable that the rapidity of burning is ever great enough to permit us to discard the consideration of time from among the elements which must enter into this problem. On the contrary, the pressures registered by Rodman's indicator, when rifle powder and large grained cannon powder are successively fired in the same gun and with the same weight of projectile, show that the evolution of gas in the second case is much less rapid than in the first; and observers who have witnessed the firing of the fifteen inch guns state that the mammoth powder grains are visible in full combustion after leaving the muzzle of the gun.

Without a more extended examination of the subject than it has been found practicable to introduce into the present article, (designedly brief,) we should not perhaps be justified in stating an absolutely definite conclusion in regard to the maximum pressure which gunpowder is capable of producing; but it seems impossible to reconcile the actually observed velocities of projectiles thrown by it, with any supposition which should place this maximum at less than ten thousand atmospheres, or one hundred and fifty thousand pounds per square inch.

If we admit such a maximum, however, we find ourselves, on the other hand, obliged to discredit, to a certain extent, the results of the very able and elaborate investigation of Messrs. Bunsen and Schischkoff; and to suppose that they have underestimated the volume of the generated gases, or the amount of the heat developed. In the specimen of powder examined by them, the charcoal was in unusually small proportion. This may have produced a sensible effect upon their results; by leading to a larger production of sulphuric acid, and a smaller of carbonic. Still, after making every reasonable allowance for this consideration, we find it impossible to draw from their analysis a satisfactory explanation of the mechanical effects which gunpowder actually produces; and it seems exceedingly desirable that their investigation should be repeated.

One observation may here be made in regard to the allowance commonly made in calculation, for the effect upon the velocity of projectiles, of the windage of guns. In the table above, examples 1, 2 and 3 are similar in all respects except windage, to examples 5, 6 and 7. The computed velocities are, therefore, in the parallel cases, as they should be, almost exactly equal. But the actually observed velocities materially differ, No. 5 being greater by 47 than No. 1; No. 6, greater by 72 than No. 2; and No. 7, greater by 93 than No. 3. In the first set of examples, the windage is about $\frac{1}{45}$, and in the second $\frac{1}{46}$. The higher observed velocities correspond to the less windage. And it is observed that the effect on velocity of difference of windage seems to increase with the charge. It is thus rendered experimentally evident, that it is a great error to make the correction for loss of velocity by windage a *constant* for all charges in the same gun. That this is an error was indeed *a priori* probable. It is somewhat remarkable that it should have been so long permitted to stand unquestioned.

Washington, July, 1863.

ART. XXV.—*On Childrenite from Hebron in Maine;* by GEO. J. BRUSH.

IN an article on the occurrence of *amblygonite* at Hebron, published in this Journal, vol. xxxiv, p. 243, 1862, I mentioned that it was sometimes associated with a peculiar compact variety of apatite, containing minute prismatic crystals of a hair-brown mineral. The small amount of this hair-brown mineral, at that time in my possession, prevented me from determining fully its specific characters; but subsequent explorations of the Hebron locality, made by Mr. Oscar D. Allen, have furnished a sufficient quantity of the substance for examination, to lead to the conclusion that the mineral is probably a variety of *childrenite.*

It occurs in minute prismatic crystals, rarely over three lines in length and half a line in breadth and height: a full description of the crystalline form together with a comparison with the Tavistock childrenite is given beyond by Professor Cooke. Its other characters are as follows. Hardness =5. Specific gravity =3·03 (taken on less than half a gramme of fragments of crystals). Color dark hair-brown. Translucent. Lustre vitreous and brilliant. Streak white. Fracture uneven. Many of the crystals are partially decomposed, and converted into a lustreless earthy material. When heated in the closed tube, the unaltered material gives off neutral water, and the ignited residue is magnetic. Before the blowpipe in the forceps it swells up into ramifications and fuses on the edges, giving the flame a pale-green color, indicative of phosphoric acid. Fusibility =4, on v. Kobell's scale. Heated on charcoal becomes magnetic; with soda in the platinum loop gives a strong reaction for manganese; with borax and salt of phosphorus gives reactions for both manganese and iron. Decomposed by chlorhydric acid leaving traces of silica as an insoluble residue. Qualitative analysis proved the mineral to be a hydrous phosphate of iron, alumina and manganese.

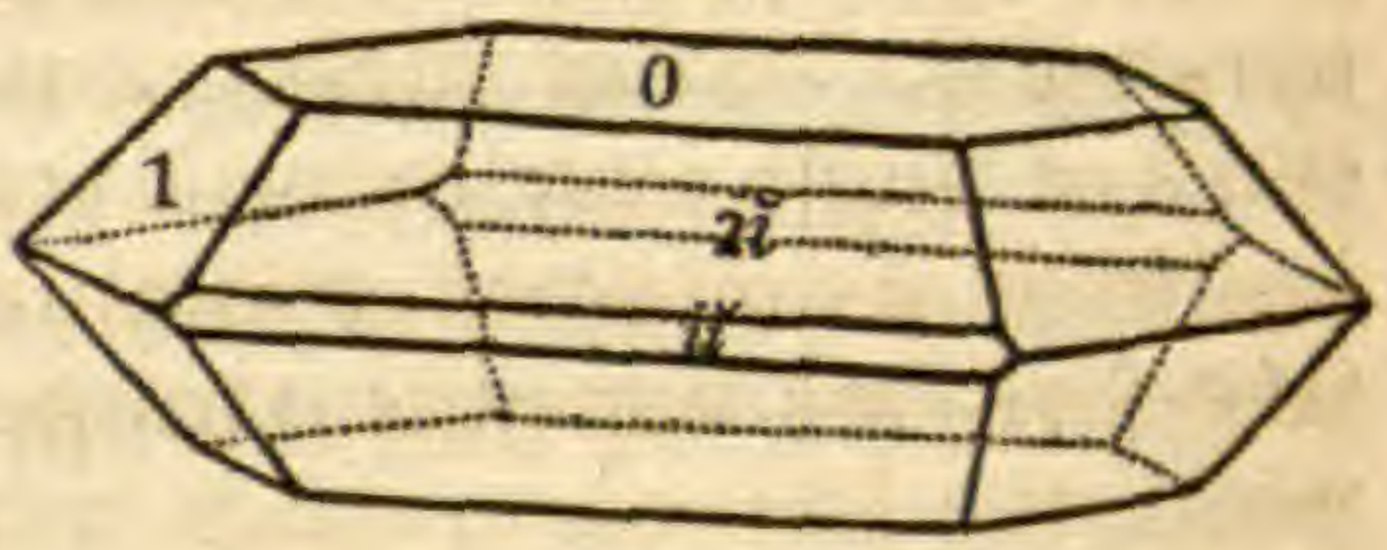
This composition, together with the physical and pyrognostic characters, seem to indicate that the mineral is identical with *childrenite*, a view that is further supported by the crystallographic examination made by Professor Cooke. Only a very few specimens of the Hebron childrenite have as yet been found. This rare species has never before been observed in America.

ART. XXVI.—*Crystallographic examination of the Hebron mineral, and comparison of it with the Childrenite from Tavistock;* by J. P. COOKE, Jr.

THE measurements, which I have made of the crystals from Hebron, sent me by Professor Brush, have afforded the following results.

Form trimetric.—Ratio of axes
$a:b:c = 0{\cdot}977:1:1{\cdot}482$.

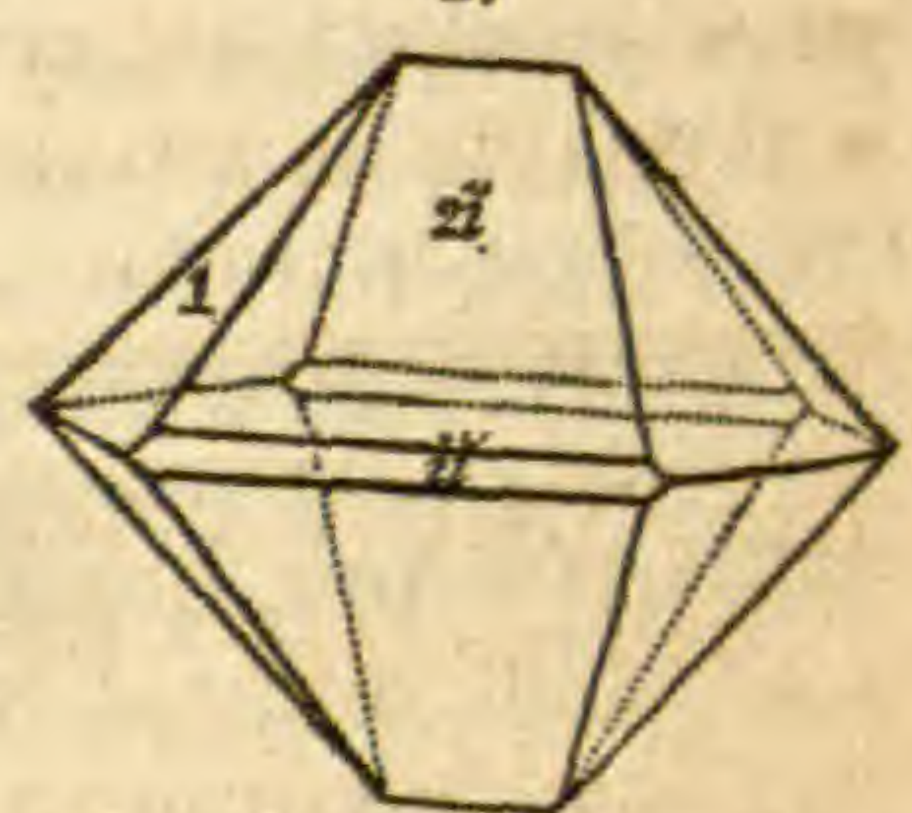
1.



	Observed.	Calculated.
1 on 1 (mac.)	129° 30′*	
1 on 1 (brach.)		101° 36′
1 on 1 (basal)		99° 22′
O on 2ĭ	127° 10′*	
2ĭ on 2ĭ (over *O*)	74° 20′	74° 22′

For comparison with the above, I have made the following measurements and figure of the childrenite of Tavistock in Devonshire, England.

2.



Ratio of axes—$a:b:c = 0{\cdot}969:1:1{\cdot}498$.

	Observed.	Calculated.
1 on 1 (mac.)	130° 10′*	
1 on 1 (brach.)	101° 43′*	
1 on 1 (basal)	98° 44′	98° 40′
2ĭ on 2ĭ (over *O*)		75° 24′

The values asterisked are in both cases those used in calculating the angles given in the second column. The faces 1 and 2ĭ, both on the crystals from Hebron and the specimens of childrenite from Tavistock, are so strongly striated parallel to the basal edges that only a few of the angles can in either case be accurately measured. Three of the Hebron crystals and two crystals of the Tavistock childrenite were examined, and the same angles on the different specimens of the same kind were found to agree very closely with the values given above in the column headed 'observed,' the extreme difference not exceeding four or five minutes, which is about the limit of the probable error of the measurements. The angles not given in this column were incapable of exact measurement on the specimen examined, on account of the striation just noticed. Although there is a difference of over half a degree in one of the fundamental angles, yet the general crystallographic characters of the two sets of crystals (the striation and lustre of the different faces) are so nearly the same, that there can be no doubt that the Hebron crystals are a variety of childrenite, differing perhaps from the English childrenite in some not fundamental point of chemical composition.

It will be noticed from the figures (in which the relative proportions of the planes have been preserved as nearly as was possible by the eye) that while on the Tavistock crystals the basal plane is wanting, it is very prominent on the specimens from Hebron, and gives to the crystals from this locality their marked prismatic character. This difference, although from our present mineralogical stand-point unimportant, nevertheless wholly alters the general appearance of the crystal, so that while the Hebron crystals are prismatic, and elongated in the direction of the brachydiagonal, the Tavistock crystals are pyramidal.[1] That differences like this, and even slight differences of angle may be occasioned by the nature of the material in which the mineral crystallized is generally admitted. When, therefore, we consider the peculiar character of the gangue in which the Hebron crystals were found, so different from that at Tavistock, we can hardly be surprised at differences of crystalline form as great as those described above. As already stated, the angles of the Hebron crystals above given are accurate only within four or five minutes, and it is to be hoped that, on further working the locality, more perfect crystals will be found, which will enable us to correct, if necessary, the above measurements.

ART. XXVII.—*Meteoric Iron from Dakota Territory—Description and analysis;* by CHARLES T. JACKSON, M.D., of Boston.

ON the 9th of June last, I received, through Messrs. John W. Shaw & Co. of this city, a mass of meteoric iron from John B. Hoffman, Esq., U. S. Indian Agent for the Ponca tribe of Indians. This mass of metal was supposed by Mr. Hoffman to be some native alloy of silver, and it was sent here to be assayed for that metal.

The mass in my possession weighs ten pounds ten ounces, and is six inches long, five inches wide, and about two inches thick, but is of an irregular form, the weathered or exterior surface being much indented, or wavy and pitted, while its opposite side is columnar, a natural fissure having existed between it and the large mass from which it was detached by the aid of a sledge

[1] At least this is true of all the specimens which have fallen under my observation, but in the original figure of the Tavistock crystals by Brooke, (Brande's *Quarterly Journal*, xvi, 274, and Dana's *Manual of Mineralogy*, ii, 424,) not only the basal plane *O* but also a second set of octahedral planes $\frac{4}{3}$ are represented. This figure in general appearance resembles quite closely the Hebron crystals and is very different from the pyramidal form we have given above. The difference is probably to be explained by the varying characters of the matrix in which the crystals occur at that locality, and it shows that, under certain conditions, the mineral has a tendency to the prismatic form which is only more fully developed in the specimens from Hebron.

hammer. It is stated that this piece was broken from a lump of the same kind, which was estimated to weigh 100 pounds.

It was found on the surface of the ground, in the Dakota Indian territory, ninety miles from any road or dwelling.

Where it has been rubbed and partially polished, the iron has a silvery appearance, and hence the mistake entertained as to its probable nature.

Excepting on the exterior of the columnar portions, which have a steel-like crust, the metal is very soft, and saws or files easily. It has a bright surface when cut. No earthy or stony matter has been found in it, and, judging from its great density, it appears to be solid in its interior. Pieces were sawed off in different places, and these were polished and tested with dilute nitric acid for the production of Widmannstättian figures, but none have thus far been produced, only a scaly like structure, quite fine, is developed by the acid, or when a lump of the iron is dissolved, ridges and fine projecting points are left on the undissolved metal. I noticed the singular phenomenon of the indifferent state of the iron to nitric acid, while dissolving this metal. After a rapid boiling effervescence, with a rush of red fumes of nitrous acid, the chemical action suddenly ceased and could not be renewed by the addition of more nitric acid, nor by a boiling heat, but on inclining the glass beaker, so as to cause the metal to come in contact with the other side of the glass, tumultuous chemical action instantly commenced, and the solution went on rapidly. This seems to show that the electrical state of the metal and of the glass was concerned in the indifferent state of the metal to the acid.

Chemical analysis of the Meteoric Iron.—Qualitative examination soon demonstrated the existence of nickel, phosphorus, tin, cobalt, and chromium in this meteorite.

Its specific gravity, taken with much care, was found to be 7·952. Its hardness that of the softest malleable iron, except on the exterior of the columnar portions, which were as hard as case-hardened iron, resisting the saw and causing a sharp cry under the file. No carbon was found.

The quantitative analysis was effected on two separate pieces, sawed from two of the columns, and the proportion of nickel was twice determined, the iron in both cases being removed as a succinate, by the well known processes.

By blowpipe examination, tin in metallic grains, was obtained, and the presence of small proportions of cobalt and chrome were proved. Phosphoric acid was found by molybdate of ammonia, and was in the analysis separated in the state of pyrophosphate of magnesia.

Although the analysis is not quite complete, yet it is enough so for our present purpose in demonstrating the meteoric nature of this metallic mass under examination.

The following are the per-centage results of my analyses, executed on a gram in each trial.

	1.	2.
Metallic iron	91·735	91·735
" nickel	6·532	7·080
Tin	0·063	0·063
Phosphorus	0·010=98·340	0·010=98·888

The cobalt was proved by the blue color the nickel gave with the borax bead, chrome, as shewn by the bead of the nickel-oxyd in microcosmic salt, the green color being persistent in the reducing flame and coming out as the red color produced by nickel in the hot bead faded. Chlorine was searched for in a solution of 53·7 grs. of the meteorite, but none was discovered.

I have requested Mr. Hoffman to procure and send to me the remainder of this interesting meteorite, and also to inquire of the Indians for other specimens and to procure them if possible. Though of no economical value, these specimens from beyond our world, are of great interest to science, and if our friends on the Pacific shore will look for them, I have no doubt many larger masses of meteoric iron may be found there.

Boston, Aug. 13, 1863.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS.

1. *On Celestial Dynamics;* by Dr. J. R. Mayer.[1]—[We avail ourselves of a translation by Dr. Debus, of v. Mayer's memoir, now in course of publication in the *London Ed. and Dublin Phil. Mag.*, to lay before our readers this remarkable paper, which has never before been printed in English. In connection with the researches of Joule it certainly marks an era in the history of physical science, and although first published fifteen years ago, the argument remains in all essential points unchanged by later researches. The recent appearance of Dr. Tyndall's most fascinating volume, "*Heat as a mode of Motion*,"[2] has given a degree of general interest to this whole subject which it never would have possessed had not the genius of Tyndall set it forth in a manner equally simple and delightful.—S.]

I. *Introduction.*—Every incandescent and luminous body diminishes in temperature and luminosity in the same degree as it radiates light and heat, and at last, provided its loss be not repaired from some other source of these agencies, becomes cold and non-luminous.

For light, like sound, consists of vibrations which are communicated by the luminous or sounding body to a surrounding medium. It is perfectly clear that a body can only excite such vibrations in another substance when its own particles undergo a similar movement; for there is

[1] *Beiträge zur Dynamik des Himmels, in populärer Darstellung*, von Dr. J. R. Mayer, Stadtarzt in Heilbronn. Heilbronn, 1848. Translated by Dr. H. Debus, F.R.S.
[2] Republished by Appleton, 12mo, pp. 480, New York, 1863.

no cause for undulatory motion when a body is in a state of rest, or in a state of equilibrium with the medium by which it is surrounded. If a bell or a string is to be sounded an external force must be applied; and this is the cause of the sound.

If the vibratory motion of a string could take place without any resistance, it would vibrate for all time; but in this case no sound could be produced, because sound is essentially the propagation of motion; and in the same degree as the string communicates its vibrations to the surrounding and resisting medium its own motion becomes weaker and weaker, until it at last sinks into a state of rest.

The sun has often and appropriately been compared to an incessantly sounding bell. But by what means is the power of this body kept up in undiminished force so as to enable him to send forth his rays into the universe in such a grand and magnificent manner? What are the causes which counteract or prevent his exhaustion, and thus save the planetary system from darkness and deadly cold?

Some endeavored to approach "the grand secret," as Sir Wm. Herschel calls this question, by the assumption that the rays of the sun, being themselves perfectly cold, merely cause the "substance" of heat, supposed to be contained in bodies, to pass from a state of rest into a state of motion, and that in order to send forth such cold rays the sun need not be a hot body, so that, in spite of the infinite development of light, the cooling of the sun was a matter not to be thought of.

It is plain that nothing is gained by such an explanation; for, not to speak of the hypothetical "substance" of heat, assumed to be at one time at rest and at another time in motion, now cold and then hot, it is a well-founded fact that the sun does not radiate a cold phosphorescent light, but a light capable of warming bodies intensely; and to ascribe such rays to a cold body is at once at variance with reason and experience.

Of course such and similar hypotheses could not satisfy the demands of exact science, and I will therefore try to explain in a more satisfactory manner than has been done up to this time the connexion between the sun's radiation and its effects. In doing so, I have to claim the indulgence of scientific men, who are acquainted with the difficulties of my task.

II. *Sources of Heat.*—Before we turn our attention to the special subject of this paper, it will be necessary to consider the means by which light and heat are produced. Heat may be obtained from very different sources. Combustion, fermentation, putrefaction, slaking of lime, the decomposition of chlorid of nitrogen and of gun-cotton, &c., are all of them sources of heat. The electric spark, the voltaic current, friction, percussion, and the vital processes are also accompanied by the evolution of this agent.

A general law of nature, which knows of no exception, is the following:—In order to obtain heat something must be expended; this something, however different it may be in other respects, can always be referred to one of two categories: either it consists of some material expended in a chemical process, or of some sort of mechanical work.

When substances endowed with considerable chemical affinity for each other combine chemically, much heat is developed during the process.

We shall estimate the quantity of heat thus set free by the number of kilogroms of water which it would heat 1° C. The quantity of heat necessary to raise one kilogram of water one degree is called a unit of heat.[3]

It has been established by numerous experiments that the combustion of one kilogram of dry charcoal in oxygen, so as to form carbonic acid, yields 7200 units of heat, which fact may be briefly expressed by saying that charcoal furnishes 7200° of heat.

Superior coal yields 6000°, perfectly dry wood from 3300° to 3900°, sulphur 2700°, and hydrogen 34,600° of heat.

According to experience, the number of units of heat depends only on the quantity of matter which is consumed, and not on the conditions under which the burning takes place. The same amount of heat is given out whether the combustion proceeds slowly or quickly, in atmospheric air or in pure oxygen gas. If in one case a metal be burnt in air and the amount of heat directly measured, and in another instance the same quantity of metal be oxydized in a galvanic battery, the heat being developed in some other place—say, the wire which conducts the current,—in both of these experiments the same quantity of heat will be observed.

The same law also holds good for the production of heat by mechanical means. The amount of heat obtained is only dependent on the quantity of power consumed, and is quite independent of the manner in which this power has been expended. If, therefore, the amount of heat which is produced by certain mechanical work is known, the quantity which will be obtained by any other amount of mechanical work can easily be found by calculation. It is of no consequence whether this work consists in the compression, percusssion, or friction of bodies.

The amount of mechanical work done by a force may be expressed by a weight, and the height to which this weight would be raised by the same force. The mathematical expression for "work done," that is to say, a measure for this work, is obtained by multiplying the height expressed in feet or other units by the number of pounds or kilograms lifted to this height.

We shall take one kilogram as the unit of weight, and one metre as the unit of height, and we thus obtain the weight of one kilogram raised to the height of one metre as a unit measure of mechanical work performed. This measure we shall call a kilogrammetre, and adopt for it the symbol Km.[4]

Mechanical work may likewise be measured by the velocity obtained by a given weight in passing from a state of rest into that of motion. The work done is then expressed by the product obtained by the multiplication of the weight by the square of its velocity. The first method, however, because it is the more convenient, is the one usually adopted; and the numbers obtained therefrom may easily be expressed in other units.

[3] The heat requisite to raise 1 kilogram of water 1° C. will heat 1 lb. av. of water 3·9681° F.

[[4] If one metre = 3·2808 English feet, and one kilogram = 2·2045 lbs. av., it follows that one Km = 7·2325 foot-pounds.—Tr.]

The product resulting from the multiplication of the number of units of weight and measures of height, or, as it is called, the product of mass and height, as well as the product of the mass and the square of its velocity, are called "*vis viva* of motion," "mechanical effect," "dynamical effect," "work done," "*quantité de travail*," &c.

The amount of mechanical work necessary for the heating of 1 kilogram of water 1° C. has been determined by experiment to be =367 Km; therefore Km=0·00273 units of heat.[5]

A mass which has fallen through a height of 367 metres possesses a velocity of 84·8 metres in one second; a mass, therefore, moving with this velocity originates 1° C. of heat when its motion is lost by percussion, friction, &c. If the velocity be two or three times as great, 4° or 9° of heat will be developed. Generally speaking, when the velocity is c metres, the corresponding development of heat will be expressed by the formula $0{\cdot}000139° \times c^2$.

III. *On the Measure of the Sun's Heat.*—The actinometer is an instrument invented by Sir John Herschel for the purpose of measuring the heating effect produced by the sun's rays. It is essentially a thermometer with a large cylindrical bulb filled with a blue liquid, which is acted upon by the sun's rays, and the expansion of which is measured by a graduated scale.

From observations made with this instrument, Sir John Herschel calculates the amount of heat received from the sun to be sufficient to melt annually, at the surface of the globe, a crust of ice 29·2 metres in thickness.

Pouillet has recently shown by some careful experiments with the lens pyrheliometer, an instrument invented by himself, that every square centimetre of the surface of our globe receives, on an average, in one minute an amount of solar heat which would raise the temperature of one gramme of water 0·4408°. Not much more than one-half of this quantity of heat, however, reaches the solid surface of our globe, since a considerable portion of it is absorbed by our atmosphere. The layer of ice which, according to Pouillit, could be melted by the solar heat which yearly reaches our globe would have a thickness of 30·89 metres.

A square metre of our earth's surface receives, therefore, according to Pouillet's results, which we shall adopt in the following pages, on an average in one minute 4·408 units of heat. The whole surface of the earth is =9,260,500 geographical square miles;[6] consequently the earth receives in one minute 2247 billions of units of heat from the sun.

In order to obtain smaller numbers, we shall call the quantity of heat necessary to raise a cubic mile of water 1° C. in temperature, a cubic mile of heat. Since one cubic mile of water weighs 408·54 billions of kilograms, a cubic mile of heat contains 408·54 billions of units of

[5] How this important result is obtained has been explained in my paper "*Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel.*"

[This essay was published in 1845. At that time de la Roche and Berard's determination of the specific heat of air was generally accepted. If the physical constants used by Mayer be corrected according to the results of more recent investigation, the mechanical equivalent of heat is found to be 771·4 foot-pounds. Mr. Joule finds it =772 foot-pounds.—Tr.]

[6] The geographical mile =7420 metres, and one English mile =1608 metres.

heat. The effect produced by the rays of the sun on the surface of the earth in one minute is therefore, 5·5 cubic miles of heat.

Let us imagine the sun to be surrounded by a hollow sphere whose radius is equal to the mean distance of the earth from the sun, or 20,589,000 geographical miles; the surface of this sphere would be equal to 5326 billions of square miles. The surface obtained by the intersection of this hollow sphere and our globe, or the base of the cone of solar light which reaches our earth, stands to the whole surface of this hollow sphere as $\frac{9,260,500}{4}$: 5326 billions, or as 1 to 2300 millions. This is the ratio of the heat received by our globe to the whole amount of heat sent forth from the sun, which latter in one minute amounts to 12,650 millions of cubic miles of heat.

This amazing radiation ought, unless the loss is by some means made good, to cool considerably even a body of the magnitude of the sun.

If we assume the sun to be endowed with the same capacity for heat as a mass of water of the same volume, and its loss of heat by radiation to affect uniformly its whole mass, the temperature of the sun ought to decrease 1°·8 C. yearly, and for the historic time of 5000 years this loss would consequently amount to 9000° C.

A uniform cooling of the whole of the sun's huge mass cannot, however, take place; on the contrary, if the radiation were to occur at the expense of a given store of heat or radiant power, the sun would become covered in a short space of time with a cold crust, whereby radiation would be brought to an end. Considering the continued activity of the sun through countless centuries, we may assume with mathematical certainty the existence of some compensating influence to make good its enormous loss.

Is this restoring agency a chemical process?

If such were the case, the most favorable assumption would be to suppose the whole mass of the sun to be one lump of coal, the combustion of every kilogram of which produces 6000 units of heat. Then the sun would only be able to sustain for forty-six centuries its present expenditure of light and heat, not to mention the oxygen necessary to keep up such an immense combustion, and other unfavorable circumstances.

The revolution of the sun on his axis has been suggested as the cause of his radiating energy. A closer examination proves this hypothesis also to be untenable.

Rapid rotation, without friction or resistance, cannot in itself alone be regarded as a cause of light and heat, especially as the sun is in no way to be distinguished from the other bodies of our system by velocity of axial rotation. The sun turns on his axis in about twenty-five days, and his diameter is nearly 112 times as great as that of the earth, from which it follows that a point on the solar equator travels but a little more than four times as quickly as a point on the earth's equator. The largest planet of the solar system, whose diameter is about $\frac{1}{10}$th that of the sun, turns on its axis in less than ten hours; a point on its equator resolves about six times quicker than one on the solar equator. The outer ring of Saturn exceeds the sun's equator more than ten times in velocity of rotation. Nevertheless, no generation of light or heat is observed on our globe, on Jupiter, or on the ring of Saturn.

It might be thought that friction, though undeveloped in the case of the other celestial bodies, might be engendered by the sun's rotation, and that such friction might generate enormous quantities of heat. But, for the production of friction, two bodies, at least, are always necessary which are in immediate contact with one another, and which move with different velocities or in different directions. Friction, moreover, has a tendency to produce equal motion of the two rubbing bodies; and, when this is attained, the generation of heat ceases. If now the sun be the one moving body, where is the other? and if the second body exist, what power prevents it from assuming the same rotary motion as the sun?

But, could even these difficulties be disregarded, a weightier and more formidable obstacle opposes this hypothesis. The known volume and mass of the sun allow us to calculate the *vis viva* which he possesses in consequence of his rotation. Assuming his density to be uniform throughout his mass, and his period of rotation twenty-five days, it is equal to 182,300 quintillions of kilogrammetres (Km). But, for one unit of heat generated, 367 Km are consumed; consequently the whole rotation-effect of the sun could only cover the expenditure of heat for the space of 183 years.——[*To be continued.*]

2. *Kirchhoff's Second Memoir on the Spectrum*[1] has just reached us. The map of the spectrum is continued in this memoir to embrace what was not given in the first, which contained, it will be remembered, the central portions from D to F. In the first plate of the 2d part we have A, B, C to D or 38° to 101°, and in the second plate, from F to part G or 256° to 287°. The work has been executed by Hoffmann, to whom it was entrusted by Kirchhoff, owing to the injury to his eyes in his former researches. Hoffmann has added the lines of numerous elements not before recorded, and gives a table of the atmospheric lines, and their coincidences with the elements and with the lines produced by the electric spark in atmospheric air.

3. *An Improved Spectroscope.—Analysis of the fixed line D;* by Professor Josiah P. Cooke, Jun. (Extracted, by permission, from a letter to Dr. Percy).—I have had a spectroscope constructed, which I believe to be the largest and most powerful ever yet applied to the spectrum. It has nine prisms, filled with CS_2, giving $2\frac{1}{4}$ inches aperture, with telescopes of corresponding size. By means of a conical wheel, against which the backs of the prisms rest, I am able to adjust them with great facility to the angles of least deviation. Two pins on the back of the prism are so adjusted that, when pushed against the wheel, the back of the prism is tangent to the circle. By means of this simple contrivance, I can make the adjustment from one end of the spectrum to the other in a very short time. The prisms are constructed on a plan suggested by my friend Professor Rood. They have wide frames with leveling screws. To the faces, pieces of the best plate glass are cemented, with a mixture of glue and honey. Outside of these, other plates are applied whose outer surfaces have been most carefully

[1] *Unterschungen über das Sonnenspectrum und die Spectren der chemischen Elemente*, by G. Kirchhoff (2nd Part), with 2 Plates. Berlin, 1863.

ground plain, with castor-oil between. This gives a very perfect prism. As the light is here bent through almost 360°, we have reached about the limit of power, unless we can reflect back the rays over the same path.

This instrument has established the following points:

1st. That the lines of the solar spectrum are as innumerable as the stars of heaven. It shows distinctly at least ten times as many lines as are given by Kirchhoff in his chart, and an infinitude of nebulous bands just on the point of being resolved. To give you an idea I enclose a drawing of the D line of Fraunhofer as seen by it. Kirchhoff gives only three lines—the two broad ones and a faint central one. You notice there are six others and a nebulous band.

Red end.

Violet end.

2d. It proves that the coincidences between the bright lines of the metallic spectra and the dark lines of the solar spectrum remain perfect, even with this greatly increased power. I am able to spread the two members of the sodium line so far apart that I can readily distinguish $\frac{1}{1000}$ of the intermediate space, and yet the coincidence with the two dark Fraunhofer lines is still absolute.

3d. It shows that many of the bands of the metallic spectra are broad colored spaces, crossed themselves by bright lines. This is the case with the orange band of the strontium spectrum, and with the whole of the calcium and barium spectra to a remarkable extent.—*Chemical News*, July 4, 1863.

4. *Spectrum of Phosphorus—Green coloration of hydrogen by phosphorus.*—Messrs. CHRISTOFLE and BEILSTEIN, starting from the fact, long ago stated by Wœhler (*Ann. der Chem. und Pharm.*, xxxix, 251), that phosphorous acid communicated a beautiful green color to a hydrogen flame, determine that pure phosphorus, introduced into the hydrogen generation apparatus, produces the same effect. Dusart has also shown the same (*Comptes Rendus*, xliii, 1126), and Blondlot has employed the same facts in toxical examinations for phosphorus (*Jour. de Pharm. et de Ch.*, [3], lv, 25). C. & B. have taken up the inquiry and, by means of the spectrum analysis, have obtained very precise results. This flame, examined by the spectroscope, showed two beautiful green lines on the left of the sodium band, besides a third, less vivid, between the two first and the sodium ray; the same lines are seen by using phosphorous and hypophosphorous acids.

An iron wire, supposed to be quite pure, introduced into the apparatus described, and attacked by HO, SO_3 to avoid any coloration from $H\,Cl$, gave a green hydrogen flame, and with the spectrum apparatus gave the same lines as phosphorus. Chemically pure iron (reduced from the oxalate by hydrogen) gave a colorless flame and no green ray in the spectrum.—*Comptes Rendus*, 2 March, 1863. 399.

5. *Osmium Spectrum.*—WILLIAM FRAZER (Dublin) finds that osmium affords three well marked lines of violet-blue color, and a fourth fainter one. These lines, measured by Steinheil's apparatus, occupied 6·0, 6·5, and 7·0 respectively, the fainter line appearing at 5·5, two being placed at the sodium line. It also affords a broad undefined band of blue light to the left of these lines.—*Chem. News*, *July* 18.

ANALYTICAL CHEMISTRY.

6. *On the Behavior of Dextrin and Gum Arabic toward Albumen.*—RUD. GÜNSBERG (*Sitzungsberichte der Wiener Academie, Mai*, 1862) finds that mineral acids, added to the turbid mixture of fresh white of egg and water, have the same effect as organic acids, if the acid be employed in *small quantity*, viz: the albumen goes into solution more perfectly, instead of being coagulated as happens if the mineral acid (but not the organic acid) be added in large quantity. The solution thus obtained is not coagulated by heating to boiling.

If, to a cold solution of albumen in a dilute mineral or stronger organic acid, a solution of dextrin, prepared from starch, either by help of diastase or sulphuric acid, be added, a heavy precipitate ensues, which shortly settles in form of flocks and is insoluble in excess of dextrin or of acid. The precipitate does not appear to be a simple combination of albumen and dextrin. Günsberg is occupied with its further study. Solution of gum arabic gives a precipitate with acid solution of albumen only when the latter is in large excess, and the precipitate immediately dissolves in excess of the gum. The solution of albumen which of itself is not coagulable by heat, becomes so on the addition of gum arabic. If, therefore, to an acid solution of albumen, gum arabic is added cautiously, a precipitate is formed which disappears on further addition of gum arabic. On heating the mixture, snow-white flocks separate. This observation furnishes a means of distinguishing these two carbo-hydrates. S. W. J.

7. *Detection of nitric acid in waters by means of Brucin.*—KERSTEN (*Ann. Ch. u. Ph.*, cxxv, 224) finds that $\frac{1}{100}$ of a milligramme of nitric acid in a cubic centimetre of water may be plainly detected with help of brucin. The reagents must be specially purified before they can be employed. The water is repeatedly rectified over potash. The brucin is washed with pure water several times to remove nitrates. English sulphuric acid is mixed with 5 per cent of carbonate of ammonia and $\frac{3}{4}$ distilled off in a glass vessel.[1]

Kersten dissolves the brucin in 1000 times its weight of water, pours 1 c. c. of this solution into a champagne glass, adds 1 c. c. of the water to be tested and finally 1 c. c. of sulphuric acid. The latter is allowed to flow down the side of the glass so as to gather beneath the water. At the plane of contact of the two liquids a rose-red zone immediately forms if the nitric acid be present in detectable quantity. S. W. J.

8. *New reaction for Veratrin.*—TRAPP, of St. Petersburg, has observed that the smallest traces of veratrin dissolve in cold concentrated chlorhydric acid, giving a colorless solution, which, on continued boiling, assumes a red color that finally becomes very intense and resembles that of permanganate of potash. This solution remains unaltered by standing for a long time.—*Polytechnisches Notizblatt*, 1863, 96.

9. *Reaction for Molybdenum.*—According to BRAUN, (*Zeitschrift für Analyt. Ch.*, 1863, 36,) sulpho-cyanid of potassium gives, with certain solutions of molybdenum, a red color similar to that produced by the same reagent in solutions of per-salts of iron. The brown solution of Mo_2O_3 in HCl, mixed with concentrated solution of an alkali-sulpho-

[1] According to Goppelsröder (*Verhandl. der naturforsch. Gesells. in Basel.* 1861, 159) *fuming oil-of-vitriol* is free from oxyds of nitrogen. This observation has been confirmed in the Sheffield Laboratory.—[S. W. J.]

cyanid, yields a reddish-yellow liquid which gradually becomes darker and finally appears carmine red.

This reaction is obtained with molybdic acid or solutions of molybdates by putting a fragment of zinc into the liquid, adding a few drops of strong solution of sulpho-cyanid of potassium, and, finally, a little sulphuric or chlorhydric acid, so that a gentle evolution of hydrogen is excited. The red color shortly appears, though it is not permanent.

In this way, $\frac{1}{300000}$ of molybdic acid is recognizable, a quantity less than can be detected by the usual reagents. The sulpho-cyanid of molybdenum is soluble in ether and is taken up by this liquid when agitated with it. It is not dissolved by chloroform or sulphid of carbon.

Since ferric oxyd and hyponitric acid give with alkali-sulpho-cyanids red liquids, they interfere with the direct detection of molybdenum. C. Claus has observed that oxalic and phosphoric acids destroy the sulpho-cyanid of iron, and that in presence of these bodies ferric oxyd is unaltered by the alkali-sulpho-cyanids. Oxalic acid converts the carmine red of sulpho-cyanid of molybdenum into a reddish-yellow color. Free phosphoric acid has, however, no effect on strong solutions of sulpho-cyanid of molybdenum. The red coloration caused by hyponitric acid in solution of alkali sulpho-cyanids, is destroyed by addition of urea or alcohol as well as by addition of oxalic or phosphoric acid. In order then to recognize molybdenum in presence of ferric oxyd and hyponitric acid, the solution under examination is first boiled with chlorhydric acid to destroy the hyponitric acid, then treated with phosphoric acid and finally with zinc and sulpho-cyanid of potassium. S. W. J.

10. *On the quantitative estimation of Arsenic.*—WITTSTEIN (*Zeitschrift für analytische Chemie*, 1863, 19) observes that the process of drying the ammonio-arseniate of magnesia in vacuo is extremely tedious, while at 100° C. loss of ammonia may occur. He recommends to expel all the water and ammonia and weigh the pyro-arseniate of magnesia. To do this, it is necessary to heat the substance cautiously and gently in a sand-bath, until the ammonia is expelled and the original snow-white color has passed into milk-white. Then the heat is gradually increased until the porcelain crucible almost glows. The residue is $2MgO\,AsO_5$, and no loss of arsenic is to be feared, except through too-rapid heating. S. W. J.

TECHNICAL CHEMISTRY.

11. *On the manufacture of Soda, Chlorine, and Sulphuric and Chlorhydric Acids;* by THOMAS MACFARLANE.—In the *Canadian Naturalist* for Feburuary, 1863, Mr. Macfarlane has described a series of processes for the decomposition of sea-salt and the manufacture of soda, chlorine and sulphuric and chlorhydric acids. These new methods, which the inventor has just patented in England, are interesting both in a theoretical and a practical point of view. The starting point in these processes is the fact that when a mixture of dried green vitriol and sea-salt is heated to redness, in a current of air, sesquichlorid of iron is first formed, and then decomposed into peroxyd of iron and chlorine, so that the residue is sulphate of soda and peroxyd of iron. This reaction is facilitated by an admixture of peroxyd of iron, which renders the mass less fusible and keeps it in a porous state. 828 parts of green vitriol are dried and partially peroxydized by a gentle heat, and are then intimately

mixed with 352 parts of sea-salt, and 78 of peroxyd of iron. The whole is then heated to low redness in a muffle calcining furnace, the muffle of which is connected with an exhausting apparatus, by means of which air dried by passing over lime is brought in contact with the mixture. The temperature of this should be kept so low that no perchlorid of iron is sublimed. The mixture is carefully stirred from time to time, and the whole of the chlorine is thus obtained in a gaseous state, mixed with nitrogen, but available for the preparation of bleaching salts and for other purposes. The muffle now contains a mixture of peroxyd of iron and sulphate of soda, for it is claimed that under the above conditions the decomposition of the chlorid of sodium is complete. This mixture is ground with 144 parts of coal and heated to fusion in a reverberatory furnace, the hearth of which is made of ground quick-lime mixed with a little basic slag or glass, and saturated with sulphuret of sodium by means of an admixture of sulphate of soda and coal melted upon its surface. The fused mass after cooling is treated with water, and yields a residue of sulphuret of iron, and a solution of caustic soda colored greenish by a portion of suspended or dissolved sulphuret of iron, which is however precipitated when the carbonic acid from the furnace is passed over the solution, yielding a solution of caustic soda and carbonate of soda which is treated by the ordinary methods.

The residue of proto-sulphuret of iron is washed, and then exposed while moist, on a perforated wooden floor covered with canvass, to the action of the air, by which it is soon converted into sulphate of iron. This is removed by solution from the residue of peroxyd of iron, and we have again the two substances necessary for the decomposition of a new portion of sea-salt.

In this process, the use of sulphuric acid and of peroxyd of manganese are dispensed with, and by the aid of a certain amount of sulphate of iron, which can be employed an indefinite number of times, sea-salt is converted into soda and chlorine, without the employment of any other reagent than a little carbon and the oxygen of the air. The whole of the chlorine is also obtained in an available form, while by the use of peroxyd of manganese one half of this element remains in the state of chlorid. The decomposition of sulphate of soda by peroxyd of iron and charcoal was patented some years since by Blythe and Kopp, but they obtained the sulphate of soda by the action of sulphuric acid on sea-salt, and burned the resulting sulphuret of iron to prepare new portions of the acid.

For the preparation of sulphuric and muriatic acids, Mr. Macfarlane employs, in connection with the chlorine evolved by the method just described, the sulphurous acid obtained by burning sulphur or by calcining iron pyrites. The two gases being mixed in equivalent proportions, and passed with a jet of steam through a condenser filled with coke, yield sulphuric and chlorhydric acids, in accordance with the equation, $SO_2+HO+Cl=SO_3+HCl$. The mixed acids are separated by distillation.

Another process proposed by Mr. Macfarlane for the manufacture of the two acids consists in calcining a mixture of one equivalent each of iron pyrites and sea-salt, with four equivalents of peroxyd of iron. Sulphurous acid is at first evolved, but, in presence of the peroxyd, so much of this acid is oxydized as to form sulphate of iron sufficient to convert the greater part of the sea-salt into sulphate of soda and chlorine gas, which is thus produced in the second stage of the heating. By charging a series of furnaces with this mixture, and bringing together the chlorine from one and the sulphurous acid from another in the presence of aqueous vapor, a constant production of sulphuric and chlorhydric acids may be kept up, while the soda, with one half of the sulphur of the pyrites, is obtained in the form of sulphate of soda. In a previous paper in the *Canadian Naturalist* for 1862 (page 194), Mr. Macfarlane has detailed numerous experiments made with reference to this transformation into sulphate of soda and chlorine of a mixture of pyrites and sea-salt in presence of a large excess of peroxyd of iron. The use of chlorine for the purpose of converting sulphurous into sulphuric acid was patented by Halmer in 1854, he, however, proposed to employ the chlorine obtained by means of oxyd of manganese, which would probably render his process more expensive than the common one, which depends upon the use of nitrous acid. T. S. H.

PHYSIOLOGICAL CHEMISTRY.

12. *On the excretion of Nitrogen in animals.*—The experiments of Regnault and Reiset, as well as those of Boussingault, have conducted to the assumption that a portion of the nitrogen which a mature animal consumes in its food escapes from the organism in a gaseous form as free nitrogen or ammonia. Bischoff and Voit, in their classical research on the Laws of Nutrition of the Carnivores,[2] are believed to have established, so far as experiments with a single animal, a dog, could serve, that all the nitrogen of food is excreted through the kidneys and intestines and is found again in the urine and fæces.

This result has been confirmed by Henneberg in case of Ruminants, by J. Lehmann in case of swine, and by Joh. Ranke in case of man.

All these experiments were however open to one objection, viz: it was possible that a loss of gaseous nitrogen resulting from a waste of tissue might occur, although the nitrogen of the excreta were equal to that of the food. In such a case the animal must continuously lose flesh.

In order to silence all cavil and to establish so important a law on an irrefutable basis, Voit (*Ann. Chem. u. Ph.*, ii, Sup. Bd., p. 238) has carried out a new series of observations, employing as the subject a pigeon, the animal which according to Boussingault exhales 35 per cent of the nitrogen of its food in a gaseous form. To settle the point in question, it was necessary, Voit remarks, to supply to the animal under trial a definite nourishment for a long period of time. If then, the excreted should be found equal to the ingested nitrogen, any loss of nitrogen by exhalation would result in the wasting away and final death of the animal.

Voit fed a pigeon 124 days (from Oct. 5, 1861, to February 6, 1862) exclusively with peas. The bird consumed 3642·8 grm. air-dry =3132·4 grm. dry (at 100° C.) peas, which contained 4·77 per cent =149·4 grm. of nitrogen. The excrements dried at 100° C. weighed 976 grm. and contained as the mean result of 12 analyses 14·95 per cent =145·9 grm. of nitrogen or 3·5 grm. less than was in the food. The pigeon had

[2] *Die Gesetze der Ernährung des Fleischfressers durch neue Untersuchungen festgestellt.* Leipzig and Heidelberg, 1860.

gradually gained during the trial 70 grm. in weight. Assuming that this increase consisted in nitrogenized tissue, as must be supposed from the character of the food, it corresponds to 2·4 grm. of nitrogen. This added to the quantity found in the excrements leaves but 1·1 grm. unaccounted for, a quantity admitting of a loss of $\frac{8}{10}$ milligramme daily, and so small, considering the duration of the trial, as to be safely attributable to errors of experiment. S. W. J.

II. METALLURGY.

1. NEW WORKS.—*Zusammenstellung der statistischen Ergebnisse des Bergwerks-, Hütten- und Salinen-Betriebes in dem Preussischen Staate während der zehn Jahre von* 1852 *bis* 1861; Bearbeitet von E. ALTHANS, 4to, pp. 156, with 4 lithographic plates. Berlin, 1863.—This valuable collection of statistics of the mineral production of the Prussian States for the ten years, 1852–61, is published as a supplement to the 10th volume of the *Zeitschrift für das Berg- Hütten- und Salinenwesen in dem Preussischen Staate.*

From this we abstract the following in regard to the productions for the year 1861:

Total value of mineral products,	31,234,628	Thalers.
Number of mines worked,	2,304	
Number of workmen employed,	115,341	
Average amount produced by each mine,	13,537	Thalers.
" " " workman,	271	"

During the past twenty-five years the value of the mineral products has increased sixfold, the number of workmen $3\frac{1}{2}$ times, and the number of mines has increased from 1587 to 2304.

We have not space to give further details from this interesting collection, but trust that our government will take some such model as this work for the future reports in regard to the Metallic and Mineral Statistics of the United States; for the absurdities contained in Mr. Kennedy's "*Preliminary Report on the Eighth Census*" (1860), as Prof. J. D. Whitney[1] has pointed out, are such as can only mislead and confuse those who resort to Government documents for information in regard to our metallic and mineral productions. G. J. B.

2. *Handbuch der metallurgischen Hüttenkunde*, von BRUNO KERL, vol. ii, 8vo, pp. 848, with 8 lithographic plates. Freiberg, 1863.—This second volume of the new edition of Kerl's work on Metallurgy treats of the special metallurgy of lead, copper, zinc, cadmium, tin, mercury and bismuth. In the first edition these subjects occupied 384 pages, while in the present volume more than twice the space is covered by them. The completeness and accuracy of Kerl's work commends it not only to practical metallurgists, but also to all scientific men who desire to have a correct idea of those principles in chemistry and metallurgy which find so extended an application in the arts, in the extraction of the useful metals from their ores. No work on metallurgy with which we are acquainted combines so many excellent qualities as this; it is exceedingly well arranged and easy of reference, and conscientiously scrupulous in the citation of authorities and in giving references to original memoirs. The classification of processes, and their illustration by

[1] *Proc. Calif. Acad. Nat. Sci.*, vol. iii, p. 6.

examples, show that the author is not only master of his subject as a practical metallurgist, but that he also has rare skill as a teacher in science. It is a work that should be in every public library, and in the hands of every metallurgist and practical chemist. It is to be completed in four volumes, accompanied with lithographic plates containing drawings of furnaces, etc. G. J. B.

3. *Etat present de la Métallurgie du Fer en Angleterre*, par MM. GRUNER et LAN. 8vo, pp. 850, with nine plates. Paris, 1862.—This work has for the most part been published in a series of memoirs in the *Annales des Mines;* but many who have not the numbers of that valuable Journal will be glad of the opportunity to obtain as a separate book so important a record of the present state of the metallurgy of iron in England. M. Gruner is the Professor of Metallurgy in the Imperial School of Mines at Paris, and M. Lan has a like position in the School for Mines at Saint Etienne. These gentlemen were sent by order of the Minister of Public Affairs in France, in May and June, 1860, to report upon the iron districts of Great Britain; they were offered every facility for making their investigations, and the results which they have published in this volume form a most important contribution to the metallurgy of iron. G. J. B.

4. *Berg- und Hüttenmännisches Jahrbuch der k. k. Bergakademien Leoben und Schemnitz, und der k. k. Montan-Lehranstalt Příbram-Redakteur:* P. TUNNER. 8vo, 261 pp. Wien, 1863.—This annual of the Mining Academies of Leoben and Schemnitz is one of the most important repositories of metallurgical information. The present 12th volume, edited by Director Tunner, is of more than usual interest, as almost half of it is taken up with a report on the objects of interest in metallurgy which were contained in the International Exhibition at London in 1862. G. J. B.

5. *Die Fortschritte des metallurgischen Hüttengewerbes im Jahre*, 1862. Dargestellt von Dr. CARL. FR. ALEX. HARTMANN. Sixth volume, with 3 lithographic plates in folio. 8vo, pp. 352. Leipzig, 1863.—This work is an annual report of the progress of metallurgy, giving a review of all that is published in regard to the metallurgical treatment of the useful metals and new facts in regard to fuel, blowing machines, and furnaces. G. J. B.

6. *International Exhibition of* 1862. *Jurors Report of Class I—Mining, Quarrying, Metallurgy, and Mineral Products.* Reporter, WARRINGTON W. SMYTH, M.A., F.R.S., etc. pp. 44. London, 1862. Price 1 shilling sterling.—This report has but just reached us, and much that it contains has already been republished in this country. The subjects are treated of in the following order:—1. Geological and topographical maps and models, and general collections.—2. Non-metallic mineral substances, coal excepted.—3. Working of mines.—4. Coal and other mineral fuels.—5. Iron.—6. Metals other than iron. Besides being a record of what was contained in Class I, it contains many valuable notes and statistics added by its able reporter, whose position as professor of mining in the Royal School of Mines, and experience as Inspector of Mines to the Crown and to the Duchy of Cornwall, gave him peculiar facilities for obtaining accurate information in regard to these subjects. G. J. B.

III. GEOLOGY.

1. *Second Annual Report upon the Natural History and Geology of the State of Maine*, 1862. 448 pp. 8vo. 1863.—This volume contains a Report on the Fishes of Maine, by Dr. Ezekiel Holmes; Notes upon certain Mammals of the State, by J. G. Rich; List of Reptiles and Amphibians, by Dr. B. F. Fogg; General observations on Insects, by A. S. Packard, Jr.; Report on the Geology of Maine, by C. H. Hitchcock.

The Geological Report of Prof. Hitchcock occupies 430 pages of the volume. It describes, first, the southern portion of the state, where the rocks are mostly metamorphic, and include limestones as well as granite, syenite, gneiss, mica schist, quartzite and other rocks. Portions of the region are designated Azoic and Taconic; but facts thus far ascertained do not fix the age of the rocks. After presenting much valuable information on this part of the State, it passes to the Schoodic region, or that of the Schoodic Lakes, near the border of New Brunswick, and the northern and unsettled portions of the State. The existence of fossils in New Hampshire near Umbagog Lake is announced; and it is stated that the rock is probably a westward extension of the Oriskany sandstone belt of Maine. Near Moosehead Lake, a mica schist is stated to have afforded a specimen of the *Favosites Gothlandica?* On Farm Island, sandstone occurs containing ripple marks, and the *Fucoides Cauda-Galli.* Descriptions of other fossiliferous regions in this northern portion of the State are given,—the rocks of which are of the age of the Lower Helderberg, Oriskany sandstone, and Cauda-Galli grit—together with lists of fossils.

On p. 402, it is stated that Dr. Dawson has identified 6 more new species of Devonian plants from Perry, Maine; making the whole number of described North American species of terrestrial Devonian plants 75; and in addition, there is material on hand for still increasing this number. The deposits are supposod to "lie between the Chemung and Hamilton groups." It is mentioned also that the Devonian of New Brunswick has afforded Mr. Hartt the wings of insects.

The Report also contains new facts on the Post-tertiary glacial scratches and other phenomena.—We cite the following on Glacial action about Penobscot Bay, from an account at p. 382, by J. De Laski.

"*Ancient Glacial Action in the Southern part of Maine.*

To Mr. George L. Goodale—*Dear Sir:*—I herewith comply with your request to furnish for the ensuing Report of the Scientific Survey of the State of Maine, an account of my examination of the boulder evidence of the Penobscot bay.

From careful personal examination of the surface of the islands and borders of the great fiord of the southern coast of Maine, I have been forced to the conclusion that a glacier once occupied that margin of the state, of a magnitude sufficient to cover the highest hills of the region, and to extend far into the interior towards the north. From a glance at the correct county maps of the locality, we observe that the general trend of the islands, headlands, streams, lakes, harbors, creeks, coves, &c., is north–south, suggesting some law of formation. In these directions, I make no allowance for magnetic variation, which is considerable in the Penobscot bay. There are, indeed, departures from this rule as in the east–west direction of the great thoroughfare separating the Fox islands, where the natural boundary between the two towns was set up at an infinitely earlier period than that of the boulder age; for the irruption of the trap of North Haven broke through the granite and Taconic slates in a line corresponding to this trend.

We also find the hills not rounded and rough, but having an elongated appearance, and a trend also north–south, as if their sides had been subjected to a gigantic system of sculpturing, on the design that these, too, should be directed towards the south. And furthermore, these hills, even where they attain an elevation of one and two thousand feet, as those of Camden and Mount Desert, present gradual slopes to the north and bold fronts to the south; and, if of granite, they are broken down more or less into step-like precipices of east–west parallels, the debris of which has not been accumulated into taluses, but has been transported south a little distance, often more and more comminuted as we advance.

The formation of the coast is syenitic granite, bordered here and there with a margin of trap or of Taconic slates, highly altered in places, and often converted into cherty flints as on Isle au Haut—and furnishes, from the general barrenness of the surface, a good opportunity to study the boulder phenomena. And this surface is everywhere ridged into furrows, often very deep and in the usual direction of the valleys, &c., and presents the finest examples of embossed rocks as described by Charles H. Hitchcock in his Elements of Geology. This is so remarkably the case that one might, in the foggiest weather, easily point out north, south, &c., by looking at these rocks; for they represent in minature, the hills and mountains of the coast as I have described them. Transverse indentations are everywhere common—*lunoid furrows*, I have called them—from an inch in length to four and five feet, having their horns pointing towards the northeast and northwest, and their steep walls facing the south. *These furrows, in all cases, are sufficient to tell the cardinal points of the compass as one passes along over them.*

Everywhere, too, the boulder striæ may be found on the south sides of these hills at their bases, and on their sides when dipping at large or small angles towards the east or west, in as finely developed examples as are found on their northern slopes. It is a fact beyond controversy, that the boulder phenomena in the Penobscot bay are *sui generis* in character, and owe their existence to one agent and the same period.

I have found these boulder striæ four hundred feet high on the side of Isle au Haut hill—which is five hundred feet above the sea—and on the southern brow of Megunticook, overlooking a precipice of two or three hundred feet, and twelve hundred feet above Camden harbor. Mount Battie, south of that mountain, the nearest to the village of any of those hills, and composed of quartzoze conglomerate, is everywhere scored and scratched, and has a very abrupt southern face. Vast masses of rock have been torn from it in this direction, and lie around its base. One large boulder here, about forty feet long, must weigh not less than six hundred tons.

There is a series of terraces in Vinalhaven, as you remember, seven hundred yards long, rising one above another, the last wall of which forms the highest margin of a dell running nearly due north–south unbroken for four hundred yards, and from twenty to thirty feet deep, and fifty yards wide. This is a trough cut out of the solid granite—a gigantic and splendid specimen of Nature's sculpturing with her rude stone chisels—all she needed in those days, when she had a vast duration of time before her to prepare a barren country with fruitful soils for the prospective worker, man. Towards the northern extremity of this rim, which is one hundred and fifty feet above the sea, there stands a high rock overlooking the village, apparently in its native bed, presenting a vertical wall towards the south twenty feet high above the soil, and twenty-four broad. No blasting by art, however carefully conducted, could perform a better operation. If this rock be a boulder, as you and I doubted, it must weigh upwards of a thousand tons. But many thousand tons from the south of it are utterly removed. Going a little further north, we reach one of the highest hills in the town, of granite, two hundred and fifty feet. To the north, we look away down upon a tide "river," now a mile long; but once three, before the land obtained its present height; and earlier still, very much longer. Looking around towards the east and south, we glance over a spacious salt meadow, a densely wooded valley, and a large salt-water pond. This depression must have been cut out of a comparatively level crust. From continued examination of the subject during the last few years, I have seen nothing to induce me to believe that the granite had been materially changed from a horizontal position before the boulder period, as those north-south depressions might suggest. But what was really the depth of the denundation, can only be vaguely conjectured; but I have no doubt that it has been many hundred feet.

The island of Mt. Desert exhibits the boulder phenomena in a more wonderful degree than those places I have mentioned. I presume you have thoroughly explored the locality. You see the southern brows of those lofty granitic hills everywhere crushed and broken into fearful precipices; whereas the sides turned to the north present plains of greater breadth, and dip at much smaller angles down towards the level country beyond. The great granitic boulders lie at their southern foot; and those specifically the same but of less magnitude, and transported the farthest off, are more worn and rounded. We have here, as elsewhere in the Penobscot bay, the evidence that it was the special business of the great denuding agent to cover the barren surface with soils, and that those soils are the result of local detritus—gravels, clays and sands crushed and ground from the detached rocks.

On the Taconic slates beyond these mountains, towards Ellsworth, we have the debris of the Taconic formation. Still beyond, through Dedham, we have a granitic formation, and the granitic boulders are in most wonderful profusion and of great magnitude. They were derived from the hills a little way toward the north. The same peculiarity may be said of North Haven above Vinalhaven. On that island, principally a trap region, you see trap boulders and rubbish. In the northern part of Vinalhaven where the Taconic slates are highly altered, you see boulders of the same character; on the granite below, granitic rocks; and still farther beyond, where the syenite has apparently been altered, the ruins of hornblendic rocks are found.

Around one of the quarries to the west of Carver's harbor, the ground is literally covered with boulders, some of which are enormous. After repeated attempts, I could not make out more than five per cent of foreign rocks among them. Many of these, turned out of their beds, exhibit the polishing and scratching of the common floor-rock of the island. Furthermore, if carefully turned over, we find some of them left just where they had last been employed in scratching the ledges, the parallel scratches of the boulder being placed parallel to those of the rock beneath. Of these foreign boulders we often have little or no evidence as to their origin. We have specimens of red and blue granite, trap, gneiss, mica schists, clay slates, and fossiliferous sandstones from the Katahdin region. We can well suppose them to have been dispersed by icebergs, or borne as freight to these localities by slowly moving glaciers. * * * * *

My conclusions, therefore, from the facts which I have enumerated, are, that a glacier once filled the basin between the Camden hills on the west, and those of Mount Desert on the east, forty miles wide—extended to a great distance north, involving several hills beside those mentioned of a thousand feet high, and certainly not less than three thousand feet thick.

Very truly yours,

December, 1862. JOHN DELASKI.

2. *Fossil Crustaceans from the Coal Measures and Devonian Rocks of British America;* by J. W. Salter, (Q. J. Geol. Soc., xix, 75, and plate.) —The specimens described were furnished Mr. Salter by Dr. J. W. Dawson. The Devonian species are from St. Johns, New Brunswick; one is a small *Eurypterus;* for the other, of undetermined relations (but supposed to be possibly related to the Squilla group), the new genus *Amphipeltes* is instituted. The Carboniferous specimens are from the Joggins, Nova Scotia, and belong to two species, one a *Eurypterus*, the other regarded (and apparently with good reason) an *Amphipod*, and named *Diplostylus Dawsoni.*

3. *On the Cambrian and Huronian Formations;* by J. J. Bigsby, (Q. J. Geol. Soc., xix, 36).—The author reviews in Part I. the facts with regard to the Cambrian, and, in Part II, those respecting the Huronian formation of Canada; and he concludes that the Huronian is not Cambrian, but more closely related to the Laurentian or Azoic.

4. *On the Lower Carboniferous Brachiopods of Nova Scotia;* by Thomas Davidson, (Q. J. Geol. Soc., xix, 158).—Besides remarks on the species under consideration, Mr. Davidson gives his opinion, as follows, with regard to the Paleozoic relations of the Permian. He says, that "Although there exist in the Permian formation some new forms which may recall to mind some which existed in the Mesozoic period, it must be allowed that that number forms the minority, and that, on the contrary, the great bulk of the Permian species, *to whatever class they may belong*, bears the most positive Paleozoic stamp, and that the species are in many cases the same that lived in the Carboniferous era, and some even in the Devonian."

5. *On fossil Estheriæ, and their distribution;* by T. Rupert Jones, (Q. J. Geol. Soc., xix, 140).—The Ostracoid Crustaceans, called *Estheriæ* (formerly referred in part to the Molluscan genus *Posidonia*), are the subject, in this important paper, of brief general remarks, and several of the species of special criticism or elucidation. The species range from the Devonian, through the Carboniferous, Permian, and Triassic, to the Oolite and Wealden, and one species is queried as to its being Tertiary. They are supposed to be all either fresh-water or brackish-water. The *Estheriæ* of the Triassic beds of Pennsylvania, Virginia, and North Carolina are all referred by Mr. Jones to one species, for which he adopts Lea's name *E. ovata.* The *E. ovalis* of Emmons was published in the same year, but apparently in a later month of the year.

The fossil shells from the Carboniferous beds at Pottsville in Pennsylvania, named by Lea *Cypricardia Leidyi*, Mr. Jones regards as a new genus of Ostracoids, and names it *Leaia*, giving the species the name *Leaia Leidyi.* It is closely allied to a species from the Lower Carboniferous of Fifeshire, England.

6. *On a new Labyrinthodont Reptile, Anthracosaurus Russelli, from the Lanarkshire Coal-field;* by T. H. Huxley, (Q. J. Geol. Soc., xix, 56). —The fossil is a part of a skull, measuring 15 inches in length and nearly 12 in breadth. Besides this, there are vertebral bodies and a rib which probably belong to the *Anthracosaurus.* Professor Huxley regards the species as related to the Triassic *Mastodonsaurus.* He observes that the vertebræ closely resemble in section the vertebræ of the *Eosaurus* (from

the Joggins) described by Mr. Marsh; and he suggests that the *Eosaurian* vertebræ may have belonged to a Labyrinthodont, or to a species between a Labyrinthodont and an Ichthyosaurian. The best preserved rib is 6½ inches long and half an inch broad.

The two types of species which have been called Labyrinthodonts are that of the ARCHEGOSAURS [Ganocephala of Owen, but true *Ganoids* according to Agassiz], most abundant in the Carboniferous; and that of the MASTODONSAURS (genera, *Mastodonsaurus, Labyrinthodon, Capitosaurus, Trematosaurus*), common in the Triassic. The *Archegosaurs* had, as von Meyer has proved, a *persistent branchial apparatus.* Nothing is known as to whether this was true or not of the *Mastodonsaurs.* With respect to the *Denderpeton* and *Hylonomus* of Nova Scotia, discovered by Dawson, and recently referred to the *Archegosaur* group by Owen, and with regard also to the *Raniceps* of Wyman and the *Hylerpeton* of Owen, Professor Huxley remarks that it is not yet safe to decide whether their affinities are Archegosaurian or Mastodonsaurian.

7. *Anniversary Address before the Geological Society of London*, Feb. 20, 1863, by Prof. A. C. RAMSAY, President of the Society. 26 pp. 8vo. —At the annual meeting, before the Address of the President, the Wollaston Medal was awarded to Gustav Bischof of Bonn, and the Wollaston Donation-fund to Prof. Sneft. Prof. Ramsay, after brief notices of members deceased during the year—Richard Trench, Dr. C. C. v. Leonhard, Robert Raid, Rev. James Cumming, J. C. Nesbit, Dr. H. G. Bronn, B. de Doue, Dr. T. S. Trail and Marquis of Breadalbane,—takes up the topic of his discourse—Breaks in the succession of the British Paleozoic strata.

8. *On the production of crystalline limestone by heat.*—In this *Journal*, vol. xxxii, p. 112, an abstract is given of Rose's experiments on the deportment of carbonate of lime at a high temperature. Among other interesting conclusions drawn by Rose, he says that "chalk or compact limestone cannot be converted into crystalline limestone (or calc-spar) by exposure to a high temperature in closed vessels, and, as a general fact, that rhombohedral carbonate of lime is not formed in the dry way." Further, "that the so-called crystalline marble, obtained by Sir James Hall in his experiments, was probably nothing more than a slightly coherent but otherwise unaltered mass, which Hall erroneously considered to be crystalline marble."

Rose states, in a recent communication to the Berlin Academy of Sciences, that he was not entirely satisfied with his former results, especially as Dr. Horner, President of the Geological Society of London, assured him that he had inspected the specimen of marble made by Sir James Hall, and that it differed entirely from the amorphous product obtained in the Berlin experiments. Rose, therefore, repeated his investigations on the subject, and has now obtained results which differ entirely from those he formerly published, and which fully confirm the correctness of Sir James Hall's conclusion, that marble can be produced by exposing massive carbonate of lime to a high temperature under great pressure. The experiments were made with aragonite from Bilin, in Bohemia, and with lithographic limestone. In one case, the mineral was heated in a wrought-iron cylinder, and in the other, in a porcelain bottle, special precautions being taken to exclude the air, and make the vessels as near air-tight as possible. These were exposed to a white heat for half an hour, and, on cooling, both the aragonite and the lithographic limestone were found to be converted into crystalline limestone, the former very much resembling Carrara marble, and the latter a grayish-white granular limestone. The change took place without any material decomposition, the resulting marble containing a trifle less carbonic acid than the lithographic limestone from which it was produced.—*Pogg. Ann.*, cxviii, 565. G. J. B.

9. *On the Flora of the Devonian Period in Northeastern America: Appendix;* by J. W. DAWSON, LL.D., F.G.S., Principal of McGill University, Montreal. (Q. J. Geol. Soc., 1863. Read Dec. 17, 1862).—In a recent visit to Perry, the author (with the aid of Mr. Brown, of that place) thoroughly examined the present exposure of the plant-bearing bed. Among the specimens obtained were the following. (1.) Wood of a Conifer of the genus *Dadoxylon.* (2.) A new *Stigmaria* of the type of *S. exigua.* (3.) Specimens of *Lepidostrobus Richardsoni*, showing it to have been the fructification of a new and interesting species of *Lycopodites.* (4.) Another species of *Lycopodites* allied to *L. Erdmanni* Germar. (5.) A new species probably of the genus *Anarthrocanna* Gœppert. (6.) A new *Cordaites.* (7.) More perfect specimens of *Cyclopteris Browniana*, showing it to have been a large and beautiful flabellate leaf or frond, possibly identical with that from the Upper Devonian of Pennsylvania, figured by Prof. Rogers, in his *Pennsylvania Report*, vol. ii, part 2, pl. 22.[1] (8.) A Fern allied to *Cyclopteris Jacksoni*, but with a stem similar to that of *C. Roemeriana* Gœppert. (9.) New species of *Sphenopteris*, *Trichomanites* and *Carpolites.* (10.) Specimens of *Leptophlœum rhombicum*, showing its leaves and fructification. These, with some interesting specimens recently collected by Mr. R. Bell, of the Geological Survey, at Gaspé, Dr. Dawson hopes to describe in a future paper.

IV. BOTANY AND ZOOLOGY.

1. *Dimorphism in the Flowers of Linum.*—Referring back to our brief note upon the subject of dimorphous flowers in this *Journal* for Nov., 1862, and more particularly to Mr. Darwin's remarkable paper on the two sexual forms in *Primula* (in *Jour. of Linnæan Society*, no. 22), we wish now to call attention to some still more curious observations and experiments of Mr. Darwin, which were read to the Linnæan Society in February last, and are just published in the 26th no. of its *Journal.* The paper is entitled: "On the Existence of two forms, and on their reciprocal Sexual Relations, in several species of the genus *Linum.*" The principal case is that of the crimson *Linum grandiflorum*, which is now common in gardens, and, as it flowers the whole summer long, is freely offered to the inspection of the curious. Dimorphic genitalia had hardly been noticed in the genus *Linum.* In the common cultivated Flax, it seems not to occur; but Planchon, in his Monograph, published 15 years ago, had noticed two or even three different states as to the relative length of the stamens and styles in *L. perenne* and *L. salsoloides*, and had even conjectured that this dimorphism might have some influence on the man-

[1] A portion of this figure is reproduced in *Dana's Manual of Geology*, in fig. 984, p. 750.

ner of fertilization. But this had been wholly overlooked "in such common garden-flowers as *L. grandiflorum* and *L. flavum*," until Mr. Darwin detected it, and worked out the case to the striking results which we record below, chiefly in his own words.

"The crimson *Linum grandiflorum* presents two forms, occuring in about equal numbers, which differ little in structure, but greatly in function. The foliage, corolla, stamens, and pollen (examined dry and distended with water) are alike in both forms. The difference is confined to the pistil: in the one form, which I will call "short-styled," the column formed by the united styles and the short stigmas together is about half the length of the whole pistil in the other and "long-styled" form. A more important distinction is, that the five stigmas in the short-styled form diverge greatly from each other, and pass out between the filaments of the stamens, and thus lie within the tube of the corolla. In the long-styled form the elongated stigmas stand nearly upright and alternate with the anthers. In this latter form, the length of the stigmas varies considerably, their upper extremities projecting even a little above the anthers, or reaching up only to about their middle. Nevertheless, there is never the slightest difficulty in distinguishing between the two forms; for, besides the difference in divergence, the stigmas of the short-styled form never reach even to the bases of the anthers. In the short-styled, the papillæ on the stigmatic surfaces are shorter, darker-colored, and more crowded together than in the long-styled form: but these differences seem due merely to the shortening of the stigma; for, in the varieties of the long-styled form with shorter stigmas, the papillæ are more crowded and darker-colored than in those with the longer stigmas. Considering the slight and variable differences between the two forms of this *Linum*, it is not surprising that they have been hitherto overlooked.

"In 1861, I had eleven plants growing in my garden, eight of which were long-styled, and only three short-styled. Two very fine long-styled plants grew in a bed a hundred yards off, and separated from the others by a screen of evergreens. I marked twelve flowers, and put on their stigmas a little pollen from the short-styled plants. The pollen of the two forms is, as stated, identical in appearance; the stigmas of the long-styled flowers were already thickly covered with their own pollen,—so thickly that I could not find one bare stigma; and it was late in the season, namely, September 16th. Altogether, to expect any result from this trial seemed almost childish. From my experiments, however, on *Primula*, which have been laid before this Society (*Journal*, vi. 77), I had faith, and did not hesitate to make the trial, but certainly I did not anticipate the full result. The germens of these twelve flowers all swelled, and ultimately six fine capsules (the seed of which germinated this year) and two poor capsules were produced; only four capsules shanked off. These two plants produced, before and after and at the time of the trial, a vast number of flowers, but the germens of not even one swelled. All these flowers, though their stigmas were so densely covered with their own pollen, were absolutely barren.

"The nine other plants, six long-styled and three short-styled, grew in the beds of the same flower-garden. Four of the long-styled produced no seed-capsules; one produced two; but the remaining long-styled plant grew so close to a short-styled plant that their branches touched; and this produced twelve capsules, but they were poor. The case was different with the short-styled plants. The plant which grew in juxtaposition with the long-styled plant produced ninety-four imperfectly fertilized capsules, containing a multitude of bad seeds, with a moderate number of good seeds The two other short-styled plants grew in a single clump, and were very small, being partly smothered by other plants; they did not stand very close to any long-styled plants, yet they yielded together nineteen capsules. These facts seem to show that the short-styled plants are far more fertile with their own pollen than the long-styled. We shall immediately see that this is the case in a slight degree. But I suspect that in this instance the difference in fertility between the two forms was in part due to a distinct cause. I repeatedly watched the flowers, and only once saw a humble-bee momentarily alight on one, and then fly away, as if it were not to its taste. If bees had visited the several plants, there cannot be a doubt that the four long-styled plants which did not produce a single capsule would have borne an abundance. But several times I saw small Diptera sucking the flowers; and these insects, though not visiting the flowers with anything like the regularity of bees, would carry a little pollen from one form to the other, especially when growing close together; and the stigmas of the short-styled plants, diverging within the tube of the corolla, would be more likely than the upright stigmas of the long-styled to receive a small quantity of pollen when brought by small insects. From the much greater number of long-styled than of short-styled flowers in the garden, evidently the short-styled would be more likely to receive some pollen from the long-styled, than the long-styled from the short-styled.

"In 1862, I raised thirty-four plants of this *Linum* in a hotbed; and these consisted of seventeen long-styled and seventeen short-styled forms. Seed sown later in the flower-garden yielded seventeen long-styled and twelve short-styled forms. These facts justify the statement that the two forms are produced in about equal numbers. The first thirty-four plants were kept under a net which excluded insects. I fertilized heteromorphically fourteen long-styled flowers with pollen from the short-styled, and got eleven fine seed-capsules; these contained on an average 8·6 seeds per capsule, but only 5·6 were apparently good. It may be well to state that ten seeds is the maximum possible production for a capsule, and that our climate cannot be very favorable to this North-African plant. On three occasions, I fertilized homomorphically the stigmas of altogether nearly a hundred flowers (but did not separately mark them) with their own pollen, but taken from separate plants, so as to prevent any possible ill effects from close interbreeding; and many other flowers were produced, which, as before stated, would get plenty of their own individual pollen; yet from all these flowers, borne by the seventeen long-styled plants, only three capsules were produced; one of these included no seed, and the other two together gave only five good seeds. Nor do I feel at all sure that this miserable product of the two half-fertile capsules from the seventeen plants, each of which must have produced at least fifty or sixty flowers, is really the result of their fertilization by their own pollen; for I made a great mistake in keeping the two forms under the same net, with their branches often interlocking; and it is surprising that a greater number of flowers were not accidentaly fertilized.

"Of the short-styled flowers, I fertilized heteromorphically twelve with the pollen of the long-styled (and to make sure of the result I previously castrated the majority), and obtained seven fine seed-capsules. These included an average of 7·6 seeds, but of apparently good seed only 4·3 per capsule. At three separate times, I fertilized homomorphically nearly a hundred flowers with their own-form pollen, taken from separate plants; and numerous other flowers were produced, many of which must have received their own pollen. From all these flowers borne on the seventeen plants, only fifteen capsules were produced, of which only eleven contained any good seed, on an average 4·2 per capsule. As remarked in the case of the long-styled plants, some even of these capsules were perhaps the product of a little pollen accidentally fallen from the flowers of the other form. Nevertheless, the short-styled plants seem to be slightly more fertile with their own pollen, in the proportion of fifteen capsules to three, than the long-styled: the real proportional excess in fertility is probably a little greater, as the short-styled flowers, when not disturbed, do not so surely receive their own pollen as do the long-styled. The greater self-fertility of the short-styled flowers was, as we have seen, also shown by the plants left to themselves, and but sparingly visited by insects, in the flower-garden in 1861, and likewise by those raised in 1862."

Next, with the view of ascertaining the immediate cause of this almost absolute sterility of long-styled pistils with their own form of pollen, and in a less degree of short-styled pistils with their own form of pollen, a series of experiments was made, in which pollen of either sort was applied to the stigmas of either sort, and the stigmas were dissected under the microscope, after an interval of 24 hours or less. When pollen of a short-styled flower was applied to the stigmas of a long-styled, and likewise when, conversely, that of a long-styled flower was applied to the stigmas of a short-styled, a microscopical dissection showed that the stigmas were freely penetrated by numerous pollen-tubes. But when homomorphic unions were attempted, no pollen-tubes, or scarcely any, were emitted; even after an interval of three days the stigmas remained straight and fresh-colored, and the pollen inactive. When two or three of the stigmas were dusted with their own form of pollen, and the others with the opposite form, the difference was striking; the former stigmas remaining straight, fresh, and unpenetrated or nearly so, while the latter were soon discolored, twisted, half-shrivelled, and penetrated by a multitude of pollen-tubes.

"This seems to me a remarkable physiological fact. The pollen-grains of the two forms are undistinguishable under the microscope; the stigmas differ only in length, degree of divergence, and in the size, shade of color, and approximation of their papillæ, these latter differences being variable and apparently simply due to the elongation of the stigma. Yet we plainly see that the two pollens and the two stigmas are widely dissimilar in action, the stigmas of each form being almost powerless on their own pollen, but causing, through some mysterious influence, by simple contact (for I could detect no viscid secretion), the pollen-grains of the opposite form to protrude their tubes. It may be said that the two pollens and the two stigmas by some means mutually recognize each other. Taking fertility as the criterion of distinctness, it is no exaggeration to say that the pollen of the long-styled *Linum grandiflorum* (and conversely of the other form) has been differentiated, with respect to the stigmas of all the flowers of the same form, to a degree corresponding with that of distinct species of the same genus, or even of species of distinct genera."

The results are nearly the same in *L. perenne*, except that pollen-tubes were found to be produced in attempted homomorphic unions, but either they did not reach the ovules, or they did not act on them. "Neither pollen when placed on its own stigma causes fertility, except occasionally and in a very moderate degree." The following remarks neatly discriminate between the action of the wind and that of insects in carrying pollen. And then the twisting of the long styles in *L. perenne* and the divergence of the short ones in both species are noteworthy:

"Botanists, in speaking of the fertilization of plants or of the production of hybrids, often refer to the wind or to insects as if the alternative were indifferent. This view, according to my experience, is entirely erroneous. When the wind is the agent in carrying pollen, either from one separated sex to the other, or from hermaphrodite to hermaphrodite (which latter case seems to be almost equally important for the ultimate welfare of the species, though occurring perhaps only at long intervals of time), we can recognize structure as manifestly adapted to the action of the wind as to that of insects when they are the carriers. We see adaptation to the wind in the incoherence of the pollen, in the inordinate quantity produced (as in the Coniferæ, Spinage, &c.), in the dangling anthers, well fitted to shake out the pollen, in the absence or small size of the perianth, or in the protrusion of the stigmas at the period of fertilization, in the flowers being produced before they are hidden by the leaves, in the stigmas being downy or plumose (as in the Gramineæ, Docks, and other plants) so as to secure the chance-blown grains. In plants which are fertilized by the wind, the flowers do not secrete nectar, their pollen is too incoherent to be easily collected by insects, they have not bright-colored corollas to serve as guides, and they are not, as far as I have seen, visited by insects. When insects are the agents of fertilization (and this is incomparably the more frequent case, both with plants having separated sexes and with hermaphrodites), the wind plays no part, but we see an endless number of adaptations to ensure the safe transport of the pollen by the living workers. We can recognize these adaptations most easily in irregular flowers; but they do not the less occur in perfectly regular flowers, of which those of *Linum* offer an instance, as I will almost immediately endeavor to show.

"I have already alluded to the rotation of each separate stigma in the long-styled form alone of *Linum perenne.* In the other species examined by me, and in both forms when the species are dimorphic, the stigmatic surfaces face the centre of the flower, and the furrowed backs of the stigmas, to which the styles are attached, face the circumference. This is the case, in the bud, with the stigmas of the long-styled flowers of *L. perenne.* But, by the time the flower in this form has expanded, the five stigmas, by the torsion of that part of the style which lies beneath the stigma, twist round and face the circumference. I should state that the five stigmas do not always perfectly turn round, two or three often facing only obliquely towards the circumference. My observations were made during October; and it is not improbable that earlier in the season the torsion would have been more perfect; for after two or three cold and wet days the movement was very incomplete. The flowers should be examined shortly after their expansion; for their duration is brief, and, as soon as they begin to wither, the styles become spirally twisted together, and the original position of the parts is lost.

"He who will compare the structure of the whole flower in both forms of *L. perenne* and *grandiflorum*, and, I may add, of *L. flavum*, will, I think, entertain no doubt about the meaning of this torsion of the styles in the one form alone of *L. perenne*, as well as the meaning of the divergence of the stigmas in the short-styled forms of all three species. It is absolutely necessary, as we now know, that insects should reciprocally carry pollen from the flowers of the one form to those of the other. Insects are attracted by five drops of nectar, secreted exteriorly at the base of the stamens, so that to reach these drops they must insert their proboscides outside the ring of broad filaments, between them and the petals. In the short-styled form of the above three species, the stigmas face the axis of the flower; and had the styles retained their original upright and central position, not only would the stigmas have presented their backs to insects as they sucked the flowers, but they would have been separated from them by the ring of broad filaments, and could never have been fertilized. As it is, the styles diverge greatly and pass out between the filaments. The stigmas, being short, lie within the tube of the corolla; and their papillous faces, after the divergence of the styles, being turned upwards, are necessarily brushed by every entering insect, and thus receive the required pollen.

"In the long-styled form of *L. grandiflorum*, the parallel anthers and stigmas, slightly diverging from the axis of the flower, project only a little above the tube of the somewhat concave corolla; and they stand directly over the open space leading to the drops of nectar. Consequently, when insects visit the flowers of either form (for the stamens in this species occupy the same position in both forms), they will get their proboscides well dusted with the coherent pollen. As soon as the insect inserts its proboscis to a little depth into the flower of the long-styled form, it will necessarily leave pollen on the faces and margins of the long stigmas; and as soon as the insect inserts its proboscis to a rather greater depth into the short-styled flowers, it will leave pollen on their upturned stigmatic surfaces. Thus the stigmas of both forms will indifferently receive the pollen of both forms; but we know that the pollen of the opposite form alone will produce any effect and cause fertilization.

"In the case of *L. perenne*, affairs are arranged a little more perfectly; for the stamens in the two forms stand at different heights, and pollen will adhere to different parts of an insect's body, and will generally be brushed off by the stigmas of corresponding height, to which stigmas each kind of pollen is adapted. In this species, the corolla is flatter, and in the one form the stigmas and in the other form the anthers stand at some height above the mouth of the corolla. These longer stigmas and longer stamens do not diverge greatly; hence insects, especially rather small ones, will not insert their proboscides between the stigmas or between the anthers, but will strike against them, at nearly right angles, with the back of their head or thorax. Now, in the long-styled flowers of *L. perenne*, if each stigma had not rotated on its axis, insects in visiting them would have struck their heads against the backs of the stigmas; as it is, they strike against the papillous fronts of the stigmas, and, their heads being already charged with the proper coherent pollen from the stamens of corresponding height borne by the flowers of the other form, fertilization is perfectly effected. Thus we can understand the meaning of the torsion of the styles in the long-styled flowers alone, as well as their divergence in the short-styled flowers."

Linum Lewisii is inferred to be distinct as a species from *L. perenne* on the ground of Planchon's remark, that the styles are in the same specimen sometimes equalling, sometimes shorter, and sometimes longer than the stamens. The inference may not be correct: and although the plant is said to extend northward even to the shores of the Arctic sea, it does not specially belong to the "Arctic Zone," but abounds on the Western plains.

A remark in this interesting paper lets us know that Mr. Darwin has detected *trimorphism*, viz: three distinct forms, each of which produces two kinds of pollen, in *Lythrum Salicaria*, neither pollen when placed on its own stigma producing fertility, "except occasionally and in a very moderate degree; yet the pollen-tubes in each case freely penetrate the stigmatic tissue." Here the number of heteromorphic and therefore fertile unions possible is largely increased. These degrees of sterility in homomorphic unions,—from complete inertness of the pollen to the occasional production of an inefficient pollen-tube, to the copious production of inefficient pollen-tubes penetrating the style, and to the occasional fertilization of an ovule,—are very noteworthy, and Mr. Darwin will some day turn them to account in his own way. "*Natura non agit saltatim.*" Let us add, in conclusion, that when such fine biological discoveries are so readily made by the study of some of the commonest plants, no botanical student, however restricted his range, need slumber for lack of occupation, nor suppose that the field is exhausted. Out of old fields, indeed, not only comes all this new corn from year to year, but such gleanings as these are richer far in interest than any crop of new species from a virgin soil. A. G.

2. *Variation and Mimetic Analogy in Lepidoptera.*—Mr. Bates (whose interesting book of travels, *The Naturalist on the River Amazon*, is exciting much attention in England, and which we trust will be reprinted here) has contributed an elaborate paper to the *Transactions of the Linnæan Society*, vol. xxiii (1862), entitled *Contributions to the Insect Fauna of the Amazon Valley, Lepidoptera, Heliconidæ.* The materials were gathered by the author during eleven years of travel and research in the Amazon region. The introduction to this paper treats, among other subjects, most largely of the highly curious one above referred to, i. e., the extraordinary mimetic resemblance which certain butterflies present to other butterflies belonging to distinct groups. There are also collections of pregnant facts upon variation and divergence into races. We had marked many pages for extract; but room has not been found for them. It seems less needful to copy large parts of Mr. Bates' narrative now, since a good abstract of his paper has recently appeared in the *Natural History Review.* The bearing of Mr. Bates' observations may be inferred from the remark of his reviewer: "it is hardly an exaggeration to say, that, whilst reading and reflecting on the various facts given in this memoir, we feel ourselves to be as near witnesses as we can ever hope to be of the creation of a new species on this earth." The two subjects, *variation* and *simulation*, as may be inferred, are considered in respect to their bearing upon Mr. Darwin's theory, of which Mr. Bates is a zealous upholder, although his observations were made before this celebrated theory was promulgated. The facts set forth about variation appear excellently to illustrate the formation of races and nearly related species through gradual divellent variation; and those on mimetic analogy are not only wonderfully curious, but are most ingeniously applied to illustrate the doctrine of natural selection, under a peculiar phase.

We will first notice some of the reported facts about *variation.* Such an amount and such gradations of variability as Mr. Bates reports of butterflies, we have ceased to think very extraordinary in the vegetable world; yet we had been led to suppose that forms in the animal world were everywhere more definite and fixed. But Mr. Bates' observations seem to have convinced him "that there is a perfect gradation in variability, from butterflies of which hardly two can be found alike, to slight varieties, to well marked races, to races that can hardly be distinguished from species, to true and good species." In the genus *Ceratinia*, for instance, those parts of structure [i. e. the veining of the wings] which form fixed generic characters in other groups are here variable in the sexes, and in individuals of the same sex. *C. Ninonia* "evidently varies in different ways in different localities; yet the local varieties are not definite, the segregation of the races is not complete: so that it is embarrassing to decide whether to treat the form as one polymorphic species, including the variations under one and the same definition, or to describe separately the type and the local varieties. Besides these incomplete local modifications, easily traceable to the type, there are, as often happens in the case of prolific, widely distributed, and variable species, a number of other forms rather more strongly marked and better defined, which inhabit regions rather more distant from the locality of the type than those which the mere varieties inhabit. These are admitted on all hands to be distinct species; but I think it would be difficult to prove that these were not also varieties of *C. Ninonia*, which have become more completely segregated from the parent form." The examples are given. This is essentially what De Candolle concludes of Oaks, as we have seen in a former article. *Mechanitis Polymnia* affords one of the most striking cases. The typical form of the perfect insect, as figured by Cramer, prevails at Para and elsewhere in the region of the lower Amazon. There all the specimens are very much alike: while at Ega, on the Upper Amazon, very few individuals conform to the Cramerian type. Among the numerous forms, one, which he names *M. Egaënsis*, predominates; but all the intermediate forms between it and the typical *M. Polymnia* occur there, only in fewer numbers. At St. Paulo, 260 miles further west, the species was again extremely variable, some individuals coming near the type, but none identical with it. The varieties were quite different from those of Ega; the *M. Egaënsis* was wholly absent, but a new variety abounded, of which there was no trace at Ega; this has been figured and described as a distinct species. The complete set of connecting forms convinced the observer that all belonged to one species, disseminated over a large area, and modified in certain districts. He affirms that the varieties were of such a nature, as to form and colors, that they could not be thought to be hybrids between two or more distinct species. And also, that the amount of local modification in no way accorded with obvious differences in the local conditions; for the species was, on the one hand, totally changed within 260 miles of very similar soil and climate, but, on the other hand, was constant in districts 600 miles apart and very different in physical conditions. Extending the view up to the eastern slopes of the Andes, there are said to be still other forms, some of them clearly varieties of *M. Polymnia*, although they have been described as species; others more sharply defined and having the appearance of true species. So Mr. Bates thinks that,—

"The conclusion is unavoidable that these apparently distinct species are modifications, as well as the undoubted varieties are; for we have the species in all stages of modification,—simple variation, local variety scarcely distinguishable from a mere variation, complete local variety, and well marked race or species. The forms of *M. Polymnia* found in South Brazil confirm this view. At Rio Janeiro the well marked race or species *M. Lysimnia* alone is found; at Bahia (traveling towards the home of the type, *M. Polymnia*), *M. Lysimnia* in company with *M. Nesœa;* at Pernambuco (further northward) *M. Nesœa* alone occurs; at Pará this form is seen no more, and *M. Polymnia* in its typical dress monopolizes the field. These facts seem to teach us that, in this and similar cases, a new species originates in a local variety, where the conditions are more favorable to it than to the typical form, and that a large number of such are simultaneously in progress of formation from one variable and widely distributed species. The new species cannot be proved to be established as such, unless it be found in company with a sister form which has had a similar origin, and maintaining itself perfectly distinct from it. Cases of two extreme varieties of a species being thus brought into contact by redistribution or migration, and not amalgamating, will be found to be numerous when the subject is inquired into." . . . . "It is an advantage to a form to have a sphere of life different from its allies; when two sister forms keep themselves distinct in a locality, it is a sign that they have acquired sufficient difference to fill two separate spheres. If they paired together they would soon become one again. Nature may be said to place a premium on diversity; for she thus destroys the incompletely formed race, and preserves the completely formed one. The case of *Mechanitis Polymnia* differs from that of

Leptalis Theonöe in exhibiting the production, generally, of only one local form in a district, instead of many. As far as my observations go, this seems to have been the most frequent course in nature. More than one new race would with difficulty be formed in a limited area, when the individuals live in close neighborhood, except in such cases as our *Leptalis*, where rigid destruction of intermediate forms is going on, thus restricting the choice of mates to the surviving forms; or in such genera as *Ithomia*, where there is no doubt the insects carefully select their exact counterparts in pairing."

In the latter case, where each sort strictly interbreeds, the races once originated would be kept distinct as long as they existed. Mr. Bates asserts that in the *Ithomiæ* and allied genera, "where a number of very closely allied species fly together, they keep themselves perfectly distinct, there are no hybrid forms, and, on observing individuals *in copula*, I almost always found the pair to be precisely the same in color and markings." The exception was in *Mechanitis Polymnia*, above mentioned, "a polymorphic species, whose local varieties are in an imperfect state of segregation." This pairing of exact counterparts would—upon principles which we have particularly explained in this Journal—accelerate the diversification or divergence into races, by enabling each advance of variation to be held. And it would, as Mr. Bates remarks, enable a number of closely allied forms to exist, either together or in contiguous areas, without amalgamating.

In his remarks upon *Mechanitis Polymnia*, as illustrating the course apparently followed by nature in the formation of local species, the author repeats that:—

"We find, in this most instructive case, all the stages of the process, from the commencement of the formation of a local variety (var. *Egaënsis*) to the perfect segregation of one (var. *Lysimnia*) considered by all authors as a true species. In this species, most of the local varieties are connected with their parent form by individuals exhibiting all the shades of variation; and it is on this account only that we know them to be varieties. In the species allied to *Ithomia Flora*, the forms are in a complete state of segregation (with the exception of *I. Illinissa*, which throws light on the rest), and therefore they are considered as species; they are, in fact, perfectly good species, like all other forms considered as such in natural history. It is only by the study of variable species that we can obtain a clue to the explanation of the rest. But such species must be studied in nature, and with strict reference to the geographical relations of their varieties. Many closet naturalists, who receive disconnectedly the different varieties of any group, treat them all as independent species; by such a proceeding, it is no wonder that they have faith in the absolute distinctness and immutability of species."

The *mimetic analogies*, of which many of the *Heliconidæ* are the objects, have been mentioned by modern authors who have written on the group; "but no attempt has been made to describe them fully or to explain them." Mr. Bates exhibits the more striking cases in a tabular view, which gives some idea of the extent to which this imitation prevails, and, of the various tribes of Lepidoptera to which the imitators belong. It is concluded that the *Heliconidæ* are the *imitated*, because they have all the same family aspect, while the imitators or analogous species are dissimilar to their nearest allies,—are perverted, as it were, from the facies of the group to which they severally belong.

"The resemblance is so close that it is only after long practice that the true can be distinguished from the counterfeit when on the wing in their native forests. I was never able to distinguish the *Leptalides* from the species they imitated, although they belong to a family totally different in structure and metamorphosis from the *Heliconidæ*, without examining them closely after capture. They fly in the same parts of the forest, and generally in company with the species they mimic. I have already given an account of the local modifications to which the *Heliconidæ* are subject. It is a most curious circumstance that corresponding races or species of counterfeiting groups accompany these local forms. In some cases I found proof that such species are modified from place to place to suit the peculiar forms of *Heliconidæ* there stationed."

The details in evidence of this are fully explained and illustrated by plates. Nothing can be more curious. The *Ithomiæ* imitated are excessively numerous in individuals; the imitating *Leptalides* are rare, not more than one to a thousand of the other. The latter has not been found in any other district or country than in those inhabited by the *Ithomiæ* which they counterfeit. The resemblance is often carried to minutiæ, such as the color of the antennæ and the spotting of the abdomen. Not only are the *Heliconidæ* thus imitated; some of them are themselves imitators, i. e., they counterfeit each other, species belonging to distinct genera having been confounded, owing to their close resemblance in coloring and marking.

"These imitative resemblances, of which hundreds of instances could be cited, are full of interest, and fill us with the greater astonishment the closer we investigate them; for some show a minute and palpably intentional likeness which is perfectly staggering. I have found that those features of the portrait are most attended to in nature which produce the most effective deception when the insects are seen in nature."

Similar imitations are said to occur in the Old World, in other families of Butterflies and Moths; but no instance is known of a tropical species of one hemisphere counterfeiting a form belonging to the other. Other orders of insects supply such cases in certain families. "Many instances are known where parasitic bees and two-winged flies mimic in dress various industrious or nest-building bees, at whose expense they live in the manner of the Cuckoo. I found on the banks of the Amazon many of these Cuckoo bees and flies, which all wore the livery of working bees peculiar to the country." Mr. Wallace has noticed two similar and equally striking instances of mimicry in birds.

Now, as to the final cause of these mimetic analogies,—

"When we see a species of Moth which frequents flowers in day-time wearing the appearance of a Wasp, we feel compelled to infer that the imitation is intended to protect the otherwise defenseless insect by deceiving insectivorous animals which persecute the moth and avoid the wasp. May not the Heliconide dress serve the same purpose to the *Leptalis*? Is it not probable, seeing the excessive abundance of the one species, and the fewness of the other, that the Heliconide is free from the persecution to which the Leptalis is subjected?" . . . .

"I believe that the specific mimetic analogies exhibited in connection with the *Heliconidæ* are adaptations,—phenomena of precisely the same nature as those in which insects and other beings are assimilated in superficial appearance to the vegetable or inorganic substances on which, or amongst which, they live. The likeness of a Beetle or a Lizard to the bark of the tree on which it crawls cannot be explained as an identical result produced by a common cause acting on the tree and the animal."

A full series of such imitations by insects, both of inanimate and of living objects, is then given. That such imitative resemblances as we are considering are of the same class as these, and subject to the same explanation, is obvious from the fact of one species mimicking an inanimate object, while one of an allied genus imitates an insect of another family. They are all evidently "adaptations having in view the welfare of the creatures that possess them." Every species maintains its hold upon existence only through some endowment enabling it to withstand the various adverse circumstances to which it is exposed; and the means are of endless diversity,—organs of offense, great fecundity, capabilities for wide dispersion, and, among the rest, the adaptive resemblances of a defenseless species to one which enjoys some kind of protection. The multitudinous swarms of slow-flying *Heliconidæ* on the Amazon, apparently defenseless, must enjoy some immunity from the insectivorous animals. Mr. Bates never saw them preyed upon by birds or Dragon-flies, or molested by Lizards when at rest; and their dead bodies set out to dry were rarely attacked by vermin. They all have a peculiar smell. So it is probable that they are unpalatable to insect enemies. "If they owe their flourishing existence to this cause, it would be intelligible why the *Leptalidæ*, whose scanty number of individuals reveals a less protected condition, should be disguised in their dress and thus share their immunity."

This naturally leads to Mr. Bates' explanation of the process by which these mimetic resemblances and other such adaptations are brought about. The admirer of *natural selection* finds here a beautiful application of the principle. Given the *Heliconidæ* as they are, segregated and in course of segregation into variations, varieties, races, and species under conditions of natural selection which are still occult, and supposing (what their flourishing numbers prove) that their taste, odor, or something else, affords a comparative immunity from the attacks of their natural enemies, the existence of their more exposed analogues, in each locality, would seem to depend upon the closeness of their resemblance to the protected *Heliconidæ* of the district, such resemblance being apparently the only means of escaping extermination by insectivorous animals. As the imitated species vary from place to place, so must the imitators if they would retain their hold upon life. And, of all the variations which are constantly arising, only those which do resemble the protected form near enough to deceive the insectivorous enemy, will retain their hold. This is natural selection, the insectivorous animals being the selecting agents; and the operation proceeds to draw out steadily, in certain favorable directions, the suitable variations which arise from generation to generation, as a result of the extermination of those sorts or varieties which are not enough like the protected species to deceive the enemy.

"If a mimetic species varies, some of its varieties must be more and some less faithful imitations of the object mimicked. According therefore to the closeness of its persecution by enemies, who seek the imitator but avoid the imitated, will be its tendency to become an exact counterfeit,—the less perfect degrees of resemblance being, generation after generation, eliminated, and only the others left to propagate their kind." "The fact of one of the forms of *Leptalis Theonoë*, namely *L. Lysinoë*, mimicking an Ega, not an *Ithomia*, but a flourishing species of another quite distinct family (*Stalachtis Duvalii*), shows that the object of the mimetic tendencies of the species is simply disguise, and that, the simple individual differences in that locality being originally in the direction, not of an *Ithomia*, but of another object equally well answering the purpose, selection operated in the direction of that other object." "When the persecution of a variable local form of our *Leptalis* is close or long continued, the indeterminate variations naturally become extinct; nothing then remains in that locality but the one exact counterfeit, whose exactness, it must be added, is henceforth kept up to the mark by the insect pairing necessarily with its exact counterpart, or breeding in and in. This is the condition of *L. Theonoë*, &c. . . . . When (as happens at St. Paulo, where a greater number of individuals and species, both of *Ithomia* and *Leptalis*, exists . . . .) many species have been in the course of formation out of the varieties of one only, occasional intercrossing may have taken place; this would retard the process of segregation of the species, and, in fact, aid in producing the state of things (varieties and half-formed species) which I have already described as there existing." "Such, I conceive, is the only way in which the origin of mimetic species can be explained. I believe the case offers a most beautiful proof of the truth of the theory of natural selection. It also shows that a new adaptation, or the formation of a new species, is not effected by great and sudden change, but by numerous small steps of variation and selection."

At a time like the present, when the notion that species are derivative, somehow or other, is received as the most probable opinion by such an increasing number of competent observers and thinkers—including, it may be added, the names of Lyell and of Owen,—and when it appears to the thoroughly conservative and well-informed President of the Linnæan Society[1] "that the tide of opinion among philosophical naturalists is setting fast in favor of Mr. Darwin's hypothesis," such illustrations of the latter as Mr. Bates has presented are worthy of attentive consideration. But we need not agree with Mr. Bates in his conclusion that the impression produced "of there being some innate principle in species which causes an advance in organization in a special direction," so that the result is "a predestined goal," is untenable, and the appearances which suggest such idea, illusory. Because variations are picked out, preserved, and led to useful ends by natural selection, it does not follow, nor has it ever been shown, that they occur lawlessly and at random.

A. G.

3. *Flora Australiensis: a Description of the Plants of the Australian Territory*; by GEORGE BENTHAM, F.R.S., P.L.S., assisted by FERDINAND MÜLLER, M.D., F.R.S. & L.S., Government Botanist, Melbourne, Victoria, vol. I. (*Ranunculaceæ* to *Anacardiaceæ.*) London: Reeve & Co.

[1] Address of George Bentham, Esq., President, read at the Anniversary Meeting of the Linnæan Society, May 25, 1863. Published at the request of the Fellows. It is mainly a critical review of the recent progress of biological (i. e. in its properest sense, physiological) science, and is in almost every respect well-considered and forcible. In referring to Professor Wyman's paper, in this Journal, on the production of Infusoria, Mr. Bentham, probably relying upon others, has failed to appreciate the thorough care, appositeness, and simplicity of his experiments,—which, as we judge, stand at an advantage over Pasteur's,—especially in the very point remarked on, viz: the degree of heat applied. This was not only considerably higher in some of Wyman's experiments than in Pasteur's, but must have been far more efficient, as it was not exposure to dry heat, but boiling.

1863, pp. 508, 8vo.—This is the first volume of another of those Floras of British Colonies, published under the authority of the Home Government, and in the present instance, we believe, mainly at the expense of the colonies concerned, upon a plan arranged by Sir Wm. Hooker, in connexion with Mr. Bentham and Dr. Hooker. The Flora of Hongkong, by Mr. Bentham, was the first of the series. This related to a very small district. The present embraces vast regions and a preëminently interesting and peculiar vegetation. So immense is this undertaking that, with all his vast resources and preparation, and all his courage and aptitude, we can hardly suppose that Mr. Bentham would have taken it in hand, were it not for the coöperation of his colleague in this work, Dr. Ferdinand Müller, who has already done so much for the Australian Flora. While we with pleasure see Dr. Müller's name upon the title-page of this work, it is satisfactory to find that his important aid is offered and secured without the drawbacks which are inseparable from joint authorship, except where the parties can literally work side by side. Mr. Bentham is here undividedly responsible for the execution of the present volume. He has assumed the task with alacrity, prosecuted it thus far with his wonted vigor, and, from his wonderful knack of carrying through to completion whatever he undertakes, we may confidently hope that the botanists of each and every Australian colony may within a few years have furnished to his hands a Flora as perfect for its purpose as is the Handbook of the British Flora itself.

The excellent Outlines of Botany with special reference to Local Floras, which was prepared originally for the Handbook of the British Flora, is prefixed to the present as well as to the Hongkong Flora, having undergone studied revision. We notice that the radicle, which in the first instance was spoken of as "the future root," is now called "the base of the future root,"—expressing no opinion as to its morphological nature. In the next Colonial Flora, however, we confidently expect that it will be called the primordial internode, upon the node, i. e. the summit, of which the cotyledons are inserted,—a view which obviously suggests itself to the morphologist, and which, as we suppose, may be demonstrated by its position, its growth, and its structure. We know of nothing which is true of the internode next above the cotyledons which is not also true of that below them. In the first edition of these Outlines, "all the pistils of a flower" are spoken of, and the word is used as Linnæus used it. But in the revision, the author falls back to the Tournefortian use. This is a question of terminology, upon which opinions may fairly be divided: but we side with Linnæus. A. G.

4. *Notes on the Loranthaceæ, with a Synopsis of the Genera;* by Daniel Oliver, F.L.S., Professor of Botany, University College, London. —An important paper, read to the Linnæan Society in January last and recently printed in its *Journal*, No. 26. Prof. Oliver repudiates Mr. Miers' attempt to establish *Viscum* and its near allies as a separate order, wishes to unite *Santalaceæ* with the *Loranthaceæ*, and would even follow Baillon in adding the *Olacineæ*,—all of these clearly belonging to one natural group, the great divisions of which are kept apart mainly, it would seem, because of their wide separation in the Candollean sequence of orders. Some genuine species of *Phoradendron* are found to have the

anthers one-celled by confluence: indeed Prof. Oliver has found them so in some specimens of our North American Mistletoe, *P. flavescens.* No. 1125 of Fendler's Venezuela collection is found to belong to Pœppig's genus, *Antidaphne.* The bracts or scales subtending flowers in the genus *Lepidoceras* of Dr. Hooker are found to persist as the apex of a true lamina of a leaf, which is subsequently developed by a growth of its base in a very curious mannner. The scale does duty first as a bract, and afterwards, by a basal growth, the insertion or petiolar portion of this scale becomes a green leaf. The true Loranthaceous genera here admitted, and succinctly defined, are twelve, with indications that *Eremolepis Wrightii* of Grisebach, one of Mr. C. Wright's discoveries in Cuba—may be the type of a thirteenth genus. A. G.

5. *Parthenogenesis in Plants.—A presumed Case of Parthenogenesis in a species of Aberia*, a Bixaceous genus, reported by Dr. T. Anderson to the Linnæan Society, is recorded in the *Journal* of the proceedings of that Society, No. 26. One or two bushes of the species (of unknown origin) flowered in the Calcutta Botanical Garden in the year 1861. "They were female plants, no stamens were detected, yet they bore a large crop of well ripened fruits." "The seeds obtained from these plants were sown, and there is now a vigorous stock of young plants." In 1862 the same plant flowered again, and during a month produced only pistilliferous flowers. "From the opening of the first flower-bud until the last withered flower dropped off, not a day passed without a careful examination being made by me for the traces of a stamen in the flowers, but without finding one." The fruit set from many of the ovaries; but the tree was soon after destroyed in a gale. The evidence is not wholly complete; but as far as it goes it confirms the case of *Cœlebogyne.*

A. G.

6. *Structure and Fertilization of certain Orchids.*—In this *Journal* for November, 1862, I gave some notes on the arrangements of the *genitalia*, &c. of most of our Orchids of the Northern States of the genus *Platanthera* or *Habenaria.* One common species, which was not met with last summer in season, I have now glanced at, viz.,

Platanthera flava, or *Habenaria flava* Gray. This, although ascertained by me to be the *Orchis flava* of Linnæus, so ill deserves its specific name, which I restored to it, (the flowers being in fact green, instead of yellow) that, notwithstanding priority, one would like to see it take Muhlenberg's name of *virescens.* This might well enough be allowed on the ground that the Linnæan name is a "*nomen falsum.*"

As respects its arrangements for fertilization, I had anticipated that this would be an interesting species, on account of the strong protuberance or crest on the base of its labellum. This narrow and nasiform protuberance projects upwards and backwards, so as almost to touch the column between the two disks or glands of the stigma (or rather between the two cups or deep grooves which contain them), and therefore lying over and dividing the orifice of the spur. The anther cells are parallel, but set at a little distance apart: they lie almost in line with the labellum, but with the front ends depressed, so that the disks are a little lower than the base of the protuberance. These disks and this protuberance are so correlated in shape and position, that the proboscis of an insect fitted to suck out nectar from the spur, inserted, as it must be, obliquely from above, cannot keep the median line at the entrance, but will take the right or the left of the protuberance, as may happen, and so will slide into the disk-bearing groove of that side. The structure of the disk-bearing portion of the column answers, perhaps, to what is expressed by Lindley's vague character of *Gymnadenia*, "*rostello complicato*," and is quite different from that which prevails in the more genuine species of *Platanthera*. But nearly every species has its peculiar arrangement. Viewed from the front (on removing the labellum), each disk is found to line an oblong cavity or deep groove: viewed vertically from above, this appears as a ring with the front edge cut away, or as something more than a semicircle, lined by the thin broad disk. On directing a delicate bristle vertically from above into the spur, taking either side of the protuberance of the labellum, the bristle will either enter the discal groove from above, as a thread enters the eye of a needle, or, if presented more obliquely from the front, will slide into the groove when, as it enters the spur, it is raised, as it must be, to a more vertical position. The disk clasps the bristle, adhering by its sticky surface, and is withdrawn with it along with the attached pollinium. No good observations were made as to any movement of the stalk of the pollinium on the disk when thus extricated, nor as to its application to the stigma.

It is evident that in this species self-fertilization cannot occur, that only one pollen-mass will be likely to be extracted at one visit of an insect, and that this will doubtless be conveyed to another flower to impregnate its stigma.

Gymnadenia tridentata, Lindl.—Examinations of flower-buds and open flowers, July 27–30, substantially confirm those of the previous year, which are recorded in this *Journal*, vol. xxxiv, p. 426, and in a foot-note on p. 260. The flowers before expansion are horizontal and somewhat reclining, so that the packets of pollen, which spontaneously detach themselves from the pollen-mass, may fall out when the anther-cells open. The anthers dehisce before the flower-bud is full grown, or at least four or five days before the flower opens. In every instance when the flower has naturally opened, the anther-cells will be found widely gaping, and several or many pollen-packets will be found upon the three "stigmatic processes," into which their pollen-tubes will have copiously and deeply penetrated. This, indeed, is the case two or three days before the blossom would have opened.

These three processes are so remarkably developed in this species, and they so strikingly represent functionally, and to appearance morphologically, three elongated clavate stigmas, that the species would not be regarded as a congener of *G. conopsea*, the type of Brown's genus *Gymnadenia*. *G. flava* Lindl., and *G. nivea* Lindl., which I have examined only in dried specimens, however, present intermediate states. In *G. flava*, there are two strong stigmatic lobes projecting laterally beyond the disks by the side of the base of each anther-cell. The middle or rostellar lobe is hidden by the approximate anther-cells, and is functionless. In *G. nivea*, the rostellar lobe is minute and hidden; the conspicuous lateral lobes are linear and deflected forwards so as to lie along the border of the base of the labellum on each side. I do not know whether they receive the pollen, or whether there is a stigmatic surface between them under the disks, or both. But neither of these two species show any evidence of being self-fertilized in the bud. In *G. tridentata*, the species now in hand, the three elongated and somewhat clavate stigmatic bodies or processes, which are nearly alike, ascend, one on the outside of each anther-cell, and one between the two cells, to considerably above the level of the almost horizontal anther; their surface is loosely cellular and slightly viscid, so that the pollen-packets stick to them readily: all three, as already remarked, act functionally as stigmas. But also, underneath the disks and the common origin of the three stigmatic processes, I find a green surface, in position and character just like the stigma in *Orchis* and *Platanthera* except that (so far as I have observed, in unexpanded or freshly expanded flowers) it is only very slightly viscid. On application, few or no pollen-packets are left sticking to it, while they stick in considerable numbers to the upper part of the "processes." From the appearance, thus far, I should suppose that this normal stigmatic surface had become functionless. But, on the other hand, the large disks are in perfect condition; in the expanded flowers they adhere to a bristle or other body, and are thus removed from the shallow cups in which they rest, bringing away the caudicle with the considerable portion of the pollen-packets which still remain attached: the caudicle effects a prompt movement of depression, and now, if the bristle be returned to its position at the entrance of the spur, the pollen-mass will strike this broad ordinary stigmatic surface. The examination of older flowers may be expected to settle the question. But it is certain that the three linear club-shaped bodies act as stigmas.

In a systematic point of view, it is evident that the *Ophrydeæ* with naked disks need to be studied and arranged anew, upon living plants. But the forms cannot be clearly described and correlated until the morphology and terminology of the parts of the column have been reconsidered and elucidated. A. G.

Zoology.—

7. *Monograph of the Aye-Aye* (*Chiromys Madagascariensis* Cuvier); by Richard Owen, D.C.L., F.R.S., &c.—The curious animal which forms the subject of this monograph, was first noticed in Madagascar by Souverat in 1780, and owes its name to an exclamation of astonishment uttered by the natives of the east coast, to whom, it is said, he exhibited it for the first time. Souverat brought home with him a stuffed skin and a cranium, which have since remained in the museum of the Garden of Plants, the only representatives of the species in European cabinets. Zoologists have been puzzled as to the true affinities of the Aye-Aye, some placing it among the Rodents, and others among the Quadrumana. Buffon assigned it a position in the former group, and in so doing was followed by Cuvier, who at the same time distinctly stated that it "is related to the Quadrumana in more points than one." In view of these differences of opinion, it is easy to see how desirable whole specimens were.

Science is indebted to the Hon. H. Sandwith, M.D., for the specimen, preserved in spirits, which forms the subject of the present investigation. This has enabled Prof. Owen to determine definitely its position among the Lemuridæ, and at the same time add another to the admirable series of monographs by which the great English anatomist has contributed so largely to the progress of zoology and physiology in our day.

However remarkable the mingling of Rodent and Quadrumanous characters may be in the Aye-Aye, they are surpassed in the correlations of physical structure and strange habits. "The wide openings of the eyelids, the large cornea and expansile iris, the subglobular lens and tapetum, are arrangements for admitting to the retina and absorbing the utmost amount of light which may pervade the forest, at sunset, dawn, or moonlight. Thus the Aye-Aye is able to guide itself among the branches in quest of its hidden food. To detect this, however, another sense had need to be developed to great perfection. The large ears are to catch and concentrate, and the large acoustic nerve and its ministering 'flocculus' seem designed to appreciate any feeble vibration that might reach the tympanum from the recess in the hard timber, through which the wood-boring larva may be tunneling its way by repeated scoopings and scrapings of its hard mandibles." The food of this nocturnal animal, to whose strange physiognomy the eyes and ears add so much, consists mostly, as was ascertained by Souverat, of wood-boring grubs. To extract these, there are, united with the common Lemurine characters, chizel-shaped incisors, resembling those of Rodents, and a most remarkable modification of the middle finger, which is not only used for eliciting by percussion the hollow sound from the bored limb, but as a hook for extracting the grub. All the fingers are of somewhat unusual length, but the middle one "has been ordained to grow in length, but not in thickness with the other digits; it remains slender as a probe, and is provided at the end with a small pad and a hook-like claw." The use made of this part will be best learned from the very interesting letter to Prof. Owen by Dr. Sandwith, in which his own observations on the habits of the Aye-Aye are recorded.

Mauritius, Jan. 27, 1859.

My dear Mr. Owen:

After very great difficulty and much delay, I have at length obtained a fine male healthy adult Aye-Aye, and he is enjoying himself in a large cage which I have had constructed for him. . . . . . I observe that he is sensitive of cold, and likes to cover himself up in a piece of flannel, although the thermometer is now often 90° in the shade. . . . . . Now as he attacked every night the wood-work of his cage, which I was gradually lining with tin, I bethought myself of tying some sticks over the wood-work so that he might gnaw these instead. I had previously put in some large branches for him to gnaw upon; but the others were straight sticks to cover over the wood-work of his cage, which alone he attacked. It so happened that those I now put into his cage were bored in all directions by a large and destructive grub called the *Montouk*. Just at sunset, the Aye Aye crept from under his blanket, yawned, stretched, and betook himself to his tree, where his movements are lively and graceful, though by no means so quick as those of a squirrel. Presently he came to one of the worm-eaten branches, which he began to examine most attentively; and, bending forward his ears and applying his nose close to the bark, he rapidly tapped the surface with the curious second digit, as a woodpecker taps a tree, though with much less noise, from time to time inserting the end of the slender finger into the worm-holes as a surgeon would a probe. At length he came to a part of a branch which evidently gave out an interesting sound, for he began to tear it with his strong teeth. He rapidly stripped off the bark, cut into the wood, and exposed the nest of a grub, which he daintily picked out of its bed with the slender tapping finger, and conveyed the luscious morsel to his mouth.

I watched these proceedings with intense interest, and was much struck with the marvellous adaptation of the creature to its habits, shown by his acute hearing, which enables him aptly to distinguish the different tones emitted from the wood by his gentle tapping; his evidently acute sense of smell, aiding him in his search; his secure footsteps on the slender branches, to which he firmly clung with his quadrumanous members; his strong rodent teeth, enabling him to tear through the wood; and lastly, by the curious slender finger, unlike that of any other animal, and which he used alternately as a pleximeter (percuteur?), a probe, and a scoop.

But I was yet to learn another peculiarity. I gave him water to drink in a saucer, on which he stretched out a hand, dipped a finger into it, and drew it obliquely through his open mouth; this he repeated so rapidly that the water seemed to flow into his mouth. After a while he lapped like a cat; but his first mode of drinking appeared to me to be his way of reaching water in the deep clefts of the trees.

I am told that the Aye-Aye is an object of veneration at Madagascar, and that if any native touches one he is sure to die within a year, hence the difficulty of obtaining a specimen. I overcame this scruple by a reward of £10. . . . . .

Believe me, yours very faithfully,

H. Sandwith.

The "Conclusion" of the memoir will certainly attract very general attention, since it contains a distinct expression of Prof. Owen's view with regard to the great question of the day, that of "the origin of species." Those who have joined in the issue involved in this question may be arranged in one of two classes: 1st, comprising those who maintain that the present condition of the animal and vegetable kingdom was reached by a series of progressive creations, each species being created and suddenly introduced upon the surface of the earth, and the first formed individuals having the same specific characters as all the successors; 2d, those who deny the preceding view, and assert that all animals and plants are the result of "progressive development," "derivation" or "transmutation" of species, the first created forms being of the simplest kind, or at all events of a simpler kind than those of the present day, and in the course of time transformed into them. How the changes from simple to complex forms were effected, or how specific characters were modified, has been very differently explained. Lamarck says by a "*besoin*," Darwin by "natural selection" and "the struggle for existence," and Owen "by the ordained potentiality of second causes," and by transmutation "under law."

We do not propose, in this bibliographical notice, to enter into a discussion of these different theories, but, before citing Prof. Owen's views, we will merely remark that, if the progressive-creation hypothesis is adopted, we should be glad to see a better answer than has yet been made to the question, how and in what condition did the first forms make their appearance? When a mammal was created, did the oxygen, hydrogen, nitrogen, and carbon of the air, and the lime, soda, phosphorus, potash, water, &c., from the earth, come together and on the instant combine into a completely formed horse, lion, elephant, or other animal? If this question is answered in the affirmative, it will be easily seen that the answer is entirely opposed by the observed analogies of nature. In the practical study of the history of the earth and the changes which it has undergone, of the development of individual animals and plants, the "order of nature" points in one direction, viz: to the process of differentiation. The one-celled plant and the tree, the polyp and man, and all organic forms intermediate between these extremes pass from the homogeneous to the heterogeneous, from the nucleated cell, or even from what is more simple still, from plasma to the adult individual consisting of organs more or less complex, according to the position in the series. We nowhere see plants or animals reach maturity in any other way than by development or growth.

At the same time, we must not lose sight of the fact that what is true of the successive stages of individual organisms may not necessarily prove true with regard to the history of the races; that while, from the earliest embryonic condition of each individual to the last, there is a connected series of observed changes or differentiations, and no break in the organic continuity, there are no observations whatever to prove a like organic continuity in the races. In the absence of such direct proof, we have no other alternative than to look to the analogies of nature and the geological record. The direction in which the former point is obvious; the testimony of the latter is thus far negative, but is it complete enough to be a safe guide?

In view of the difficulties met with in explaining the first introduction of living forms, Agassiz has put forth the hypothesis of the creation of eggs. "I then would ask, is it probable that the circumstances under which animals and plants originated for the first time can be much simpler or even as simple as the conditions necessary for their reproduction only, after they have been once created? Preliminary then to their first appearance, conditions necessary for their growth must have been provided for; for, if, as I believe, they were created as eggs, the conditions must have been conformable to those in which the living representatives first introduced now reproduce themselves. If it were observed that they originated in a more advanced stage of life, the difficulty would be still greater, as a moment's consideration cannot fail to show, especially if it is remembered how complicated the structure of some of the animals was, who are known to have been among the first inhabitants of our globe."—(*Contrib. Nat. Hist. of U. States*, i, 12.)

This hypothesis would answer very well for spawning fishes and reptiles, whose eggs may be trusted to the effects of physical agents. But does it help us with regard to viviparous fishes, viviparous reptiles, and mammals? To take the case of the mammals, what "conditions conformable to those in which the living representatives first introduced now reproduce themselves" would answer the purpose for the development of the young, except an uterus, or something analogous to an uterus, and for its nourishment after birth, except a mammary gland or something analogous to one? And how could there be an uterus or a mammary gland without organs of nourishment, locomotion, &c.; in other words, before creating the egg, it would be necessary to create some kind of an organism for the egg to live in. If such organism offered the same conditions with those of the individuals now living, why create the egg at all? Rather than this, it would seem to be a simpler matter to create the whole animal capable of producing eggs to begin with. If it be asserted that the conditions were not the same, this assertion would seem to be equivalent to the admission of variation, inasmuch as the first egg would be capable of being developed under different circumstances from the later ones.

How Prof. Owen meets this difficulty with regard to the first introduction of species may be inferred from the following passages quoted from the monograph on the Aye-Aye.

"But the conception of the origin of species by a continuously operative secondary cause or law is one thing; the knowledge of the nature and mode of operation of that law is another thing. One physiologist may accept, another refute or reject, a transmutational or natural-selective hypothesis, and both may equally hold the idea of the successive coming-in of species by law."

"What I have termed the 'derivative hypothesis' of organisms, for example, holds that there are coming into being, by aggregation of organic atoms, at all times and in all places, under the simplest unicellular condition, with differences of character as many as are the various circumstances, conditions, and combinations of the causes educing them,—one form appearing in mud at the bottom of the ocean, another in the pond or the heath, a third in the sawdust of the cellar, a fourth on the surface of the mountain rock, &c., but all by the combination and arrangement of organic atoms through forces and conditions acting according to predetermined law. The disposition to vary in form and structure, according to the variation of surrounding conditions, is greatest in these first formed beings; and from them, or such as them, are and have been derived all other and higher forms of organisms on this planet. And thus it is that we now find, energizing in fair proportions, every grade of organization from man to the monad." . . . .

"Now the foregoing hypothesis is at present based on so narrow and, as regards the origin of life, so uncertain a foundation of ascertained facts, that it can be regarded only as a kind of vantage-ground artificially raised to expand the view of the outlooker for the road to truth, and perhaps as supporting sign-posts directing where that road may most likely be fallen in with." . . . .

. . . "And herein is one main distinction between it (origin of species by natural selection) and the 'derivative hypothesis,' which maintains that single-celled organisms, so diversified as to be relegated to distinct orders and classes of *Protozoa*, are now, as heretofore, in course of creation or formation, by the ordained potentiality of second causes; with innate capacities of variation and development, giving rise in a long course of generations to such differentiated beings as may be distinguished by the term 'plant' and 'animal;' from which all higher animals and plants have, through like influences, ascended and are being ascensively derived. This, as the naturalist knows, is mere hypothesis, at present destitute of proof. But it is more consistent with the phenomena of life about us, with the ever-recurring appearance of mould and monads, and with the coexistence, at the present time, of all grades of life rising therefrom up to man, than is the notion of the origin of life which is propounded in Mr. Darwin's book, 'On the Origin of Species by Natural Selection.'" . . . . .

"That organic species are the result of still operating powers and influences is probable from the great paleontological fact of the succession of such so-called species from their first appearance in the oldest fossiliferous strata; it is more probable from the kind and degree of similitude between the species that succeeds and the species that disappears never to return as such; the similitude being, in the main, of a nature expressed by the terms of 'progressive departure from a general to a special type.' Creation by law is suggested by the many instances of retention of structures in Paleozoic species, which are embryonal and transitory in later species of the same order or class; and the suggestion acquires force by considering the analogies which the transitory embryonal stages in the higher species bear to the mature forms of the lower species. Every new instance of structures which does not obviously and without straining receive a teleological explanation, especially the great series of anatomical facts expressed by the 'law of vegetative or irrelative repetition,'—all congenital varieties, deformities, monstrosities—opposes itself to the hypothesis of the origin of species by a primary or immediate and never repeated act of adaptive construction."

If we correctly understand Prof. Owen's views, as expressed in the above paragraphs, he inclines to, in fact adopts, though cautiously, the hypothesis of the origin of species by "transmutation" or "deviation;" these transmutations being in no accordance with a pre-arranged plan, but carried out under the influence of second causes. The first organisms were unicellular, brought into existence by spontaneous generation "under law," and, by a slow and orderly transmutation, ascensively differentiated into the highest vegetable and animal organisms. For the precise mode of bringing about the individual changes, he offers no conjecture, whatever.

We leave it for the advocates of progressive creation to answer these views—and will conclude with expressing the belief, that there is no just ground for taking, and that we arrive at no reasonable theory which takes, a position intermediate between the two extremes. We must either assume, on the one hand, that living organisms commenced their existence fully formed, and by processes not in accordance with the usual order of nature, as it is revealed to human minds, or, on the other hand, that each species become such by progressive development or transmutation; that, as in the individual so in the aggregate of races, the simple forms were not only the precursors, but the progenitors of the complex ones, and that thus the order of nature, as commonly manifest in her works, was maintained. J. W.

8. *On the "Minute Vertebrate Lower Jaw."*—In the *Annals and Magazine of Natural History*, for October, 1862, Dr. Wallich has given a figure and description of a jaw-like object $\frac{1}{100}$ of an inch in length, dredged at St. Helena, which he considers as "evidence of the existence of a vertebrate animal measuring only $\frac{1}{20}$ inch in length!" This has excited much discussion, several papers having since been written upon the subject, and although its vertebrate character has been fully disproved, there is much diversity of opinion in regard to the true character of the object. C. Spence Bate (*Ann. and Mag.*, Dec., 1862) thinks it to be the claw of an Amphipod. It has also been suggested that it may be part of the lingual ribbon of a Gasteropod; or part of the manducatory apparatus of a Rotifer. Mr. Busk, in an illustrated paper

in the *Quarterly Journal of Microscopical Science*, for Jan., 1863, has given the most probable solution :—that the jaw figured by Dr. Wallich is one of the valves or jaws of a pedicellaria of an Echinoderm, allied to *Amphidotus.*

In connection with this subject of the pseudomorphs of vertebrate jaws, we may take occasion to mention the jaws of some Annelids, the resemblance of which to those of the higher animals has not been mentioned by any of Dr. Wallich's critics. In the mouth of the proboscis of worms of the family *Aphroditacea*, such as *Polynoe*, *Sigalion*, and *Acoëtes*, we find upper and lower jaws, each composed of two rami, and armed with strong tooth-like processes. In seizing prey, these jaws work upon each other vertically, as in fishes and reptiles,—in contradistinction from the lateral action which takes place in all other Articulates, and which has been stated to be universal in that sub-kingdom. In the cut, fig. *a* represents the lower jaw of *Acoëtes lupina*, seen from above, the rami being pressed outward a little, to show their inner surfaces. Fig. *b*, a lateral view of the exterior surface of one of the rami. In the strong pair of teeth representing the canines and some other points, we have certainly as much resemblance to a vertebrate lower jaw as in the object originally figured by Dr. Wallich. W. S.


a b

9. *Note on the Megatherium;* by Prof. AGASSIZ, (*Proc. Bost. Soc. Nat. Hist.*, ix, 193.)—Prof. Agassiz, remarking on the Megatherium cast, presented by Mr. Bates, which had recently been mounted at the Museum of Comparative Zoology, stated that he regarded the position given it, which is very nearly that of Owen, as not quite accurate; and that the Megatherium, instead of being set up erect, should have been placed in a crouching attitude, with the hind legs bent, sufficiently so that the tail should touch the ground,—with the head bent down between the front legs, the broad chest resting upon the ground, supported by the fore-legs, extended in such a way that they should rest for nearly their whole length, and leave simply a free play for the extremities to reach out beyond the head.

V. ASTRONOMY AND METEOROLOGY.

1. *Procession and Periodicity of the November Star-shower.*—In the last number of this Journal (p. 148) it was suggested that the November star-shower has a motion along the *sidereal* year of one day in seventy years. The following dates, taken from the tables in the last number of the Journal, show this motion, and also indicate that the shower has a period of about a third of a century, as Prof. Olmsted, Prof. Twining, and Mr. Herrick have supposed. In the last column is given the date obtained by adding to the corresponding day of 1850 one day for each 70 years from the time of the shower to 1850.

A.D. 1833,	Nov. 12·7	corr. to A.D. 1850, Nov. 13·3,	becoming	Nov. 13·5
1799,	" 11·6	" " 12·9,	"	" 13·6
1698,	" 8·6	" " 11·6,	"	" 13·8
1533,	" 3	" " 7·0,	"	" 11·5
1366,	Oct. 29·5	" " 5·6,	"	" 12·5
1202,	" 26	" " 4·1,	"	" 13·4

A.D. 1101,	Oct. 24	corr. to A.D. 1850,	Nov.	3·0,	becoming	Nov.	13·7
1002,	" 20	"	"	Oct. 31·1,	"	"	12·2
934,	" 19	"	"	" 31·5,	"	"	13·8
931,	" 19	"	"	" 31·3,	"	"	13·4
903,	" 18·5	"	"	" 30·0,	"	"	12·5

The last date is given in the previous table as A.D. 902, Oct. 29th or 30th. A partial examination of the historical evidence leads me to the probable conclusion that the true date is the night of Oct. 18–19th, A.D. 903.

To comprehend fully the force of the argument involved in this table, the original records of these several showers should be given. This procession seems to imply that the orbit of the body furnishing these meteors has only a small inclination to the ecliptic, and that the motion is retrograde. The small distance of the radiant from the point to which the earth is moving, to wit 7°, confirms this conclusion. H. A. N.

2. *Star shower in* 1606.—On a bright night, Nov. 15th, 1606, it seemed as though it rained stars; first fell only the largest and brightest stars from heaven, then indiscriminately the large and the small ones in great numbers. Before they reached the earth they were extinguished.—E. A. Bielz, *Verhandlungen des Siebenbürg. Vereins zu Hermannstadt.*, Jahrg. xiii, 1862. Quoted in *Pogg. Ann.*, cxviii, 496.

3. *Meteorite of Tucson.*—The meteoric iron of Tucson, described on pp. 153–154 of this volume, and called on p. 154 the Bartlett meteorite, in allusion to its being figured and described by Mr. Bartlett in his Report, was first brought to the notice of the scientific world by Dr. John L. LeConte, as mentioned in this *Journal*, vol. xiv, 2nd ser., p. 289, (March, 1852).

4. *Observations of the August Meteors.*—The Committee upon Periodical Meteors of the Connecticut Academy of Arts and Sciences have this year undertaken a more extended system of observations than in any year preceding. In accordance with the desire and action of the Academy, a stellar chart suited to observations at all times was prepared by Prof. H. A. Newton, of the Committee, and distributed to observers at various stations, together with instructions for observing at the August period. The plan was to have parties of observers at two principal stations, New Haven and Hartford. These parties were to communicate by telegraph, so as to identify the meteors and to enable each corps of observers to give especial attention to those which had been actually seen at the other station. It was also an important part of the plan to secure the coöperation of other persons, especially of such as were near enough to see the meteors observed at Hartford and New Haven. Four hours on each of the three nights, from Aug. 8th to Aug. 11th, were designated as hours for watching. Notwithstanding the very unfavorable state of the weather, especially on two of the nights, and the failure of the telegraph also on a part of the third night, the Committee feel that they have been successful in their efforts. A goodly number of paths have been observed with such care as to afford data for computing their parallax. Data also for obtaining some idea of the velocities of these bodies have been obtained.

Two of the Committee, Prof. Elias Loomis and the undersigned, took charge of the party at the New Haven station. The remaining two members, Prof. C. S. Lyman and Prof. H. A. Newton, directed that at Hartford.

Observations have been received from Prof. Bache at the Coast Survey station near Wolcottville, Conn., from Prof. Hopkins at Williams College, from Mr. Hough, Director of the Dudley Observatory at Albany, from Mr. B. V. Marsh at Philadelphia, and from various observers at other stations, as will appear more fully from the report of Prof. Newton given below; who has undertaken the collation and reduction of the observations.

It appears that the meteors were most abundant on the morning of the 11th. For the Committee,

ALEX. C. TWINING, *Chairman.*

New Haven, Aug. 17, 1863.

Summary of observations of shooting stars during the August period, 1863, *compiled by* H. A. NEWTON.

July 25th.—From $9^h\ 30^m$ to 10^h P. M., I saw six shooting stars. There was a bright moon.

Aug. 3d.—From 2^h to 4^h A. M., Dr. A. W. Wright and myself recorded 16 paths. The sky was clear and moon bright. We could just see δ Ursae Minoris. One eighth of the time was lost in recording. The same day from $9^h\ 30^m$ to $10^h\ 30^m$ P. M., I saw only four flights. The moon had just risen.

Aug. 5th.—In the evening in ten minutes two of us saw four meteors.

Aug. 7th.—From $9^h\ 30^m$ to $10^h\ 45^m$ P. M., I saw 17 flights. The time actually employed in looking was estimated at 50 minutes. The sky was very clear and without moon.

Dr. A. W. Wright observed with a comet seeker, the diameter of whose field was about $2\frac{3}{4}°$, and whose magnifying power was 16, for a little more than an hour, commencing at 10 o'clock. He saw four shooting stars, three of which were not much brighter than the smallest stars visible through the instrument. He looked towards Polaris.

Aug. 8th.—The committee had proposed to make concerted observations from 9^h P. M. till 1^h A. M. But the clouds intercepted all view of the heavens at most places, and interfered seriously at the others.

At New Haven, Prof. Twining, Prof. Loomis, Dr. Wright, Mr. Robert Brown, Jr., Mr. W. Stocking, Mr. J. H. Kerr and Mr. T. W. Twining formed the observing party. They saw about 20 meteors and recorded most of their paths.

At Hartford were Prof. Lyman, Mr. P. H. Woodward, Mr. C. G. Rockwood, Mr. T. Hooker and myself. We recorded on the chart over 20 paths, and described in a general manner about 10 more.

At Hamden, 4 miles from New Haven, Prof. W. D. Whitney secured four observations.

At Philadelphia, Mr. B. V. Marsh, looking towards the northeast, recorded 14 paths, which were nearly all that he saw in three hours.

Mr. W. H. Hale, Ph.D., at Albany, saw two meteors, but the clouds prevented farther watching.

At Easton, Pa., Prof. J. H. Coffin saw and recorded 14 paths.

At least five shooting stars were observed at two places so as to furnish their parallax. Three of them were seen at three different places.

Two or three others were seen at two places, but not well enough to obtain altitudes.

Dr. Wright, looking with the comet seeker towards Polaris, saw, in about an hour of time suited for observing, four shooting stars. Two were nearly as bright as Polaris. The others were much fainter.

Aug. 9–10*th.*—The clouds and rain prevented observation at most places. We had arranged to observe from 11^h P. M. until 3^h A. M. Soon after one o'clock the clouds broke away partially at Hartford and New Haven. We obtained about 10 correspondences between these two places. Hon. J. Hammond Trumbull assisted us in Hartford. There were more shooting stars to be seen than on the previous night.

Prof. Whitney at Hamden, Mr. Marsh at Philadelphia, Mr. C. M. Whittelsey at Colchester, Conn., and Dr. Hale at Albany, each recorded several flights.

Aug. 10*th*–11*th.*—The sky was clear at some places but not at others. At New Haven were Prof. Twining, Mr. Brown, Mr. Stocking, and Mr. T. W. Twining. Dr. Wright was using the comet seeker. At Hartford, Prof. Lyman, Mr. Trumbull, Mr. Allen, Mr. Rockwood, Mr. Hooker and myself observed.

During the first hour and a half, the telegraph noted about 20 coincident observations, when one of the operators was taken ill. The flights were so frequent that some of these may be found to be coincidences of time only. The sky at New Haven soon after clouded over, and the party broke up. At Hartford, about 150 paths were recorded and many others described but not drawn on the chart.

Dr. Wright had about a half hour of good time for observing, in which he saw four very faint meteors.

At a station of the U. S. Coast Survey near Wolcottville, Prof. Bache and his assistants had made very careful preparations for observing. The weather was however very unfavorable. Prof. Bache reports 16 paths recorded by Mr. Geo. W. Dean, Assistant U. S. Coast Survey, Mr. R. E. Halter, Sub-assistant, and Mr. S. H. Lyman, Aid.

At Williamstown, Prof. Hopkins, assisted by Mr. Edward W. Morley, who was formerly an assistant in the observatory, recorded about 40 paths. The times of appearance were kept by an assistant at the transit clock.

Mr. Hough, aided by Mr. Simons and Mr. McClure, made at the Dudley Observatory careful estimates of the duration of flight, and a considerable number of determinations of place of appearance and disappearance.

Mr. R. Norman Foster, at Northampton, Mass., reports places of more than 20 paths, the times of appearance being noted by Mr. Louis Lamporte. Assistance was given him by Mr. Orlando Hastings and Mr. N. S. Wiard.

From Hamden, Prof. Whitney reports about 70 paths seen between 10^h P. M. and 3^h A. M.

Dr. Hale sends from Albany about 40 paths observed.

Mr. C. M. Whittelsey, at Colchester, Conn., recorded over 40 flights between 9^h and 12^h P. M.

Rev. T. S. Potwin, at East Windsor, Conn., and Rev. Wilder Smith at Berlin, Conn., each recorded about ten paths.

J. H. Worrall, Ph.D., at Newark, Delaware, recorded about 25 paths. Mr. Marsh, at Philadelphia, and Mr. F. Bradley, at Chicago, also made similar observations.

The numbers of shooting stars seen on this night were much larger than on the previous night. We think they were a little larger than on previous years.

Mr. B. V. Marsh observed alone near Philadelphia 130 flights between 10^h P. M. and 1^h 50^m A. M. He counted as conformable all which radiated from Perseus or Cassiopeia.

From 10^h 0^m	to 10^h 15^m,	in 15 minutes	he saw	2	conformable,	1	unconformable.
" 10 15	" 10 30	" 15	" "	6	"	1	"
" 10 30	" 10 45	" 15	" "	7	"	0	"
" 10 45	" 11 00	" 15	" "	4	"	0	"
" 11 0	" 11 15	" 15	" "	6	"	0	"
" 11 15	" 11 30	" 15	" "	7	"	4	"
" 11 30	" 11 45	" 15	" "	12	"	1	"
" 11 45	" 12 0	" 15	" "	4	"	1	"
" 12 0	" 12 15	" 15	" "	9	"	2	"
" 12 34	" 12 45	" 11	" "	10	"	1	"
" 12 52	" 1 0	" 8	" "	5	"	1	"
" 1 0	" 1 15	" 15	" "	7	"	0	"
" 1 15	" 1 30	" 15	" "	17	"	2	"
" 1 30	" 1 45	" 15	" "	14	"	0	"
" 1 45	" 1 50	" 5	" "	3	"	3	"
	Total,	in 204 minutes,		113	conf'mable,	17	unconformable.

There was spent in locating tracks, making notes, &c., probably one-fifth of the time, say 41 minutes. This reduces the time actually engaged in watching for meteors to 163 minutes, making the hourly number for one observer about 48.

At Manchester, Me., Mr. Joseph G. Pinkham and Mr. E. Pope Sampson counted in three hours 257, as follows:

From 9^h	to 10^h,	44	conformable, and	2	unconformable.
" 10	" 11	57	"	4	"
" 12	" 1	142	"	8	"
Total in 3 hours,		243	conformable,	14	unconformable.

At Williamstown, a party of about a dozen counted 162 between 11 and 12 o'clock.

At Hartford, six of us, counting aloud to prevent duplication, saw—

From 3^h 10^m	to 3^h 20^m, A. M.,	55	flights.
" 3 20	" 3 30 "	49	"
" 3 30	" 3 40 "	49	"
Total in a half hour,		153	"

Many of these were seen by two or more persons. It was estimated that the average number seen by each one during the half hour was about 40. The moon, the clouds near the horizon, and the twilight somewhat diminished the number. It was estimated that there was no increase of frequency over the two hours preceding. About a dozen were unconformable.

At Northampton, two persons saw 43 meteors between 9^h and 10^h 30^m. One person, between 10^h 30^m P. M. and 2^h A. M., assisted by a second observer during the last ten minutes, saw 360.

Capt. C. E. Dutton, near Portsmouth, Va., counted 38 in about an hour, early in the evening.

A party of three persons at the house of Prof. Silliman, Jr., counted 96 meteors from $9^h 15^m$ to $10^h 15^m$, about fifteen per cent of them were unconformable. A fourth person was present one third of the time.

Mr. Francis Bradley, at Chicago, had made arrangements for observing throughout the period. The clouds covered the sky until near one o'clock on the morning of the 11th. From $1^h 45^m$ to 2^h A. M. he counted 14 flights. From 1^h to 2^h he saw 87, of which 14 were not conformable to the radiant.

Mr. Francis Miller, at Sandy Spring P. O., Md., with an assistant, observed during the three nights, but the results have not yet reached us.

It was noticed at several places that almost the only very brilliant flights were early in the evening. This appears to have been connected with the fact that the radiant was then near the horizon.

Prof. Twining has given especial attention to the determination of the duration of flight of the meteors. He employed the method described by him in a previous number of this Journal ([2], vol. xxxii, p. 448). The average of over 20 estimates is about $0^s{\cdot}6$, and the average angular velocity $14\frac{1}{2}°$. This he considers too great for the average duration of flight of *all* the shooting stars, which is not probably more than $0^s{\cdot}4$ or $0^s{\cdot}5$. His estimates of time include the instant between the first perception and first definite sight. The space moved over in this instant not being usually included in the arc as laid down, the angular velocity above given is probably too small. The discussion of the estimated times of flight and velocities will be considered more at length hereafter. Valuable materials for this purpose have been contributed by various observers.

Prof. Twining watched on the evening of Aug. 11th from $11^h 45^m$ to 1^h A. M., for an exact determination of the radiant. This he locates at R. A., 47° 45′, N. P. D., 32° 45′. The radiant for the preceding evening he places near the sword handle of Perseus. At $10^h 25^m$ of that evening he saw a stationary star (the fourth he had ever seen) at R. A., 35°, N. P. D., 31° 40′. He considers the motion of the radiant, pointed out in a previous article (this Journal, [2], xxxii, 444), confirmed by the appearances this year.

I feel great doubt however myself of the motion of the radiant. The radiant is not a point, but a region of some size. Its exact determination is to be made out, I think, only by considering a large number of paths. The stationary star seen by Prof. Twining of course fixes only the direction of that path. Mr. Allen, at Hartford, at $2^h 18^m$ A. M., saw a stationary star in the constellation Triangulum. If the exact shape and size of the region of emanation could be determined, it would give us valuable materials for finding the shape and extent of the ring, or disk, which furnishes these small bodies.

At Natick, Mass., Mr. F. W. Russell, assisted by Mr. J. H. Wilson, Mr. F. W. Harwood and Mr. E. H. Wolcott, observed on various evenings from Aug. 4th to Aug. 13th. The following facts are condensed from his report.

Aug. 4th.—Three observers while walking in the evening saw in one hour 12 meteors.

Aug. 5th.—From 9^h to 10^h P. M., two persons saw, while walking, 15 flights, 10 of them in the last 15 minutes.

Aug. 6th.—Three observers saw 30 meteors in an hour. A faint aurora visible a part of the time.

Aug. 7th.—Sky much obscured. In ten minutes two persons saw 5 flights.

Aug. 9th.—A friend saw 20 very bright meteors in less than 30 minutes.

Aug. 10th.—In the morning, from 3^h 40^m to 4^h, Mr. Russell saw 13 flights, twelve conformable. In the evening, four observers watched from a little after 9^h P. M. until a little after 12 P. M., and three observers the rest of the time until 2 o'clock A. M. The following is the result:

From 9^h	to 10^h,	74	conformable,	2	unconformable,	
" 10	" 11	123	"	9	"	
" 11	" 12	140	"	24	"	
" 12	" 1	124	"	21	"	
" 1	" 2	119	"	25	"	
	Total,	580	"	81	"	in all 661.

No two meteors were counted twice. The hourly averages from the above table are, for each observer, 1st hour 19, 2d hour 33, 3d hour 41, 4th hour 46, and 5th hour 48. While arranging the watch, 27 more were seen, making 688 in about 5^h 10^m. The radiant was in the triangle, η, γ, τ Persei. Seven meteors were observed which passed through Cassiopeia almost into this triangle.

Mr. W. G. Bryant, at Winchendon, Mass., saw 41, between 8^h 30^m and 10^h P. M.

Aug. 11th.—In the evening, Mr. Russell saw—

From 9^h	to 10^h,	6	conformable,	4	unconformable.
" 10	" 11	29	"	10	"

Thin clouds and distant lightning interfered with observation.

Aug. 12th.—Two observers, from 9^h to 11^h P. M., saw 67 meteors, as follows:

From 9^h	to 10^h,	23	conformable,	11	unconformable.
" 10	" 11	18	"	15	"

Aug. 13th.—Two observers, from 9^h to 10^h P. M., saw 12 conformable, and 11 unconformable.

Mr. Russell gives the following positions of the centre of the radiant:

Aug. 4th,	R. A.,	7°	0′,	N. P. D.,	35°	30′,
" 5th,	"	358	20	"	31	10,
" 6th,	"	20	15	"	31	10,
" 7th,	"	27	40	"	33	45,
" 10th, A. M.,	"	43	0	"	37	0,
" 10th–11th,	"	39	35	"	36	20,
" 11th, P. M.,	"	42	30	"	37	40,
" 12th,	"	43	20	"	38	10,
" 13th,	"	49	25	"	31	20.

On some of these days, he was not able to fix satisfactorily the position, yet he thinks none will vary more than one degree from the truth, and that most of them are exact within half of a degree.

VI. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Electric Illumination at Boston,—Photometrical powers of the light*, (in a letter to Prof. B. SILLIMAN, Jr., from Prof. WILLIAM B. ROGERS, dated Boston, August 14, 1863).—I send you a short account of my observations on the power of the electric light exhibited with such striking effect by Mr. Ritchie on the evening of the 6th inst. (the National 'Thanksgiving') in this city. Through his kindness, I was enabled to make a more satisfactory measurement of the illuminating force of the battery at the State House, than I had the means of doing on the occasion of its former exhibition on the 4th of July.

The battery in question, consisting of 250 Bunsen elements, having each an acting zinc surface of about 85 inches, and grouped in five batallions of 50 each, was arranged in the dome of the State House, and the carbon light and the photometric apparatus prepared for the purpose were placed in line across the same apartment, commanding a range of about fifty feet.

In view of the immense power of the light as observed in the previous experiment, I substituted for the 20-candle gas burner, used at that time as the standard of comparison, a unit ten times as great, formed by the flame of a kerosene lamp placed in the focus of a small parabolic reflector and throwing its concentrated light on a photometric screen of prepared paper fixed in front of it, at the distance of five feet. Before the observation, the lamp and reflector were so adjusted as to make the light cast on the near side of the screen equivalent by measure to the action of 200 candles.

This was done by the intervention of a kerosene lamp fitted up with a bridge of platinum wire for defining and restricting the height of the square flame. Such a lamp I find of frequent use in ordinary photometry, as, when suitably adjusted, it gives the light of about eight standard candles, and thus transfers the measurement in the photometer to the wider divisions of the scale. Being suspended in a balance of peculiar construction, its rate of consumption enables us to correct for any slight departure from the assigned illumination. The lamp thus regulated was placed with its flat flame 12 inches from the screen, while the lamp in the reflector was distant 60 inches—and the flame of the latter was adjusted until the effects on the screen were equalized.

A platform supporting the standard lamp and screen at the assigned distance was arranged to slide on a horizontal graduated bar, extending directly towards the carbon points so that the screen should receive the rays from the electric light and from the reflector perpendicularly on its opposite faces. In making the observations, the platform was moved to and fro until the illumination on the opposite sides of the screen was judged to be equal, and then the measured distances of the two antagonizing lights from the screen gave by easy computation their relative illuminating power.

By a series of such observations, it was found that the carbon light had a force varying from 52 to 61 times that of the lamp with reflector, making it equivalent in illuminating power to the action of from 10,000 to 12,000 standard sperm candles pouring their light from the same distance upon the surface of the screen. This it will be remembered is the effect of the unaided carbon-light sending its rays equally in all directions from the luminous centre, and falls vastly short of the illuminating force of the cone of collected rays which was seen stretching like the tail of a comet from the surface of the great reflector. Judging from some recent experiments on the power of such a reflector to augment the intensity of the light emanating from its focus, there can be no doubt that, along the axes of the cone when brought to its narrowest limits, the illuminating force of the carbon light as displayed on the State House could be rivalled only by that of several millions of candles shining unitedly along the same line.

In the above described observations, a thick screen was necessary on account of the great intensity of the lights to be antagonized. I need hardly say that the different color of the two lights added much to the difficulty of the measurements. But, by marking in each case the extreme limits on either side, it was practicable to adjust the screen pretty accurately to equality of illumination.

The only previous experiment of precisely the same kind which I can recall is that of Bunsen, cited in the books, which was made with a battery of 48 elements. In this the photometric equivalent of the carbon light was estimated at 572 candles, or nearly 12 candles to the cell. My observations show a power more than three times as great, or about 40 candles to the cell, a difference due no doubt largely to the more intensive battery at my disposal and the cumulative effect of its arrangement. I suspect too that the elements in Bunsen's observation were of inferior size, but on this point I am without definite information.

2. *Vermilion Rock Salt Mine at Petite Anse, Louisiana.*—One of the facts of scientific interest brought to light by the Southern rebellion is the discovery of an important deposit of rock salt of remarkable purity in the island of Petite Anse, in Vermillion Bay, on the Gulf coast of Louisiana. By the kindness of Mr. Geo. D. Colburn, a large specimen of this salt has reached us. Its analysis by Dr. J. L. Riddell, of New Orleans, gives the following composition: Chlorid of sodium 98·88, sulphate of lime 0·76, chlorid of magnesium 0·23, chlorid of calcium 0·13, =100. This analysis, it will be seen, makes the Petite Anse salt almost pure.

Salt springs had been known on the island from an early period, but no suspicion existed of there being rock salt near the surface until the late owner, Judge Avery, with the view to improve the flow of water from one of the saline springs, caused an excavation to be made, when, at the moderate depth of only fourteen or fifteen feet from the surface, the laborers struck the bed of white rock salt, which they at first imagined must be ice. It was at once recognized, and proved of incalculable advantage to the Confederates, as well as a source of great wealth to the owner. The Island of Petite Anse (Little Elbow Island), so named from its shape, is a body of very fertile land, supporting rich crops of sugar cane and corn, besides forest trees, about 2½ miles long by 1½ wide and containing about 2100 acres. It rises 170 feet above tide in the midst of a wide spreading marine swamp. The soil of the island is an umber-colored argillaceous sandy loam, capable of forming good bricks.

The salt is covered by a whitish, cream-colored solid smooth rock, at an average depth of about 19½ feet below the surface or 4½ feet below tide water. There is no moisture or brine in the deposit, the salt being compact, hard and perfectly dry. Our intelligent correspondent is not a geologist, nor does he send us any fossils with the salt. But the deposit is undoubtedly of Tertiary age.

3. *Note on the Rule of Priority.*—In reading the reclamation of Mr. Crookes, reproduced in the March No. of this Journal (pp. 277–279), it seems that some confusion prevails, through which a rule of naturalists, regulating nomenclature, is misapplied. Mr. Lamy is said to state, and Mr. Crookes cites it as if indisputable, that "*it is priority of publication which constitutes priority of invention;*" and the latter thinks himself thereby precluded from producing even the personal testimony of an observer in his behalf. We venture to say that the rule above-cited has no existence, and from the nature of the case could and ought to have none. The fact of a *discovery* is to be established by evidence, and no sort of evidence by which it may be established can be excluded. Abundant illustrations of this may be adduced from the history of almost every science. The rule which has been here misapprehended is one which fixes *nomenclature.* Naturalists have established, and physicists, &c., have adopted, the very necessary rule that publication is essential to give validity to a *name,*—that the name first published takes precedence. The discovery of a fact or a thing, and the imposition of a name, are two different matters, and not rarely dissociated. The first is to be established by any good evidence: the second is governed by an arbitrary but most just rule. Thus the name *Thallium,* it appears, is established by Mr. Crookes, by priority of publication. The date of the discovery of the metal, to which this name is given, is to be authenticated by whatever testimony can be adduced,—is a question of fact and not a question of nomenclature. A. G.

[The preceding 'Note' was prepared immediately after the issue of the March number of this Journal. As it states clearly a well established though often forgotten principle in the ethics of science, we now put it on record for future reference. In the June number of the *Phil. Mag.*, Mr. Crookes has published a full history of the discovery of thallium with all its documents, for an early copy of which he has our thanks. Tested by the criterion of the above Note, all doubt on the priority of Mr. Crookes's discovery disappears, if any before existed. It is not requisite to discuss whether the printing of labels and the exposure of specimens in the great Exhibition of 1862 constituted publication. The question of *priority* is decided in Mr. Crookes's favor by abundant testimony which no weight of academic or personal influence can overthrow.]

4. *The thirty-third Meeting of the British Association for the Advancement of Science* was held at Newcastle-upon-Tyne, Aug. 26, but at this time, of course, no report of the proceedings has reached us.

5. *Personal.*—Dr. WOLCOTT GIBBS, one of the associate Editors of this Journal, has accepted his election to the Rumford foundation in Harvard College, left vacant some time since by the resignation of Prof. E. N. Horsford. Dr. Gibbs enters on his new duties at Cambridge at once, having already removed from New York, where he has so long and ably discharged the duties of the chair of Chemistry and Physics in the New York Free Academy.

VII. BOOK NOTICES.

1. *Heat considered as a mode of Motion:* being a course of twelve lectures delivered at the Royal Institution of Great Britain in the season of 1862; by JOHN TYNDALL, F.R.S., &c., Prof. of Nat. Phil. in the Royal Institution. With illustrations. New York: D. Appleton & Co. 1863. 12mo, pp. 480.—This book is destined to become a classic in the literature of science. With all the skill which has made Faraday the master, in Great Britain, of experimental science, Dr. Tyndall enjoys the advantages of a superior general culture, and is thus enabled to set forth his philosophy with all the graces of eloquence and the power of superior diction. The Royal Institution is truly fortunate in the succession of the eminent men who, from its organization at Sir Joseph Banks's house on the 9th of March, 1799, to this time, have made its name illustrious. Young, Davy, Faraday, Tyndall, in physical science, Smith (Sir James Edward), Lindley, Roget, Grant, Jones, Carpenter, Huxley, and Owen, in natural history and anatomy, form a list such as few literary or scientific institutions in the world can boast.

The lectures at the Royal Institution have always been of commanding interest. In them Davy first expounded the philosophy of flame, the nature of chlorine, the existence of the alkali metals. Faraday followed with his ever-memorable electrical researches, the laws of electrolysis, the conductibility of gases, diamagnetism, and the magnetism of gases; Tyndall, on the transmission of heat through gases, the glaciers and vegetation, and now with the course before us, in which, with a simplicity and absence of technicalities which render his explanations lucid to unscientific minds, and at the same time a thoroughness and originality by which he instructs the most learned, he unfolds all the modern philosophy of heat, commencing with the researches of von Mayer fifteen years ago, and embracing those of Joule and others at a later date. The first seven lectures deal with *thermometric heat:* its generation and consumption in mechanical processes; the determination of the mechanical equivalent of heat; the conception of heat as molecular motion; the application of this conception to the solid, liquid, and gaseous forms of matter; to expansion and combustion; to specific and latent heat; and to calorific conduction.

The remaining five lectures treat of *radiant heat:* the interstellar medium, and the propagation of motion through this medium; the relations of radiant heat to ordinary matter in its several states of aggregation; terrestrial, lunar, and solar radiation; the constitution of the sun; the possible sources of his energy; the relation of this energy to terrestrial forces and to vegetable and animal life.

The author rises to the level of these questions from a basis so elementary, that a person possessing any imaginative faculty and power of concentration can accompany him.

Our readers will have an opportunity of becoming familiar with many of the fundamental ideas developed so beautifully in Dr. Tyndall's book, by perusing the memoir of von Mayer, the republication of which is commenced in the present number. It will need no addition of ours to induce all who feel an interest in this department of research to read up by Dr. Tyndall's "Heat as a mode of Motion."

2. *Brande and Taylor's Chemistry.*[1]—This book is remarkable among English manuals of chemistry for entering directly upon the discussion of pure chemistry without the usual chapters on general and chemical physics. Matter and its properties, crystallization, isomorphism, chemical affinity, solution, electrolysis, equivalent weights and volumes, and nomenclature and notation, are all disposed of in about seventy pages. The body of the book (609 pages) is almost equally divided between the subjects metalloids, metals, and organic chemistry. It is eminently a practical old-fashioned book, prepared by authors who are neither of them remarkable for their own additions to the science, but who have a well-earned reputation as teachers and compilers, functions which, when ably discharged, are certainly most meritorious. This volume is very carefully prepared, and abounds on every page with marks of fidelity and patient reading. It is an excellent book to put into the hands of students, and as a guide to the teacher, although its uniform small type, unrelieved by any change of appearance beyond italic heads, and destitute entirely of all illustrative cuts, renders it by no means easy reading. It is impossible to prepare a book on chemistry more completely in contrast with the recent work of Dr. Odling, (the first part which we owe to the kindness of the author) in which the unitary system is, for the first time in English, thoroughly carried out.

3. *Supplement to Ure's Dictionary.*[2]—Dr. Ure's dictionary has a hold upon the general mind beyond that of any similar work in the English language. It is a very acceptable thing, therefore, to a large class of readers, to have this Supplement by Mr. Hunt, designed to bring matters up to the present time in very numerous departments in which the rapid progress of science and art had left very much to desire.

Assisted by about fifty of the best men in England, Mr. Hunt has produced in this Supplement a work certainly of varied, but in the main of great excellence. While it is truly a Supplement to Ure's Dictionary, it is also an independent and very comprehensive book of reference by itself, furnished at a moderate cost and consequently acceptable to a large number. In the new English edition of Ure in three volumes (at a cost of $38) the materials of this American edition are distributed throughout the older matter. The American edition is also in three volumes (embracing 3212 pages), of which this Supplement is the third.

4. *The American Annual Cyclopedia and Register of Important Events of the year* 1862. Vol. II. New York: D. Appleton & Co. Large 8vo, pp. 830.—This work is designed to embrace a double character, as an annual register and as a continued supplement to the American Cyclopedia by the same editors, the completion of which we have already announced (this Journal, [2], xxxv, 304). As is both natural and

[1] Chemistry. By William Thomas Brande, D.C.L., F.R.S., L. and E., and Alfred Swaine Taylor, M.D., F.R.S., &c. Philadelphia: Blanchard & Lea. 8vo, pp. 696.

[2] A Supplement to Ure's Dictionary of Arts, Manufactures, and Mines, &c., edited by Robert Hunt, F.R.S., F.L.S., Keeper of Mining Records, assisted by numerous contributors eminent in science and familiar with manufactures. Illustrated with 700 engravings on wood. New York: Appleton & Co. 1863. Large 8vo, pp. 1096.

proper, a large space is devoted to the registration of the great political events which are now in progress in the United States. Thus, under "Army operations," we have a review of military events for 1862, filling 172 pages, with numerous maps and plans. But science is not forgotten under the heads Astronomy, Auroras, Barometer, Building materials, Chemistry, Earth, Electricity, Illumination, Meteorology, and many other titles, which are partly reviews of progress and partly new additions. The work is very valuable as a book of reference, and displays great industry and good judgment in its preparation.

5. *Transactions of the Illinois Natural History Society*, Vol. I, 2nd ed. 194 pp. 8vo. Springfield, Illinois; edited by C. D. Wilder, Secretary.—This first part of volume I. contains a short paper on a geological section of the Rock river valley, Ill., by O. Everett, M.D.; another on the remains of the Mastodon in Illinois, by C. D. Wilder; others containing catalogues of Illinois species in Botany and some departments of Zoology, by R. H. Holder, C. Thomas, and G. Vasey, besides an article on Insects injurious to vegetation, by B. D. Walsh. The Mastodon remains described were found in Northern Illinois near Aurora (west of Chicago). They consist of two tusks and seven teeth. The tusks were 10 feet long.

6. *On the Origin of Species, or the Causes of the Phenomena of Organic Nature*—a course of six Lectures to Working Men; by Thomas H. Huxley, F.R.S., etc. 150 pp. 8vo. 1863. New York, D. Appleton & Company.—This little volume, republished from the English edition, is a brief popular discussion of the great subjects mentioned in the title. We have already expressed some of our objections to the teachings of the author on species, in our notice of his work on "Man's Place in Nature."

OBITUARY.

Dr. Samuel Prescott Hildreth, of Marietta, Ohio, for nearly forty years a constant contributor to this Journal, died July 24, 1863, at Marietta, after an illness of about three weeks. His disease, enteric fever, commenced July 5, resulting in hemiplegia on the 18th, manifested by insensibility and paralysis of the right side, which continued until death. He was nearly 80 years of age, having been born Sept. 30th, 1783, in Methuen, Essex Co., Massachusetts.

Dr. Hildreth was one of the first pioneers of science in the country west of the Alleghany Mountains. His first communication appeared in the tenth volume of this Journal, (1826), "On facts relating to certain parts of Ohio." His series of 'Meteorological registers' commenced in vol. xvi, for 1828, and has been continued without intermission for thirty-five consecutive years, the last being published in March of this year (xxxv, 181). At the request of the Senior editor of this Journal, Dr. Hildreth undertook an exploration of the coal regions of the Ohio, which was published in January, 1836, (vol. xxix, p. 1), under the title of "Observations on the Bituminous coal deposits of the valley of the Ohio, and the accompanying rock strata; with notices of the fossil organic remains and the relics of vegetable and animal bodies, illustrated by a geological map, by numerous drawings of plants and shells, and by views of interesting scenery." This was the most important of Dr. Hildreth's scientific labors and by far the most valuable contribution which up to that time had appeared on the subject discussed. It filled an entire number

(155 pages) of this Journal, and was profusely illustrated by figures of fossils, sections, and original drawings, embraced in thirty-six plates on wood.

In 1837, Dr. Hildreth was appointed one of the assistant Geologists upon the Geological Survey of the State of Ohio; his report forms part of the published documents relating to that survey. The memory of Dr. Hildreth will always be cherished among the early contributors to American geology.

Dr. Hildreth was also an industrious and acute observer of facts in his special department, and the medical journals from 1808 to 1825 contain many valuable papers from his pen.

His active mind embraced among the objects of his research various interesting historical questions, and we are indebted to his industry and personal knowledge for the preservation of many valuable facts relating to the early history of Ohio and Western Virginia. Such are his "*History of the Settlement of Belville, Western Virginia*," published in the *Hesperian Magazine*. In 1848 he published "*Pioneer History of the Ohio Valley and of the Northwest Territory*," a volume of 525 pages. This work is drawn chiefly from original sources, and is full both of entertainment and instruction. It contains plans and picturesque views of the early forts and villages of the pioneer settlers, who were emigrants from New England and chiefly from Connecticut. In 1852, he issued his "*Lives of the Early Settlers of Ohio*," an 8vo. of 539 pages.

His collections in various departments of natural history, to the number of about 4000 specimens, he presented, together with his scientific library, to Marietta College, Ohio, where they occupy a room known as the 'Hildreth Cabinet.'

Dr. Hildreth did not shrink from his share of the duties and responsibilities of civil life which the republic imposes on all her sons. At the age of 27 we find him (in 1810 and again in 1811) in the Ohio Legislature as a supporter of Jefferson and Madison. He held the office of collector of non-resident taxes for eight years, when the office (in 1819) was abolished. He was also Clerk of the Ministerial Lands, to the close of his life, for 53 years.

In his private life he illustrated every virtue of a christian gentleman. Bright and cheerful by nature, he loved nature with the simple enthusiasm of a child. Industrious and systematic in a high degree, no moment of his life was wasted. In his family, we have seen a beautiful example of domestic happiness and warm hearted hospitality. He lived with nature and nature's God—and among the patrons and co-workers in this Journal who have left its founder almost alone, no one has shed a purer and more mellow light in the horizon of his setting sun—no one has departed more loved and regretted by the Senior Editor.

Joseph Stillman Hubbard, Professor of Mathematics in the U. S. Navy, and since 1845 detailed to duty in the Naval Observatory at Washington, died at the house of his widowed mother, in New Haven, Aug. 16th, 1863, aged 40 years.

Few of the younger men of science in America have a more honorable reputation than Prof. Hubbard. His taste for astronomy and his mathematical ability were evident during his undergraduate course at Yale College, where he graduated in 1843. Early in 1844, he was appointed an assistant in the High School Observatory, at Philadelphia, then in charge of the distinguished astronomer, Sears C. Walker. The next autumn he was employed by Captain (now Major General) Fremont to reduce his Rocky Mountain observations, and was invited to accompany him on his next expedition. Declining this offer, he was, at the instance of Fremont and Senator Benton, appointed by Hon. Geo. Bancroft, then Secretary of the Navy, a Professor of Mathematics in the U. S. Navy, and assigned to duty in the then new Naval Observatory, at Washington. This post he filled with distinguished zeal and fidelity to the time of his death. The printed volumes of the Washington observations are full of the evidences of his skill as an observer and a computer. His powers, under the peculiar management of Superintendent Maury, were made to contribute, perhaps, more to the factitious reputation of that person, than to his own advantage. The flight of Maury to his own place, and the accession of Gilliss to the head of the Observatory, was no less a matter of congratulation to American science than to the officers of the Observatory.

Prof. Hubbard was a frequent contributor to the *Astronomical Journal.* His investigations on Biela's comet are there recorded in a series of elaborate papers, as also those on the great comet of 1843, on the orbit of Egeria and many others. The article, Telescope, in Appleton's *American Cyclopedia*, a valued paper of much labor, is also from his pen.

His labors of love in the cause of benevolence and religion were not less zealous and unremitting in the discharge of every duty, than in the paths of science. He was married in 1848, but his wife died before him. He left no children.

James Renwick Chilton, of New York, long known as a leading commercial chemist, died at Yonkers, N. Y., July 24, 1863, at the age of 54 years.

Stillman Masterman, of Weld, Maine, died July 19, 1863, aged 32 years. He was an esteemed astronomical observer and an occasional correspondent of this Journal. A notice of his life and labors will appear in our next.

Memoirs of the American Academy of Arts and Sciences, New Series, vol. viii, part i.—1, An unpublished Grammatical Fragment: Petronius Arbiter de Antiquis Dictionibus; *Charles Beck.*—27, On the Alloys of Copper and Zinc; *Frank H. Storer.*—57, On the Impurities of Commercial Zinc, with special reference to the Residue insoluble in dilute acids, to Sulphur, and to Arsenic; *Charles W. Eliot* and *Frank H. Storer.*—97, Remarks on the latest form of the Development Theory; *Francis Bowen.*—123, On the Secular Variations and Mutual Relations of the Orbits of the Asteroids; *Simon Newcomb.*—153, Plantæ Wrightianæ e Cuba Orientali (Polypetalæ et Apetalæ); *A. Grisebach.*—193, Filices Wrightianæ et Fendlerianæ,—nempe in Insula Cuba a Carolo Wright et in Venezuela ab Aug. Fendler, ann. 1854–60 (nonnullis Panamensibus etc. interjectis), Enumeratæ curæ *Daniel C. Eaton, A.M.*—221, On the light of the Moon and of the planet Jupiter; *George P. Bond.*—287, Comparison of the light of the Sun and Moon; *G. P. Bond.*—299, A Catalogue of the declinations of 532 Stars culminating near the zenith of the Observatory of Harvard College, Cambridge; *T. H. Safford.*—333, The Lumbar Enlargement of the Spinal Cord; *John Dean, M.D.,* with four plates.—354, On some of the relations of Salts of Zinc and Alumina to Soda and Potassa; *E. N. Horsford.*

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[SECOND SERIES.]

ART. XXVIII.—*On certain parallel relations between the classes of Vertebrates, and on the bearing of these relations on the question of the distinctive features of the Reptilian Birds;* by JAMES D. DANA.

AT the close of an article by Prof. Hitchcock, in this volume (p. 57), a portion of a letter of the writer is quoted, in which a parallelism is drawn between the Oötocoid or semi-oviparous Mammals (*Marsupials* and *Monotremes*), the Ichthyoid Reptiles (*Amphibians* of DeBlainville, *Batrachians* of many authors), and the Reptilian Birds. The general fact of this parallelism throws light on (1) the classification of Mammals, (2) the distinctive features of the Reptilian birds, and (3) the geological progress of life.

1. *Classification.*—The Amphibians are made by many zoologists an independent class of Vertebrates, on the ground of the fish-like characteristics of their young. The same systematists, however, leave the Marsupials in the class of Mammals, notwithstanding their divergencies from that type. The number of classes of Vertebrates, usually regarded as four, thus becomes *five*, namely, Mammals, Birds, Reptiles, Amphibians and Fishes. There are some indications that this number will soon be further increased by some zoologists, through the making of another class out of the *Reptilian Birds.*[1]

[1] Professor Agassiz, in vol. i of his *Contributions to the Natural History of the United States*, page 187, subdivides Fishes into four *classes*, namely, 1, Myzonts; 2, Fishes proper, or Teliosts (Ctenoids and Cycloids); 3, Ganoids; 4, Selachians; which would make the total number of classes of Vertebrates *nine*.

The discovery of the Reptilian Birds has brought the general law to view, that, among the four classes of Vertebrates, ordinarily received, each, excepting the lowest, consists of, *first*, a grand *typical* division, embracing the majority of its species, and *secondly*, an inferior or *hemitypic* division, intermediate between the typical and the class or classes below.

Before proceeding with our illustrations of this point, a word may be added in behalf of these four classes. In order to appreciate their true value, it is necessary to have in view the *type-idea* which is the basis of the fundamental characteristics of each, and which is connected with the existence of *three* distinct habitats for life—the water, the air, and the land: that in Fishes, this idea is that of *swimming aquatic* life; in Reptiles, that of *creeping terrestrial* life; in Birds, that of *flying aerial* life; in Mammals, that of *terrestrial* life, again, but in connection with a higher grade of structure, the Mammalian. The type-idea is expressed in the adults both of the typical and hemitypic groups; and any attempt to elevate the hemitypic into a separate class tends to obscure these ideal relations of the groups in the natural system of Vertebrates.

The following are the illustrations of the law above mentioned.

(1.) In the classification of Vertebrates, Mammals, the first class, are followed by Birds, as the second; and while the former are viviparous, the latter are, without exception, *oviparous*. The species of the inferior or hemitypic group of Mammals, partake, therefore, in some degree, of an *oviparous* nature, as the term *semi-oviparous* or *Oötocoid* implies.

In fact, all Vertebrates excepting Mammals are typically oviparous, although some cases of viviparous birth occur among both Reptiles and Fishes. In the viviparous Mammal, the embryo during its development derives nutriment directly from the body of the parent until birth, and also for a time after birth; while in the viviparous Fish, the Selachians excepted, there is simply a development of the egg internally, in the same manner, essentially, as when it takes place externally. Applying then the term oviparous to all cases in which the embryo is shut off from any kind of placental nutrition, Reptiles and Fishes, with the exception mentioned, are as essentially oviparous as Birds. Hence, the Oötocoids or non-typical Mammals are actually intermediate in this respect, and in others also, between the typical Mammals, on one side, and the inferior oviparous Vertebrates collectively, on the other.

(2.) Again, the class next below Birds is that of Reptiles. And, correspondingly, the inferior or hemitypic group of Birds is *Reptilian* in some points of structure.

(3.) Again, the class next below Reptiles is that of Fishes; and therefore the inferior or hemitypic group of Reptiles is the intermediate or *Ichthyoid* one of Amphibians—the young of frogs and salamanders and other included species having gills like fishes, besides some additional fish-like peculiarities.

The parallelism between the three classes, Mammals, Birds and Reptiles, is thus complete.

(4.) Fishes have no class of Vertebrates below them, so that an *inferior* hemitypic division is not to be looked for. It might be suspected that the intermediate group in this case would be one between Fishes and the lower subkingdoms either of Mollusks or of Articulates; but none such exists. The lowest fish, an Amphioxus, is as distinctly a Vertebrate as the highest, and no Mollusk or Articulate exhibits any transition towards a vertebrate structure.

There are, however, *hemitypic* Fishes; but their place is towards the *top* of the class instead of at its bottom. Ganoids constitute one group of this kind, between Fishes and Reptiles, as long since pointed out by Agassiz. Again, Selachians (or Sharks and Rays) constitute another, between Fishes and the higher classes of Vertebrates. This last idea also has, we believe, been suggested by Agassiz (although we cannot refer to the place where published), this author regarding the species as intermediate in character between Fishes and the allantoidian Vertebrates. Moreover, Müller long ago observed the relation of the Sharks to the Mammals in having a vitelline placenta, by which the embryo draws nutriment from the parent, as does the mammalian fetus by means of its allantoidian placenta.

Ganoids and Selachians are, thus, two *hemitypic* groups in the class of Fishes.

The scheme of grand divisions is then as follows:[2]

I.
A. Typical Mammals,
B. Hemitypic Mammals.
or OÖTOCOIDS.

II.
A. Typical Birds,
B. Hemitypic Birds.
or ERPETOIDS.

III.
A. Typical or true Reptiles.
B. Hemitypic Reptiles,
or AMPHIBIANS.

IV.
A. Hemitypic Fishes, or SELACHIANS.
B. Hemitypic Fishes, or GANOIDS.
C. Typical Fishes, or Teliosts.

One of the groups of hemitypic Fishes looks directly towards Reptiles, and the other towards the three higher classes of Vertebrates collectively, but especially Mammals and Birds.

[2] It is here seen that the term *Oötocoid*, applied to Marsupials and Monotremes, has great significance; and so likewise, *Erpetoids*, and *Amphibians*. *Oötocoid* is simply the Greek form of the term *semi-oviparous*.

It is plain from the preceding that the subkingdom of Vertebrates, instead of tailing off into the Invertebrates, has well-pronounced limits below, and is complete within itself.

2. *Distinctive features of the Reptilian division of Birds.*—The skeleton of the fossil Bird, discovered at Solenhofen, has some decided Reptilian peculiarities, as pointed out by Wagner, Owen, and others. But even if perfect, it could not indicate all the Reptilian features present in the *living* animal. It is, therefore, a question of interest, whether the relations of the hemitypic to the typical species in the two classes, Mammals and Reptiles—one superior to that of Birds, and the other inferior—afford any basis for conclusions with regard to characteristics of the hemitypic Birds undiscoverable by direct observation. The following considerations, suggested by analogies from the classes just mentioned, may be regarded as leading to unsatisfactory results; and yet they deserve attention.

A. *Mammals.*—(1.) It is a fact to be observed that the hemitypic Mammals are as truly and thoroughly *Mammalian*, as regards the fundamental characteristic of the type—the suckling of their young—as the typical species.

(2.) The departure from the typical Mammals is small in the *adult* individuals, especially the adult males. But it is profoundly marked in their *young*, they thus approximating in period of birth and some other respects to oviparous Vertebrates.

B. *Reptiles.*—(1.) The *adult* Amphibians, or hemitypic Reptiles, depart but little from the typical Reptiles, either in structure or habits.

But (2.) the *young*, in their successive stages, from the egg upward, partake strikingly of characters of the inferior class of Fishes.

The law seems, then, to be that the species of the hemitypic group have their principal or most fundamental resemblance to those of the class or classes below in the *young* state. We should hence conclude that the *young* of the Reptilian Birds or Erpetoids possessed more decided Reptilian peculiarities than the adults.—What these unknown peculiarities, if real, were we can infer only doubtingly from the analogies of the known cases already considered.

The characteristic of the intermediate type, on which the intermediate character depends, is, in the case of both Mammals and Reptiles, that particular one which is the special distinction of the inferior type. The types inferior to Mammals are *oviparous*, and hence the hemitypic Mammals are semi-oviparous. The type inferior to Reptiles, or that of Fishes, is distinctively *aquatic* and breathes consequently by means of *gills* instead of lungs, and hence the hemitypic Reptiles have gills in the young state.

What then are the characteristics of Reptiles that may have been presented by the inferior or hemitypic Birds? The more prominent distinctions of Reptiles are the following:

(1.) A covering of scales, or else a naked skin, instead of a covering of feathers.

(2.) A terrestrial creeping mode of life instead of an aerial or flying mode.

(3.) Incomplete circulation, and hence, to some degree, cold-blooded, instead of complete, and warm-blooded.

Now, as to the young of the Reptilian Birds, it may be inferred that—

(1.) They were unquestionably unfledged. For this is universal among birds, for a while after leaving the egg. It is quite probable that they were more completely unfledged, or for a longer time, than is common for the young of ordinary birds; for even the adult bird, judging from the Solenhofen specimen, was less completely feathered than usual.

(2.) They were unquestionably *walking* chicks. For Birds in the lower division of the class (*Præcoces* of Bonaparte) have the use of their legs immediately after leaving the egg, and seek their own food. A brood of Reptilian bird-chicks, with long tails and nearly naked bodies, creeping over the ground, would have looked exceedingly like young Reptiles—very much, indeed, as if the eggs of a Reptile had been hatched by mistake. Moreover, these Reptilian Birds were probably not only walking birds when young, but as much so as hens and turkeys are, if not more exclusively so, even when adults; for, in the inferior division of ordinary birds, the species are far inferior as flying animals to those of the superior division, and in some, as is well known, the wings only aid in running.

(3.) But the characteristics which have been mentioned under (1) and (2) are not of fundamental value, like that of the existence of gills in the young of hemitypic Reptiles, or that of the semi-oviparous method of reproduction in Oötocoid Mammals; and it would seem that there must have been some more profound Reptilian characteristic. It is therefore probable that the third distinction of Reptiles stated belonged also to the young Reptilian Bird; that is, it had incomplete circulation, and, hence, an approximation to the cold-blooded condition of Reptiles. The heart may have had its *four* cavities complete, as in Birds, and in Crocodiles among Reptiles; but, in addition, there may have been a passage permitting a partial admixture of the venous and arterial blood, such as exists not only in Crocodiles but also in the young Bird during an early stage in its development. This peculiarity in the vascular system of the young Bird of the present day ceases with the beginning of respiration. But in the Reptilian birds it may have continued on through the early part, at least, of the life of the chick, or until it was fledged.

This conclusion is made to appear still more reasonable by the following comparison of the three obvious methods of subdividing Vertebrates, and the connection therewith of the characteristics of the hemitypic groups. These three methods are—

1. Into *viviparous* and *oviparous;* which places the dividing line between Mammals, and the inferior Vertebrates.

2. Into *warm-blooded* and *cold-blooded*, or those having perfect, and those having imperfect, circulation; which places the line between Mammals and Birds, on one side, and Reptiles and Fishes, on the other.

3. Into *pulmonate* and *branchial*, or those with lungs, and those with gills; which places the line between Mammals, Birds and Reptiles, on one side, and Fishes, on the other.

Now the characteristic of the *first* of these methods of subdivision is that on which the hemitypic group of the first class, or that of Mammals, is based. The characteristic of the *third* is that on which the hemitypic group of the third class, or the Reptilian, is based. Hence, the characteristic of the *second* should be, if the analogy holds, that on which the hemitypic group of the second class, or that of Birds, rests for its most fundamental distinction.

3. *Geological history.*—It has been observed, on page 318, that the Vertebrate subkingdom has well-drawn limits below, instead of tapering downward into Mollusks or Articulates. This feature of the subkingdom is further evident from the fact in geological history that the earliest species of Fishes were not of the *lower* group, that of Teliosts, but of the two higher, or those of Ganoids and Selachians. The Vertebrate type did not originate therefore in the subkingdom of Mollusks, or of Articulates; neither did it start from what might be considered as its base, that is, the lower limit of the class of Fishes; but in intermediate types, occupying a point between typical Fishes and the classes above.

Moreover, the inferior group did not come into existence until the Cretaceous period, in the latter part of geological history, when the Reptilian age was commencing its decline.

In the Devonian age, or closing Silurian, appeared the first Ganoids and Selachians. In the Carboniferous, Reptiles were introduced,—first the inferior Amphibians, and then typical species. Afterward, in the early part of the Reptilian age, as Reptilian life was in course of expansion, there were the first of the Reptilian Birds and the first of the Marsupials or hemitypic Mammals (with probably some typical species of each of these classes). Thus the Vertebrate type, commencing at the point of approximation of Reptiles and Fishes, expanded until each of its higher classes had representative species, before the inferior division of true or typical fishes—Teliosts—came into existence. Afterwards, in the Cenozoic, the true or typical Birds and Mammals had their full expansion.

The Vertebrate type, therefore, not only was not evolved along lines leading up from the lower subkingdoms, but was not, as regards its own species, brought out in lineal order from the lowest upward. The subkingdom has, therefore, most evidently a separateness and a roundness below, so to speak, or an entireness in its inferior limits, which belongs only to an independent system.

We find in the facts no support for the Darwinian hypothesis with regard to the origin of the system of life.

ART. XXIX.—*The Classification of Animals based on the principle of Cephalization;* by JAMES D. DANA.—Number I.

As the principle of cephalization is involved in the very foundation of the diverse forms that make up the animal kingdom, we may look to it for authoritative guidance with reference to the system that prevails among those forms. Some of its bearings on zoological classification have already been pointed out.[1] I propose to take up the subject more comprehensively; and, in the present article, to bring the light of the principle to bear on the relations of the subkingdoms, classes, orders, and some of the tribes of animal life.

It is essential, first, that the methods or laws of cephalization be systematically set forth, that they may be conveniently studied and compared. The following statement of them is an extension of what has already been presented.

As an animal is a *cephalized* organism, (or one terminating anteriorly in a head,) the anterior and posterior extremities have opposite relations. The subdivision of the structure into *anterior* and *posterior* portions has therefore a special importance in this connection. As these terms are used beyond, the *anterior* portion properly includes the head, which is the seat of the senses and mouth, with whatever organs are tributary to its purposes, anterior in position to the normal locomotive organs; the *posterior* portion is the rest of the structure. The anterior is eminently the cephalic portion. The digestive viscera from the stomach backward, and the reproductive viscera, belong as characteristically to the posterior portion.

[1] Expl. Exp. Report on Crustacea, p. 1412, 1855; this Journal, [2], xxii, 14, 1856; xxxv, 67, xxxvi, 1, 1863.

It follows, further, from the cephalized nature of an animal, that its *primary centre of force*, or the point from which concentration and the reverse are to be measured, anteriorly and posteriorly, is in the head, near the anterior extremity of the structure. In an Insect or Crustacean, its position is between the mouth and the organs of the senses—over which part the cephalic mass is located. This is sustained by embryogeny; and also by the fact, that, as the two most fundamental characteristics of an animal are its being sense-bearing and mouth-feeding, the mouth, on descending to the simplest of animals, is the last part to become obsolescent. Only in the inferior Invertebrates is the position of the mouth approximately *central* in the structure, as explained on page 328.[2]

1. *Methods of Cephalization.*

The methods, according to which the grades of cephalization are exhibited, may be arranged under the following heads:

A. *Size* (*force-measured*) *of life-system:* each type, between Man at one extreme and Protozoans at the other, having its special range of variation in this respect.

B. *Functional:* or variations as to the distribution of the functions *anteriorly* and *posteriorly*, and as to their condition.

C. *Incremental:* or variations as to vegetative increment, that is, as to amplitude, and multiplicative development.

D. *Structural:* or variations in the conditions of the structure,—whether (1) compacted, or, on the other hand, resolved into normal elements; (2) simple, or complex by specialization; (3) defective, or perfect; (4) animal-like, or plant-like.

E. *Postural:* or variations as to posture. (Only in Vertebrates.)

F. *Embryological:* or variations connected with the development of the young.

G. *Geographical distribution.*

For greater convenience and uniformity, the methods under these heads are mentioned beyond as they appear when viewed along the *descending* line of grade, instead of the ascending. This is, in fact, the more natural way, since the typical form in a group—the fixed point for reference—holds a position towards the top of the group. The methods, as given, are therefore more strictly methods of *decephalization* than of cephalization; but the former are simply the reverse of the latter.

A. Size (or force) of life-system.

1. *Potential.*—Exhibited in less and less force and size of life-system with decline of grade (and the reverse, with rise of

[2] There may also be one or more *secondary* centres of force; but they are, as regards the subject before us, of compartively small importance. The independent development of the abdomen and cephalothorax in Crustaceans is a case of the kind, as explained elsewhere by the writer. See paper on the Classification of Crustaceans referred to.

grade); as that in passing from the type of Megasthenes (Quadrumanes, Carnivores, Herbivores and Mutilates) to that of Microsthenes (Chiropters, Insectivores, Rodents, and Edentates); or from that of Decapods to that of Tetradecapods among Crustaceans—in which latter case, unlike the former, there is also *retroferent* decephalization; and so, generally, in passing from a higher to a lower type, it being equivalent to passing to a type of smaller and weaker life-system. See further, this volume, pp. 8 and 338.

B. Functional.

2. *Retroferent.*—A transfer of functions backward that belong anteriorly in the higher cognate type.

Under this method, there are the following cases:

a. A transfer of members from the cephalic to the locomotive series; as the transfer of the fore-limbs to the locomotive series in passing from Man to brute Mammals; that of a pair of maxillipeds or posterior mouth-organs to the locomotive series in passing from Insects to Spiders; that of two pairs of maxillipeds to the locomotive series in passing from Decapod to Tetradecapod Crustaceans.

b. A transfer of locomotive or prehensile power and function, more or less completely, from the anterior locomotive organs to the posterior.

c. A transfer of the locomotive function, more or less completely, from the limbs (these often becoming obsolete) to the body, and mainly to the caudal extremity.

Under *b* and *c*, the condition may be described as—

(*a*) *Prosthenic*, (from the Greek *προ*, *before*, and *σθενος*, *strong*,) if the anterior locomotive organs have their normal superiority.

(*b*) *Metasthenic* (from *μετα* *after*, etc.), if a posterior pair is the more important and the anterior are weak or obsolete.

(*c*) *Urosthenic* (from *ουρα* *tail*, etc.), if the posterior part of the body, or the caudal extremity, is the main organ of locomotion.

Ordinary flying Birds are *prosthenic*, while the *Præcoces* (Gallinaceous Birds, Ostriches, &c.), being poor at flying, or incapable of it, are *metasthenic*, and they thus exhibit their inferiority of grade. Hymenopters, Dipters, Lepidopters, &c., among Insects, are *prosthenic*, while Coleopters, Orthopters, Strepsipters, etc., in which the fore-wings (the *elytra*) do not aid in flight, or but little, are *metasthenic*. Fleas, which are degradational species, related to Dipters, have the third or *posterior* pair of legs much the longest and strongest. Among Macrural Crustaceans, the strongest legs are, in the higher species, the *first* pair; in others inferior, the *second;* in others still inferior (the Penæids) the *third* pair.

(See further, for examples, this Journal, [2], xxii, 14, and xxxvi, 1.)

Viewed on the ascending grade, this method is the *preferent.*

3. *Pervertive.*—A subjection of an organ to any abnormal function inferior to that normal to it;—as in the adaptation of the nose of the Elephant to prehension; of the antennæ of many inferior Crustaceans to prehension or locomotion; of the maxillipeds of inferior Macrurans to locomotion; of the forehead in many Herbivores to purposes of defense.

The perverted nose of the Proboscideans is one of the indications of their inferiority to the Carnivores; but it is not necessarily a mark of inferiority among Herbivores themselves, as the faculty of prehension is one of those especially characterizing Carnivores and other higher Mammals, and nearly all Herbivores fail of it.

Viewed on the ascending grade, this method and the following may be included under the term, *perfunctionative.*

4. *Defunctionative.*—Exhibited in the defectiveness or absence of the normal function of an organ;—as in the absence of the function of prehension from the fore-limbs of Herbivores (this prehension in the fore-limbs belonging to the Mammalian type); and that of locomotion mostly from all the limbs in the Mutilates; that of locomotion from the female Bopyrus; that of locomotion from Cirripeds and other attached animals; that of the sense connected with the *second* pair of antennæ (and probably also the *first*, these organs being obsolete) in the Lernæas and Cirripeds, these antennæ being simply prehensile organs in a Lernæa, and constituting the base of the peduncle in an Anatifa.[3]

This degradation and loss of functions is connected often with the *elliptic* and *amplificative* methods of decephalization (see beyond). It is connected with the latter in the Bopyrus, and also in Cirripeds and other attached species.

C. INCREMENTAL.

5. *Amplificative.*—Exhibited in an elongation or general enlargement of the segments or members, and an increased laxness of the parts. Includes the cases—

a. Lengthening, widening, or laxness in the *anterior* portion of the body; the same in the *posterior* portion.

b. An abnormal enlargement of the general structure.

The elongation or enlargement which takes place with decline of grade is mainly *posterior*, it being small anteriorly, and sometimes none at all. In passing from the Brachyural to the Macrural type of Crustaceans, the change anteriorly is princi-

[3] See *Expl. Exp. Report on Crustacea*, p. 1393, and plate 96, where it is shown that the antennæ of the young Anatifa have a sucker-like organ for attachment, and become, in the metamorphosis, the bottom of the peduncle by which the adult Anatifa is attached.

pally in an increased laxness and lengthening of the parts, with little increase in the dimensions of the body anterior to the mouth; while the abdomen (or *posterior* extremity) is enlarged 10 to 50 times beyond the bulk it has in the Crab. Descending from a snail to an oyster, there is diminution anteriorly and great enlargement posteriorly, and the animal is little more than a visceral sac.

In less marked cases of the *amplificative* method, there is only an attenuation or lengthening of the body and limbs, as in many Neuropters, Orthopters, Homopters, wading Birds, etc. The Lepidopters, also, in their very great expanse of wing, exemplify this method. In species that are attached, as the Cirripeds, the young are usually free; and it is only when they begin to outgrow, amplificately, the minute life-system (Entomostracan in the Cirripeds) that they become fixed. As attached animals, they often attain great size.

Viewed on the ascending grade, this method is the *concentrative;* and it is exhibited in the increased abbreviation and condensation of the anterior and posterior members and segments, or of the whole structure.—For examples, see further volume xxii and the present, as already referred to.

6. *Multiplicative.*—Exhibited in an abnormal multiplication of segments or members; as in Myriapods, Worms, Phyllopods, Trilobites, etc. There may be—

a. Simple multiplicative; as in the superior Myriapods, the Chilopods, in which the body-segments, thus multiplied, have each its single or normal pair of members.

b. Compound multiplicative; as in the Myriapods of the Iulus division, or Diplopods (Chilognaths), in which there is a duplication of the pair of legs of a body segment. The name *Diplopod*, adopted by Gervais and some other authors, has the advantage of having thus a dynamical value.

The multiplicative method is, in general, a degradational one. When it affects only subordinate parts of the structure, as the length of the tail of Mammals, or of Reptiles, etc., the forms are not necessarily degradational. But when it affects the general structure, and the types are indefinite in segments, like the Myriapods, Worms, and Snakes (see page 4 of this volume), the forms are degradational. In Mammals, the tail may be said to have indefiniteness of limit; but, since this part is only an appendage to the body and has little functional importance, its elongation cannot properly be regarded as a mark of degradation, although one of inferiority. When, however, the posterior extremity is, in magnitude and importance, a part of the main body structure itself, as in Snakes and Fishes, the case is properly an example of multiplicative degradation.

The abnormal number of segments under the multiplicative method may arise from a self-subdivision of enlarging normal segments, or from additions beyond the range of the normal number. The many joints of the antennæ in Crustaceans of the Cyclops group, the writer has shown to result through the former method, and the multiple segments of Phyllopods may be of the same origin: but there are no facts yet ascertained that would refer the multiplication of segments in Myriapods and Worms to this method.

Viewed on the ascending grade, this method is the *limitative.*

D. STRUCTURAL.

7. *Analytic.*—Exhibited in a resolving of the body-structure, or of an organ, more or less completely, into its equal normal elements, or in a tendency to such a resolution.

A relaxed state of the cephalic power leads to a relaxed and elementally-constituted structure. When this method characterizes strongly the general structure, the form is usually degradational; as in Myriapods, Worms, larves of Insects,—these structures consisting of a series of nearly similar rings, (the normal elements of an Articulate,) without a subdivision into head, thorax and abdomen. Fishes, of the Vertebrate type, are, as nearly as may be, in this elementalized condition. An approximation towards analysis or resolution of the body appears in the absence of the constriction between the head and thorax in Spiders and Crustaceans; and still further, in the absence of the constriction between the thorax and abdomen in the lowest of Spiders, the Acaroids.

Under this method, there is, in no case, among adults or larves, a complete analysis or resolution of the head into normal segments; the closest approximation to it, in Insecteans and Crustaceans, occurs in the Gastrurans (Squilla group) as explained in a note to page 6 of this volume. But here the mandibular and one, two, or more maxillary segments are still united. In an Insect, the head, as stated on page 234 of this volume, contains six normal segments, and the thorax three; and yet the thorax has 3 to 5 times the bulk of the head;—showing a condensation in the head-part equal to 6 to 10 times that of the thorax. Concentration in an animal structure is therefore eminently cephalic concentration, or, in a word, *cephalization,*—the head being the part most condensed, and least liable to occur resolved into its elements.

The analytic method, viewed on the ascending grade, is the *synthetic.*

8. *Simplificative.*—Exhibited in increased simplicity of structure, and in an equality of parts that are normally identical. The cases are—

a. Simplicity from diminished number of internal or external organs for carrying on the processes of life; as in the absence of distinct respiratory organs, or of different parts in the digestive system, etc.; or the union of the sexes in one individual, etc.;—a simplification which reaches its extreme limit among Radiates in the Hydra, and among animals, in the Protozoans.

b. Simplicity from equality in parts normally alike; as, equality in the height of the teeth of some of the earliest of Tertiary Mammals; in the annuli of Worms. This case is related to the analytic.

Viewed on the ascending grade, this method is the *differentiative*, the facts exhibiting which are embraced under the well known law of differentiation or specialization, which is fundamental in all development.

Differentiation internally, as it multiplies and perfects the means of elaborating the structure, is attended with an increasingly higher grade of chemical change, more perfect nutrition, and more complete decarbonization of the blood; and implies, therefore, improvement in all tissues, a more sensitive nervous system, and greater cephalic power and activity. And from the reverse comes the reverse effect.

9. *Elliptic.*—Exhibited in the defectiveness, or absence, of segments or members normally pertaining to the type of the *order* or *class* containing the species. The cases are—

a. Incomplete, or deficient, segments or members, in either the *anterior*, or the *posterior* portion of the body; as with certain teeth in the Herbivores, toes in the foot of the horse, one or two pairs of antennæ in some inferior Crustaceans.

b. Defective, or deficient, senses.

When the deficient parts are only those that are normally deficient in the type of the *order* or *class*, the examples may come under the *simplificative* above. It differs from the *defunctionative* in implying a deficiency not of function only, but of organ or member. The foot of the horse is elliptic, whether viewed with reference to the Animal-type, or the Megasthenic-type. The Fish is elliptic as regards limbs, if considered with reference to the Vertebrate-type, but not so with reference to the Fish-type, unless the fins corresponding to the Vertebrate limbs are wanting.

Viewed on the ascending grade, this method is the *completive.*

10. *Phytozoic.*—Exhibited in a departure from the Animal-type through a participation in structural features of the Plant-type, that is, through a plant-like arrangement of the organs.—The cases are—

a. A radiate arrangement of external organs; as in the Bryozoans and inferior Tunicates.

b. A radiate arrangement of internal as well as external organs; as in Radiates.

c. Perfect, or nearly perfect, symmetry in the radiation, instead of eccentric or irregular forms. Perfect symmetry is most general where the number of rays is based on the numbers 4 or 6 (which, it is to be noted, are multiples of 2 and 3), 4 being the number for the class of Medusæ, and both 4 and 6 occurring in that of Polyps. But if the number of rays is 5, as in the highest of Radiates, the Echinoderms, while examples of perfect symmetry occur, there are many cases of unsymmetrical forms (as in the Spatangi) in which the Radiate type seems to tend to emerge from phytoid towards true animal-like forms. In the regularly radiate, the mouth is central or very nearly so, while in the Spatangi, there is something of the fore-and-aft form of the animal.

Among species under the true animal-type, there are forms showing an approximation to the central position which the mouth has in Radiates. In a Limulus, for example, the mouth-aperture is only one-half less remote from the anterior margin of the body than from the posterior (base of caudal spine). The Limuli are extreme in *amplificative* decephalization and in lowness of grade. Under the *multiplicative* method also, there is something similar in Worms and Myriapods. The head is here strictly at the anterior extremity; but the cephalic force has so feeble control, that joints multiply behind; and in the lowest of Worms, each separate segment is nearly equal in all functions to the cephalic segment. Moreover, in the embryological development of an Annelid, the first segment (with its pair of appendages) that is formed after the appearance of the head is not the anterior one close to the head, but the *eighth* (or one near this); and from this point the rings form in succession posteriorly, and also towards it from the head; as if, in these *multiplicate* species, there was a *secondary centre of force* distant from the front which preponderates over the *primary* one.

This method viewed on the ascending grade is the *holozoic*, (from ὅλος all, and ζῶον animal); it is exhibited in a rise from the plant-like type to the true animal-like type.

E. Postural.

11. *Postural.*—Exhibited in an increasing proneness in the position of the nervous system—the extremes being *verticality* in Man, and *horizontality* in the Fish.

F. Embryological.

12. *Prematurative.*—Exhibited in precocity of young or larves. Thus, the chicken, as soon as born, runs about and seeks its own food, while the young of those Birds which belong to the superior group,—the true flying Birds—remain helpless until able to fly; a fact recognized in Bonaparte's classification of Birds. So the young colt or calf (Herbivorous) is on its legs almost as soon as born; but the young kitten (Carnivorous, and higher in type) is for a considerable time helpless.

Prematurity has often been recognized as evidence of low development and low rank; and the following is the explanation of it. When an animal has reached the condition required for locomotion and for the care of itself, it has already the essential faculties of an adult; and although these faculties of locomotion and self-feeding are of comparatively low grade, the animal possessing them is approximately mature in its cephalic forces, and afterwards rises but little with growth. Prematurity hence involves inferiority. The pupa-state of an Insect is a means of higher development the more perfect its inactivity. For this complete rest allows all the forces of the individual to be concentrated on the internal processes, and favors, therefore, that cephalic growth which makes a special demand on these forces; while in an *active* pupa (or rather the larve that passes through no pupa-state), activity, whether that of locomotion, or of digestion, constantly exhausts force; and only the balance, not thus run away with, goes towards the maturing process. With such an open outlet of force, the animal may mature physically, that is, grow and perfect its outer structure; but cephalically, or, in all those points of structure, as well as psychical powers, that are connected with superior cephalic development, it makes little advance.

Hence, (*a*) those insects whose larves are essentially like the adults and undergo no metamorphosis are inferior in type,—as generally so recognized.

Again, (*b*) those Insects (as most Hymenopterous) whose larves are footless grubs are superior in type to those (as the Lepidopterous) whose larves are most highly developed and active.

Viewed on the ascending grade, this method is the *permaturative.*

13. *Gemmative.*—Exhibited in multiplication by buds. Budding may produce—

a. Perfect individuals, capable of egg-production.

b. Individuals capable only of budding, and giving origin to a perfect egg-producing individual as the last of a series of buddings.

c. Caducous, or persistent buds; the *latter* leading to compound forms, either branching, lamellar, or massive.

This power of reproduction by buds occurs in many Worms, both superior and inferior; in Bryozoan and many Ascidian Mollusks; in Polyps and many other Radiates. The production of persistent buds is the lowest grade, and is common in the budding Mollusks and Radiates, but not the Articulates. Among budding Articulates, case *b* appears to be of lower grade than case *a.*

This method is allied to the *multiplicative*, p. 325. It is also *phytozoic* (p. 327), or a plant-like feature in animal life.

14. *Genetic.—Number of young or eggs.*—As is well known, there is a mark of grade in the number of eggs or young produced at a single period or in a given time—the number, other things equal, being inversely as the rank or grade of the species.

15. *Thermotic.—Temperature required for embryonic development.*—Another mark of grade is afforded by the temperature required for egg-development:—for, in general, the higher the temperature, the higher the grade. Thus, the eggs of Birds require heat above ordinary summer heat, while those of Reptiles do not. The embryos of Mammals require still higher and more uniformly continued heat until their maturity, the Oötocoids alone excepted, in which birth is premature. The eggs of some Hymenopterous Insects mature inside of the larves of other Insects, where they are never exposed to a temperature of 32° F.; while those of ordinary Lepidopters and many other species mature in the summer heat, and may stand a temperature below 0° F.

The necessity of a higher temperature indicates, ordinarily, that the chemical processes in the vital economy are of a higher or more delicate character, or those required for a higher grade of cephalization.

G. Geographical Distribution.

16. *Habitational.*—(1.) *Terrestrial species higher than aquatic.*—This law, announced by Agassiz, is also directly dependent on the conditions determining the grade of cephalization.

a. In the case of *aquatic* species, the ova, as well as the adult animals, are bathed in a liquid that penetrates to the interior, and dilutes, to some degree, the nutrient or developing fluids; and, under such circumstances, the grade of chemical or vital evolution cannot be as high as in the atmosphere. The germ must therefore be one of an inferior kind. Aquatic animals are, in an important sense, *diluted* animals.

b. Again, *terrestrial* species whose ova are hatched in water, or whose young are aquatic, are for the same reason inferior, as a general rule, to those whose ova are hatched on the land.

Aquatic development or life is one of the most important marks of low grade. Among embryological characteristics, it has often a profounder value than prematurity. The *inferior division* of a *class*, *order*, *tribe*, and even *subordinate group*, is often one consisting either of *aquatic* species, or those that are *semiaquatic* (aquatic in habit though not strictly so in mode of life, or aquatic in the young state when not in the adult).

(2.) *Living* (a) *in impure waters, or those abnormal in condition; or* (b) *in deficient light, as in shaded places, or the ocean's depths, a mark of inferiority.*—Muddy waters, or salt waters excessively saline as in some inland lakes, or waters only brackish, are here included.

But *oceanic waters*, although saline, are not properly impure. Of the subkingdoms and the classes containing aquatic animals, the *highest* groups are those of *marine* waters. Thus, the highest of Mollusks, the Cephalopods, are marine; the highest of Radiates, the Echinoderms; the highest of Fishes, the Selachians; of Crustaceans, or the Maioid or Triangular Crabs; of Worms, the Dorsibranchs; of Acalephs, all but the Hydroids are marine; while *all* species of Echinoderms and Polyps are marine. Among the subordinate groups there are some fitted particularly for fresh water. Types that belong to fresh water sometimes have inferior species in brackish or salt water; and those that belong to salt water sometimes have inferior species in brackish or fresh water.

(3.) *Species of cold climates inferior to those of warm.*—According to the 15th canon, the highest oviparous animals should be tropical species; but not necessarily so the viviparous Mammals, since, with them, the requisite temperature for embryonic development is obtained within the parent.

An exception to this, as regards oviparous species, is afforded by Crustaceans; for, as shown by the writer, the highest kinds, the Maioid or Triangular Crabs, have their fullest development in the cooler temperate zone.

(4.) *Having a wide range with regard to any of the earth's physical conditions, as* (a) *climate,* (b) *height,* (c) *oceanic temperature,* (d) *oceanic depth,* (e) *hygrometric conditions, etc., commonly a mark of inferiority.*—For, if the development of a high order of cephalized life requires rest for a while in the young, as, for example, the nursing time in the higher Mammals and Birds and the Pupa-state in Insects, and also an absence from diluting or impure waters and the presence of the full light of the sun, it should also equally demand precise or narrowly restricted limits in all physical conditions, these being essential to the more refined or delicate chemical or vital processes. Man is the chief exception to this law,—and for the reason that he is not simply in and of nature, but also above nature, and has the will and power to bring her forces under subjection, overcoming the rigors of climate and subjugating other inimical agencies by his art. Protophytes and Man are the only species that have the range of the world—the one because so low, the other, so high. The Dog accompanies Man in his wide wanderings: but only through the virtue which is in Man, who provides the artificial heat, protection and food his brute attendant needs. Even the human race dwindles in extremes of climate, either hot or cold.

Recapitulation.—The following are the names of the several methods of cephalization pointed out, both those based on the descending and ascending lines of grade.

	Descending.	Ascending.
A. Size of Life-system,	1. Potential.	1. Potential.
B. Functional,	2. Retroferent.	2. Preferent.
"	3. Pervertive.	3. } Perfunctionative.
"	4. Defunctionative.	4. }
C. Incremental,	5. Amplificative.	5. Concentrative.
"	6. Multiplicative.	6. Limitative.
D. Structural,	7. Analytic.	7. Synthetic.
"	8. Simplificative.	8. Differentiative.
"	9. Elliptic.	9. Completive.
"	10. Phytozoic.	10. Holozoic.
E. Postural,	11. Postural.	11. Postural.
F. Embryological,	12. Prematurative.	12. Permaturative.

The remaining terms fall into both columns.

With *ascending* grade, the changes are mostly *concentrative;* with *descending*, they are *diffusive* or *decentrative.*

2. *Additional Observations.*

1. *Typical, Degradational and Hemitypic forms.*—Typical species are those within type-limits, and *degradational* those outside of the same.[4] But, as groups of all grades have each their own type and type-limits, species may be typical in one relation, and degradational in another; as Fishes, for example, while degradational Vertebrates, have still their own type and type-limits, the Teliosts being the typical Fishes, or those within these limits.

The characteristics of a type, in any case, are those fundamentally distinctive of the group. As to that of the animal kingdom at large,—we observe that an animal is (1) a fore-and-aft, (2) cephalized, (3) forward-moving organism. The type-idea is hence expressed in a structure having (1) fore-and-aft and dorso-ventral polarity; (2) a head at the forward extremity containing the seats or organs of the senses, as well as the mouth and mouth organs; and (3) the power of locomotion, if not also limbs for the purpose. Consequently Radiates, as they fail in the first criterion, are not within type-limits; neither are any *attached* species of animal, and only in a partial degree species without limbs for locomotion.

Again, the Vertebrate-type, in addition to having the characteristics of the animal type and the vertebrate structure, is essentially terrestrial, and, therefore, the requisite limbs and structure for terrestrial life are in the type-idea. Fishes are therefore outside of type-limits, or are degradational species.

The Mammal-type, the highest under Vertebrates, in addition to the characteristics of the Vertebrate type, has that of being viviparous in its births, embracing under this quality, that of sustaining the embryo by placental nutrition until its maturity

[4] The term *degradational* has no reference to any method of origin by degradation: it implies only that the forms so called represent or correspond to a degraded condition of the type.

(as is not true of the oviparous); and with this there is also that of sustaining the young for a while after birth, by suckling. Hence, the Oötocoids, in which there is only imperfect placental nutrition and birth is premature, and there is an approximation thus to oviparous species, constitute a degradational type.

The Megasthene-type, under Mammals, has its degradational group in the Cetaceans or Mutilates, which fail mostly of limbs and are aquatic species; and the Carnivore its degradational group in the Seal and related Pinnipeds. The latter have the type-structure of the Carnivores; while the Mutilates have the type-structure of neither Carnivores nor Herbivores, and are therefore an independent type under the division of Megasthenes.

Again, the Bird-type, in addition to the characteristics of the Vertebrate-type, embraces features adapting the animal to flying, as feathers and wings; perfect circulation; and also a vertebral column which is posteriorly limitate, instead of one admitting of a caudal elongation,—somewhat as Insects and Spiders are *closed* types behind, in contrast with the *multiplicate* Myriapods. Hence the Reptilian Birds, having *indefinite* posterior elongation, and some other Reptilian characteristics, are outside of type-limits. So, again, under the subdivisions of Birds, species that have the wings unfledged or but half-fledged, and which, therefore, cannot lead an *aerial* life, are degradational; and species that have the feet imperfectly digitate by their being web-footed, and which therefore lead a *semiaquatic* life, are semi-degradational in the group to which they may belong.

These examples are sufficient to illustrate the uses of the words typical and degradational.

It is of the highest importance, for the correct classification of species, that in all cases it should be rightly determined whether a degradational genus is degradational to the *family* to which it belongs, or to the *tribe*, or *order*, or to a still higher division. Although Seals and Whales are similarly adapted to the water, it is plain, to one familiar with the species, that the former are degradational Carnivores, and the latter degradational Megasthenes, as stated above. But like cases come up in every part of the animal kingdom, and close study is necessary for a true decision. The first preliminary towards such a decision is a clear idea of the class-type, order-type, tribe-type or subordinate type under which the genus or group falls.

The term *hemitypic* has been shown in the preceding paper to imply, in general, a grade of the degradational. But, in some groups, as in the class of Fishes among Vertebrates, it is applicable to cases which are not typical because of their being intermediate between the type of the group and a *superior* type or types (p. 317).

Typical groups, or, more properly, the groups above the degradational, may be of several grades. Thus, under Vertebrates, the classes of Mammals, Birds and Reptiles, represent different grades of Vertebrate types, and the grades may be designated, in order, *Alphatypic*, *Betatypic*, *Gammatypic* (from the first three Greek letters α, β, γ). Under Mammals, also, there are three grades, those of Man, Megasthenes, and Microsthenes; then, below these, the hemitypic or degradational Oötocoids. Under tribes, families and genera, the number of grades may be large.

Degradational subdivisions are strictly *hypotypic*, or *below* the typical range.

Typical subdivisions, or those above the degradational, are not, in all cases, *true* typical, as well exemplified by the orders of Fishes; the Teliosts alone being true typical, and the Ganoids and Selachians, called *hemitypic* above, being properly *hypertypic*, or *above* the typical range. Another example of this is afforded by the subdivisions of Megasthenes. Carnivores and Herbivores are different grades of the *true typical*, the former the more perfect, or *eutypic;* while the Quadrumanes or Monkeys are *hypertypic*, being an *intermediate* type between the typical Megasthenes and Man; and the Mutilates (Cetaceans, etc.) are *hypotypic*. Among the Microsthenes, the Chiropters or Bats are *hypertypic*, the Insectivores and Rodents *true typical* of two grades, and the Edentates *hypotypic*.

Among the subdivisions of Mammals there are *three* grades of true typical; and, of them, Man is *archetypic*, as he has been styled, being the *one perfect* type.

Degradational forms may be classed under three heads, as follows:

1. *Degenerative;* in which the forms are thoroughly animal in type. The methods of decephalization which lead most commonly to degenerative forms are the analytic, multiplicative, elliptic and defunctionative.

2. *Hemiphytoid;* when, without an *internal* radiate structure, the species are (*a*) attached to a support, like plants (see *defunctionative* method, p. 324); *b*, budding (*gemmative*, p. 329); *c*, radiate externally (*phytozoic*, case *a*, p. 327).

The externally radiate structure is a lower grade of hemiphytoid degradation than either being attached, or gemmate.

3. *Phytoid* (from *φυτον, a plant*); when the structural arrangements are *internally*, as well as externally, radiate (Phytozoic, case *b*).

As Radiates have no limbs and but imperfect senses, the higher grades among them are manifested most prominently in the conditions of the nutritive system. Some of them (the Echinoderms) are superior, as animals, to the lower *hemiphytoid* species such as the Bryozoans.

2. *Further exemplifications of the preceding methods of cephalization.*—In order to give greater clearness to the explanations which have been made on the preceding pages, the application of the terms expressing the methods of cephalization to grades of species may here be further illustrated.

In the class of Crustaceans, the distinction between the 1st and 2nd orders, or Decapods and Tetradecapods, depends on case *a* under the *retroferent* method—a transfer of members from the cephalic to the locomotive series. In connection with it, there is also an exhibition, to some extent, of the *analytic* method, more of the segments of the body in the latter being free, and all, more regular or normal in form.

Under Decapods, the difference between the 1st and 2nd tribes, the Brachyural and Macrural, depends mainly on the *amplificative* method—there being in the latter, by an abrupt transition, greater length and laxness before and behind. Under the *analytic*, also, the lengthened abdomen in the Macruran has its normal number of segments and members.

Among the subdivisions of *Macrurans*, the *retroferent* method appears prominently in the transfer of force from the *first* pair of legs to the *second* and, among the lower genera, to the *third* pair (see p. 323); the *amplificative*, in the length of antennæ in some families, and in the length of abdomen as compared with that of the cephalothorax in others; the *elliptic*, in the absence of posterior cephalothoracic members, and also the obsolescence of the abdominal members in many Schizopods or degradational Macrurans; the *pervertive*, in the outer maxillipeds taking the form and functions of feet, as in many inferior Macrurans.

Under *Tetradecapods*, the difference between the 1st and 2nd tribes, or Isopods and Amphipods, depends on the very same methods as that between the 1st and 2nd under the Decapods: that is, on the *amplificative*, as shown in the greater length of cephalothorax and the elongated abdomen, and on the *analytic*, the lengthened abdomen having its normal segments and approximately normal members.

Under the Amphipods, the *amplificative* method is variously illustrated; the *elliptic* in the obsolescent abdomen of the Caprellids, as well as in the absence or obsolescence in many species of two pairs of thoracic legs.

Again, in the class of Insecteans, the distinction between the 1st and 2nd orders, or Insects and Spiders, depends on *case a* under the *retroferent* method (see this vol., p. 3); and, in connection, there is an exhibition of an incipient stage of the *analytic*, the head and thorax in Spiders constituting a single mass (p. 326).

Under Insects, the difference between the two highest divisions, *Prosthenics* and *Metasthenics*, depends on *case b* under the *retroferent* method, or a transfer of the flying function mainly or wholly to the posterior pair of wings. And the third is a degradational group, in which, by the *amplificative*, *analytic* and *elliptic* methods, the species (Lepismæ, etc.) are wingless and larve-like.

Among Herbivores, the Elephant shows superiority (1) in having, as in Carnivores, the teeth (its tusks) for defensive weapons; (2) in having, as in Carnivores, the power of prehension, a quality, however, transferred from the teeth to one of the organs of sense, the nose; this organ of prehension also aids in defense; (3) in having the normal number of toes; (4) in having pectoral mammæ, as in the highest Megasthenes or Quadrumanes, the highest Microsthenes or Bats, and also in Man. The great size is not a mark of overgrowth and inferiority, for the animal is neither stupid nor sluggish. The Ruminants are inferior to the Elephant in having, not an *inferior* organ of sense, but the forehead, or typically the most important part of the head, perverted to use for self-defense; and also in other ways. Among Ruminants, the Stag or Elk-type shows superiority to the Ox-type, in (1) its more compact and smaller head; (2) its less magnitude *posteriorly*; (3) its limbs adapted to fleet motion; (4) its fore-limbs adapted for climbing and clinging, giving them a special *prosthenic* character and great superiority to those of the Ox. The Horse-type shows inferiority to the Elephant-type, in (1) its long head and neck (amplificate); (2) its one-hoofed foot; (3) its being *metasthenic*, the hind legs serving as the principal organs of defense; and also in the characters mentioned above.

The discussion of the subject of classification beyond, will be found to be a continued exemplification of the laws of cephalization, and we refer forward for additional elucidation.

3. *The forms, resulting from the expression of the same law of cephalization in diverse groups, often similar; and hence come some of the analogies between groups, or their osculations.*—It is apparent that the grades of cephalization may have expression in *any* division of the animal kingdom, and that hence may come *parallel* results as to form. For example, there may be cases of *amplificative* decephalization—or of long-bodied or long-legged species—in the different orders or tribes of Insects; and, when so, the species, in these different groups thus characterized, will be, in a sense, representatives of one another, and the groups will "osculate" at such points. One example is that of Orthopters and Neuropters through the Mantids in the former and the Mantispids in the latter; also, that of Dipters and Neuropters, through the slender Tipulids of the former. The same may be exemplified among the orders of Birds. The degradational feature, for example, of webbed feet, or that of defective wings may characterize the inferior species of different subdivisions, and so produce

osculant groups; so may the *amplificative* feature of great length of limb and neck, the Herons among the Altrices, thus representing the Grallatores among the Præcoces.

The osculations or close approximations of classes, orders, tribes, etc., are thus often connected with like expressions of the methods of cephalization.

4. *Forms resulting from high and low cephalization sometimes similar.*—High and low cephalization often lead to similar forms, the former through cephalic *concentration*, the latter through cephalic and general feebleness; just as a thing may be small, when the material is condensed or concentrated, and equally small when dilute and there is little of it. Thus the Crab has a very small memberless abdomen, from a contracting of the sphere of growth through concentrative cephalization; on the other hand, the Schizopod has a memberless abdomen, through a limitation of the sphere of growth resulting from mere feebleness in the life-system. The abbreviated memberless abdomen of the Caprellid and the obsolescent spine-like abdomen of the Limulus are other examples among Crustaceans of this *elliptic* decephalization. See also page 6 of this volume for a comparison of a Limulus and an Insect. The Butterflies have very large wings through the amplificative method; but some inferior nocturnal species have the wings narrow through inferiority of grade, on the above principle, and not properly through concentration and elevation.

There is, in general, no danger of confounding the two cases, because the accompaniments in the structure of the superior species, as well as those of the inferior, commonly indicate their true relations, at once, to the mind that is well versed in the department of zoology to which the species belong. But there are many cases in which it is not safe to make a hasty decision.

5. *Uniformity of shape and size in any group greater among the higher typical species than among the lower typical or degradational species.*—On the higher typical level in any class, order, tribe, &c., the type is represented generally in its greatest number of species, and always under the least extravagance of form and size. Thus, Insects, the higher typical division of Insecteans, are vastly more numerous in species, and less diversified in size, form and structure, than Crustaceans or Worms. And, under Insects, the Hymenopters have little variety of form of body, and form or size of wings, compared with the Neuropters, Lepidopters, Homopters and even the Coleopters; and the Coleopters, little compared with the Orthopters. The fantastic shapes, in all cases, occur in the inferior typical or the degradational groups. In these, cephalization is of low grade, and as a consequence of this relaxing of the system, or its inferior concentration, the forms run off into varied extravagances.

6. *Classification hereby placed on a dynamical or sthenic basis.*— The laws of cephalization, as is apparent from the explanations which have been made, are based upon the idea that an animal is centralized force; and that the degree of concentration of this force may be exhibited in the structure; that, consequently, the various grades of species or groups become apparent, to some extent, through size and form, and their determination is thus, in part, a matter of simple measurement. Dimensions or spatial conditions have a relation to force in the animal kingdom as well as in that of the celestial spheres.

Rank or grade are thus brought to the rule and plummet, and classification, thereby, has a dynamical basis. The distinctions between groups have a dynamical or sthenic character, and all subdivisions in classification, when thoroughly understood, will have recognized sthenic relations.

It must, however, be kept in mind that the element of *size*, when used in the application of the principle, or as a mark of superiority, is not *absolute* size. For it is one of the laws of life that vegetative growth may enlarge a weak life-system to gigantic dimensions. Thus, the life-system of an Entomostracan takes great magnitude in a Limulus; of a Tetradecapod, in a female Bopyrus; of an Edentate, in a Megathere; of a Mutilate, in a Whale. The body of a Crab has 50 times the dimensions of that of an Insect; and its head probably 100 times that of the head of an Insect, although an Insect is the superior species.

Neither is mere muscular strength an indication of grade; for there is force used in sustaining the structure which is greater the higher the organism, and, superior to this, there is sensorial and other cephalic force. Were we to base our comparison between the grade of life-system in a Crab and that of a Bee on the ground of muscular strength, we should go far astray; and still wider from the mark, were we to rely on the relative sizes of the cephalic nervous masses; for this nervous mass in a common Crab (*Maia squinado* of European seas) has 25 to 30 times the bulk of that in a Bee. Man yields in size and muscular strength not only to the higher Megasthenes, but to the Whales or lowest; and the brain in the Elephant and the Whale outweighs his. The Megathere, although much more powerful than a Rodent, has not, on this account, as his structure and habits show, any claims to a place above the lowest of Microsthenes.

The terms *Megasthenes* and *Microsthenes* are not to be understood as signifying large Mammals and small Mammals, but Mammals of *strong life-system* and *weak life-system*. Comparing the typical species of Megasthenes[5] with those of Microsthenes,

[5] These orders of Mammals, (see last volume of this Journal, page 70, and page 342, beyond), make parallel series—the Chiropters or Bats of the Microsthenes representing the Quadrumanes of the Megasthenes, the Insectivores representing the Carnivores, the Rodents the Herbivores, and the Edentates the Mutilates.

there is some correspondence between average size of structure and strength of life-system. But a comparison of the typical of the former with the degradational of the latter leads to very false results.

An approximation to the right ratio is obtained from a comparison of the degradational species of each; but this is of no importance in its bearing on the question, since vegetative growth is apt to give the greatest proportional enlargement to the *lowest* species.

These facts teach that relative size of body, or of brain, is no necessary test of relative rank. The ratio, in *bulk*, of 1 : 3 between the brain of an average Man and that of a gorilla tells nothing of the actual difference of life-system, or of brain-power. At page 70, in the last volume of this Journal, the relative *lineal* dimensions of Microsthenes and Megasthenes is estimated at 1 : 4, which gives, for the relative *bulk*, 1 : 64. If this be the typical ratio between the life-systems of the highest Microsthenes and highest Megasthenes, surely that between the highest Megasthenes and normal Man—he constituting a *distinct order* (see p. 341)—must be at least as great.

The same ratio of 1 : 4, as shown by the writer, is that for the mean size, lineally, of Tetradecapods and Decapods, under Crustaceans. In two cases, then, consecutive orders differ by a like ratio, or approximately so, in dimensions. As has been remarked, deductions from mere size may be very erroneous; yet there is no reason, in either of the above cases, to suppose the ratio of life-systems less than that thus indicated. May not, therefore, some similar ratio exist between other analogous consecutive orders, where size does not manifest it,—as, for example, between Spiders and Insects? And is not the ratio a much greater one between the highest of Insecteans and highest of Crustaceans, since these subdivisions of Articulates are not orders but *classes?* Important results may flow from following out the idea here touched upon.

After the preceding explanations, I proceed to exhibit some of the relations of the higher groups in zoological classification, as they appear in the light of this subject of cephalization.

3. *Classification of Animals.*

1. *Subkingdoms.*—Of the four subkingdoms, first recognized by Cuvier and since by most zoologists, the Vertebrate, Articulate and Molluscan are typical, or of the true *animal-type*, and the Radiate is degradational, being *plant-like* in type. Using the terms alphatypic, betatypic and gammatypic *simply as a numbering of the grades* of types (see p. 334), their relations are as follows:

Alphatypic, - - - - -	1. Vertebrates.
Betatypic, - - - - -	2. Articulates.
Gammatypic, - - - - -	3. Mollusks.
Degradational, - - - - -	4. Radiates.

An important dynamical distinction between Mollusks and Articulates has been suggested on page 10 of this volume.

2. *Classes of Vertebrates, Articulates, Mollusks and Radiates.*—(1.) The classes of *Vertebrates* are four (see page 319), namely, Mammals, Birds, Reptiles and Fishes,—three of which are typical, of different grades, parallel with the above.

(2.) The classes of *Articulates* are but three, Insecteans, Crustaceans and Worms. This is illustrated at length at page 3 of this volume, where it is shown that the three divisions of Insecteans, namely Insects, Spiders and Myriapods, are distinguished by characteristics analogous to those which separate the divisions of Crustaceans,—Decapods, Tetradecapods and Entomostracans. The facts on this point are briefly presented on page 335. Insects and Spiders do not, in fact, differ more widely in external form or in structure than Decapods and Tetradecapods.

Insecteans and Birds express in different ways the same type-idea,—that of aerial life, Birds being flying Vertebrates and Insects flying Articulates; and, in accordance, they are of the same grade of type, both being *betatypic.* This follows, further, from the fact that there are but two grand divisions of Insecteans above the degradational division, that of Worms.

(3.) Among *Mollusks*, there are two well-characterized classes, the *first* including the *ordinary* Mollusks; the *second*, the *Ascidioids*, or the Brachiopods and Ascidians, which are mostly attached species and thus hemiphytoid. Besides these, there are the Bryozoans, which either make a third division under the Ascidioids (Edwards having long since pointed out their relations to the Ascidians); or they constitute a *third* class of Mollusks, characterized by being polyp-like both in external appearance and in being attached, and hence doubly hemiphytoid.

(4.) The *Radiates* are all degradational in their relations to the animal-type. But under the *Radiate-type*, the species of the first two classes are within type-limits, while those of the third are degradational, since almost all are attached and very inferior in type of structure, being the most phytoid of phytoid animals. The grades of structure as marked in the digestive system are as follows: (1) having approximately normal viscera, as in Echinoderms; (2) having, for the digestive system, only a stomach cavity, with vessels, imbedded in the tissues, radiating from it, as in Acalephs; (3) having, for the same, no system of viscera or radiating vessels; but only a central stomach surrounded by a cavity more or less divided at its sides by partitions, as in Polyps.

The following table presents the relations and the parallelisms of these classes, and of each to the subkingdoms.

	Subkingdoms.	Vertebrates.	Articulates.	Mollusks.	Radiates.
α.	Vertebrates.	Mammals.	———	———	———
β.	Articulates.	Birds.	Insecteans.	Ordinary.	Echinoderms.
γ.	Mollusks.	Reptiles.	Crustaceans.	Ascidioids.	Acalephs.
D.	Radiates.	Fishes.	Worms.	Bryozoans?	Polyps.

Arranging the divisions according to the relations of the groups to the *animal-type*, instead of the special type of each class, the table takes the following form:

	Subkingdoms.	Vertebrates.	Articulates.	Mollusks.	Radiates.
α.	Vertebrates.	Mammals.	———	———	———
β.	Articulates.	Birds.	Insecteans.	———	———
γ.	Mollusks.	Reptiles.	Crustaceans.	Ordinary.	———
a. D.	———	Fishes.	Worms.	Ascidioids.	———
b. "	———	———	———	Bryozoans.	———
c. "	Radiates.	———	———	———	Echinoderms.
d. "	———	———	———	———	Acalephs.
e. "	———	———	———	———	Polyps.

The letters *c*, *d*, *e*, stand for different grades of phytoid degradational, *b*, hemiphytoid, and *a*, degenerative. The blank interval between Mollusks and Radiates is filled up by the inferior divisions of the higher subkingdoms.

We may now consider the subdivisions under some of the classes; and first, those of Vertebrates.

3. *Higher subdivisions of the class of Mammals.*—The higher subdivisions of the class of Mammals are four in number: Man, Megasthenes, Microsthenes, and Oötocoids, as explained in the preceding volume of this Journal, p. 70 Man is shown to stand apart from the Megasthenes on precisely the same characteristic that separates the two highest orders under the classes severally of Insecteans and Crustaceans; for, in passing from Man to the brute Mammals, there is a transfer of the forelimbs from the cephalic to the locomotive series.

Moreover, a study of the Vertebrate skeleton has shown that the forelimbs in the Vertebrate-type, as well explained by Professor Owen, are *cephalic appendages*, being normally appendages to the posterior or occipital division of the head. In the Fish, these forelimbs (the pectoral fins) have at any rate an actual *cephalic position* (back of which position they are thrown, by displacement, in other Vertebrates). Now, in Man, they are not only cephalic in normal structural relations, but *cephalic* also in *use*. The transfer of these cephalic organs to the locomotive series, by which the brute structure is made, is a manifest degradation of the type. Man is thus the only Vertebrate in which the Vertebrate-type is expressed in its perfection, and therefore occupies *alone* the sublime summit of the system of life.

Three of the orders of Mammals, namely, Man, Megasthenes, and Microsthenes, are typical, of different grades, and one, Oötocoids, as explained on pages 316 and 332, is semidegradational.

For remarks on the *subdivisions* of Megasthenes and Microsthenes, see the articles above referred to, and also p. 338, preceding.

The Oötocoids may be divided into three groups—a *megasthenic*, a *microsthenic* and a *degradational;* the *first* to include the genera Phalangista, Dasyurus, Macropus, Diprotodon, etc.; the *second*, Perameles, Didelphys, Phascolomys, Echidna, etc., or Marsupial Insectivores, Rodents and Edentates; the *third*, Ornithorhynchus.

The following table presents to view the subdivisions of Mammals and its orders. Under Oötocoids, the relations of the two higher groups are indicated by the above adjectives, without giving them special names.

	Mammals.	Megasthenes.	Microsthenes.	Oötocoids.
α.	Man.	Quadrumanes.	Chiropters.	———
β.	Megasthenes.	Carnivores.	Insectivores.	Megasthenic.
γ.	Microsthenes.	Herbivores.	Rodents.	Microsthenic.
D.	Oötocoids.	Mutilates.	Edentates.	Ornithorhynchs.

4. *Higher subdivisions of the classes of Birds, Reptiles and Fishes.*—(1.) In the class of *Birds*, there are three grand divisions: the first two, as recognized by Bonaparte, are the *Altrices* (Rapacious birds, Perchers, &c., and other birds that feed their young until they can fly), and the *Præcoces* (or the Gallinæ, Anseres, Ostriches, etc., which feed themselves as soon as hatched). The third includes the Reptilian Birds or Erpetoids (p. 317). The terms *Pterosthenics* and *Podosthenics* apply equally well with *Altrices* and *Præcoces* to the two higher divisions of Birds, as explained on page 323, and have an advantage in their direct dynamical signification.

The type of ordinary Birds (or Pterosthenics and Podosthenics) is stated on page 333 to be essentially *limitate*, like that of Insects, while the type of Erpetoids is *multiplicate*, like that of Myriapods or of ordinary Reptiles; so that the relation of Erpetoids to the higher division of Birds is in an important respect analogous to that of Myriapods to the higher division of Insecteans.

(2.) In the classification of *Reptiles* there are three prominent types of structure recognized by Erpetologists; (1) that of the Chelonians; (2) that of the Lacertoids (including Saurians, Lizards, Snakes); and (3) the degradational or hemitypic one of Amphibians. It is now well known that Snakes and Lizards are alike in type of structure, the two groups graduating almost insensibly into one another, some species ranked as Lizards being footless like the Snakes. The Snakes constitute the degradational group under the Lacertoids. The Amphibians, constituting the third order, are on the same level with the Erpetoid Birds and the Oötocoid Mammals, as presented in the following table.

The three orders of Reptiles—Chelonians, Lacertoids and Amphibians—make a parallel series with the three lower classes of Vertebrates; the Chelonians representing the Birds, to which they approximate in some points, besides being betatypic like them; the Amphibians representing the Fishes, with a still closer approximation between the two; while the Lacertoids are the typical Reptiles. The Chelonians might be viewed as *hemitypic* Reptiles; not *hypotypic* like the Amphibians, but *hypertypic*, like the Selachians and Ganoids among Fishes.

(3.) *Fishes* are all degradational species in their relations to the animal-type. The two higher groups, or those of Selachians and Ganoids as already explained (p. 334), are *hypertypic*. The third, including Teliosts, is *typical* if viewed with reference to the Fish-type. Below these, the Dermopters or Myzonts, (including Amphioxus, Myxine, etc.) constitute an inferior *hypotypic* or degradational group,—that is degradational in its relations to typical Fishes (p. 332). Thus *typical* Fishes are gammatypic in their relations to other Vertebrates, while the alphatypic and betatypic groups are *hypertypic* orders.

The following table exhibits the relations of the orders in the classes of Birds, Reptiles and Fishes; and, for comparison, those of Mammals are added.

	Mammals.	Birds.	Reptiles.	Fishes.
Alphatypic,	Man.	———	———	Selachians.
Betatypic,	Megasthenes.	Altrices, or Pterosthenics.	Chelonians.	Ganoids.
Gammatypic,	Microsthenes.	Præcoces, or Podosthenics.	Lacertoids.	Teliosts.
Hemitypic, or Degradational,	Oötocoids.	Erpetoids.	Amphibians.	Dermopters.

We pass now to Articulates.

5. *Subdivisions of the classes, Insecteans, Crustaceans and Worms into Orders.*—(1.) The higher subdivisions in each of the classes, *Insecteans* and *Crustaceans*, are three in number, none existing above the betatypic grade, which is that of Articulates among the subkingdoms, and of Insecteans among Articulates. (See page 7.)

(2.) *Worms* are of four types of structure. First, *Annelids*, or *typical* Worms, including the Branchiates, Abranchiates, and Nematoids—the last the degradational group, and showing this in the obsolete body-articulations and some internal characters.—Second, *Bdelloids*, or *Molluscoid* Worms, including the Hirudines or Leeches, Planarians and Trematodes; characterized by obsolescent or obsolete body-articulations, and by often wanting the nervous ganglia excepting the anterior; by usually a Gasteropod-like breadth and aspect, an *amplificate* feature; by being in general *urosthenic*, even the highest having a caudal disk for attachment; and in an up-and-down movement of the body in locomotion, *Mollusk-like*, instead of the worm-like lateral movement of the Annelids. The fact of this mode of movement has been recently made known to the writer by Dr. Wm. C. Minor, as a distinctive feature of the Bdelloids. Quatrefages remarks that the Planarians and Trematodes may well be regarded degraded forms of the Hirudines, and the three tribes are arranged in one group by Burmeister.—Third, *Gephyreans* (of de Quatrefages), or *Holothurioid* (*Radiate*-like) Worms, including the genera, Echiurus, Sipuncula, etc.[7]—Fourth, *Cestideans*, or *Protozoic* Worms, including the Cestoids, in which there is no normal digestive system, and the segments are independently self-nutrient.[8]

The orders of these classes of Articulates are the following:

	Insecteans.	Crustaceans.	Worms.
Alphatypic,	———	———	———
Betatypic,	Insects.	Decapods.	Annelids.
Gammatypic,	Spiders.	Tetradecapods.	Bdelloids.
a. Degradational,	Myriapods.	Entomostracans.	Gephyreans.
b. "	———	———	Cestideans.

6. *Subdivisions of the orders of Insecteans and Crustaceans into tribes.*—(1.) The orders of *Insecteans* have each three divisions, excepting that of Myriapods in which but two have been recognized. The three of Insects are indicated on pages 323, 335. The fact that Insects are, in type-idea, *flying* Articulates gives special importance to the wings in classification. The *first* order includes the *Prosthenics*, in which the anterior wings are flying wings, as the Hymenopters, Dipters, Neuropters, Lepidopters and Homopters. The *second* consists of the *Metasthenics* or *Elytropters*, in which the anterior wings are not used in flying, or but little so, as the Coleopters, Strepsipters, Orthopters and Hemipters. The Hemipters and Homopters, united in one tribe by most entomologists, are hence profoundly distinct. The *third* tribe, or *Apters*, embraces the Lepismids and Podurellids; the remaining Apterous insects being distributed among the other

[7] The Holothurioid characteristics are well exhibited by de Quatrefages in Part ii, p. 248 and beyond, of *Recherches Anatomiques et Zoologiques faites pendant un voyage sur les Côtes de la Sicile, etc.*, in 3 vols. or parts, the second by de Quatrefages. Paris.

[8] The *Acanthocephali*, according to van Beneden and Blanchard, are Nematoids, (with which they agree in form and general structure) although without a digestive system. Blanchard states that there is reason for believing that the digestive system becomes atrophied with the growth of the animal, and mentions that cases of like atrophy occur even in species of *Gordius* and *Nemertes*.

groups, as suggested by different entomologists. The Lepismæ show their degradational character in their larval forms and in other approximations to the Myriapods, and the Podurellids appear to be still inferior in having the abdomen elliptic in some segments.

(2.) The orders of *Spiders* suggested by the principles of cephalization are in precise parallelism with those of the Decapod and Tetradecapod Crustaceans. They are, first, *Araneoids*, including all the *Pulmonates*, except the Pedipalps; second, *Scorpionoids*, or the Pedipalps from among the Pulmonates, and the Chelifer-group from among the Trachearians; third, *Acaroids*.

The Araneoids are *Brachyural* Spiders; the Scorpionoids, *Macrural;* while the Acaroids are *degradational*. The last show their degradational character in having no division between the abdomen and cephalothorax; so that, while Insects have the body in *three* parts, head, thorax, and abdomen, and ordinary Spiders in *two*, cephalothorax, and abdomen, the Acaroids have it *undivided* (page 326). Thus, one of the most prominent characteristics marking the descent from Insects to Spiders becomes the characteristic of a further descent among Spiders themselves—illustrating a common principle with regard to such subdivisions. (See p. 350 beyond.) The propriety of making the Acaroids a distinct group appears therefore to be well sustained.

The usual subdivision of Spiders into Pulmonates and Trachearians depends on *internal* characters, which is not the fact with any other subdivisions in the table beyond. Moreover, these names, though *seeming* to mean much, are not based on any *functional* difference between the groups. Spiders have many relations to Crustaceans; and it is natural that the subdivisions in both should depend on the same methods of cephalization, the amplificative and analytic (p. 335).

(3.) The two orders of *Myriapods* are examples, one of case *a*, the other of case *b*, under multiplicative decephalization (p. 325).

The close relations between Isopods and the higher Myriapods, suggest that they are of like grade under their respective types, that is, betatypic.

(4.) *a*. Under *Decapod Crustaceans*, the subdivisions are *three*, as remarked upon by the author, at page 326 of this volume.[a]

The Anomurans are only degradational Brachyurans, and do not represent an independent type of structure. The Schizopods, similarly, are degradational Macrurans, with which they should be united. The *third* type is that of the *Gastrurans*, which are peculiar, among Decapods, in having the viscera extend into the abdomen, one of the marked degradational features of the type. They are the Stomapods of Latreille; but this author, in his last edition, made the group, in connection with the Schizopods,

[a] See also vol. xxv, [2], pp. 337, 338.

coördinate with that of Decapods. Being coördinate with Brachyurans and Macrurans, the change of name is necessary.

b. The *Tetradecapods* include two divisions precisely parallel with the first two of the Decapods, the first literally *brachyural*, the second *macrural*. (See p. 335 of this volume.) The *Anisopods*, of the writer, are degradational Isopods, just as the Anomurans are degradational Brachyurans. The Lemodipods (Caprellids, etc.) are only degradational Amphipods, the structure of the two being essentially the same in type. Hence, neither the Lemodipods nor the Anisopods are an independent type corresponding to a *third* subdivision.

The *third* subdivision probably is made up of *Trilobites*, although these are generally regarded as Entomostracans. One of the most prominent marks distinguishing Entomostracans from Tetradecapods is the absence of a series of abdominal appendages. It is highly improbable that the large abdominal (or caudal) plate of an Asaphus, or the many-jointed abdomen of a Paradoxides, Calymene, etc., should have been without foliaceous appendages below; and if these appendages were present, the species were essentially Tetradecapods, although degradational in the excessive number of body-segments.

c. Entomostracans (or Colopods, as they are more appropriately styled) embrace four orders. First, *Carcinoids* (as named by Latreille) consisting of the Cyclops group (Copepods of Edwards), whose species have a strong Macrural or shrimp-like habit; to which should be added the Caligoids, (Cormostomes of the writer, Siphonostomes of others,) since they are essentially identical in type of structure with the Cyclopoids, as may be seen on comparing *Sapphirina* of the latter with *Caligus*.—Second, *Ostracoids* (or the Daphnia, Cypris and Limnadia groups), which have, besides a bivalve carapax more or less complete, a much more elliptic abdomen than the Carcinoids, it being short, incurved, and without a lamellar terminal joint or terminal appendages.—Third, *Limuloids*, which have the abdomen still more elliptic, it being reduced to a mere spine, or nearly obsolete, and which have the mouth-organs all perfect feet and the only locomotive organs. (The joint across the carapax of the Limulus corresponds in position to a suture or imperfect articulation in the carapax of the Caligi, etc.)—Fourth, the *Rotifers*, a low Protozoic grade of degradation, in which all members are wanting, and locomotion is performed by cilia. The Phyllopods are distributed between the first two divisions.

The Rotifers are sometimes arranged under Worms. If they are degradational species of a limitate type, they are Crustaceans; and if of a multiplicate, they are Worms. The very small number of segments present, when any are distinct, the character of the dentate mandibles (for mandibles are *not* found in the inferior subdivisions of Worms), and the resemblance in the form of some species to Daphniæ and other Entomostracans, sustain the view that they are Crustacean.

The Cirripeds appear to be only attached, amplificate Ostracoids. (See pages 324, 325.)

The subdivisions of the orders of Insecteans and Crustaceans are then the following:

	Insects.	Spiders.	Myriapods.	Decapods.	Tetradecap's.	Entomostr.
α.	———	———	———	———	———	———
β.	Prosthenics or Ctenopters.	Araneoids.	Chilopods.	Brachyurans.	Isopods.	Carcinoids.
γ.	Metasthenics or Elytropters.	Scorpionoids.	Diplopods.	Macrurans.	Amphipods.	Ostracoids.
a. D.	Apters.	Acaroids.	?	Gastrurans.	Trilobites.?	Limuloids.
b. D.	———	———	———	———	———	Rotifers.

7. *Subdivisions of the orders of the class of Worms.*—On the true method of grouping the typical (Branchiate and Abranchiate) Annelids, I here make no suggestions. The tribes of the other orders are probably those indicated on page 343, and which need not be here repeated. The Cystics are there included with the Cestoids. If any of the *simple* Cystics are really adults, they may possibly make a second subdivision of the Cestideans.

8. *Subdivisions of the classes of Mollusks.*—The Ordinary Mollusks include three orders, as usually given: (1) *Cephalopods,* (2) *Cephalates* and (3) *Acephals;* of which, the first two correspond to different grades of typical Mollusks, and the last is degradational in its relations to the type, the species being imperfect in the senses and means of locomotion.

The Ascidioid Mollusks comprise (1) *Brachiopods* and (2) *Ascidians*, with perhaps the *Bryozoans* as the *third* order. If the last, however, be made a third *class*, as suggested (though with hesitation) on page 340, there is no third order, unless the inferior of the compound Ascidians, having water-apertures to a *group* of individuals instead of to each one, and the mouth-opening of each usually *radiated* (the number of rays *six*), be regarded as the third. This would make the orders, (1) *Brachiopods;* (2) *Ascidians;* (3) *Incrustates;* the first two typical, the last degradational and strikingly hemiphytoid.

4. *Conclusions.*

The preceding review of zoological classification appears to sustain the following general conclusions.

1. *Number and typical relations of the subdivisions of groups.*

I. The number of subkingdoms, classes, orders, and tribes in the system of animal life is either *four* or *three*, that is, the division in each case is either *quaternate* or *ternate.*

II. The lowest of the subdivisions in each group is a degradational or semidegradational subdivision, or *hypotypic.*

III. The quaternate division is confind to *six* cases (excepting two or three among inferior types in which there are *two* degradational subdivisions): 1, the number of subkingdoms; 2, the number of classes under Vertebrates, the highest of the subkingdoms; 3, 4, the number of orders under Mammals and Fishes, the highest and lowest classes of Vertebrates; 5, 6, the numbers of tribes under two of the orders of Mammals.

IV. In *three* only of the six cases of *quaternate* division are the three higher subdivisions all *true typical*, namely; 1, in the division of the animal kingdom into subkingdoms; 2, of the Vertebrates into classes; 3, of Mammals into orders. In the last we reach Man. As man alone is archetypic in the class of Mammals (p. 334), so the Mammal-type is archetypic among Vertebrates, and the Vertebrate-type among the subkingdoms.

b. Below this archetypic level, in the orders of Mammals, the number of *true typical* subdivisions is but *two*—and these are the *betatypic* and *gammatypic;* for the first or alphatypic subdivision in both Megasthenes and Microsthenes, as explained on page 334, is *hypertypic*, and not true typical.

c. Again, of the *four* orders of Fishes only *one* is typical, the *two highest* being *hypertypic* (p. 334).

V. In the rest of the animal kingdom, the number of *true typical* groups, in the classes, orders and tribes that have been reviewed, is either *two*, the *betatypic* and *gammatypic*, or *one*, the *gammatypic* alone.

2. *Lines of gradation.*—Lines of gradation between groups are lines of convergence or approximation through intermediate species. Before mentioning under this head the deductions from the preceding classification (or VIII, and IX beyond), two general principles (VI and VII), having an important bearing upon them, are here introduced.

VI. The approximations between two groups usually take place, as has been frequently observed, through their *lower limits*, or most inferior species, that is, between the degradational subdivision of the inferior as well as of the superior group.—For example, plants and animals approximate only in their simplest species, the Protozoans and Protophytes; Birds and Quadrupeds most nearly in the Ornithorhynchus or Duckbill—which, at the same time that it is the lowest of Mammals, is related to a very inferior type of Birds, the Ducks; Quadrumanes and inferior Mammals through the Lemurs of the former and the Bats and Insectivores of the Microsthenes, and not through the higher Carnivores or even any of the Megasthenes.

The classes of Reptiles and Fishes may appear to be an exception. But the *Perennibranchs* (or the species with permanent gills) among Amphibians, if referred to the type of Fishes, and especially to the Ganoid type, would rank low, as is obvious from their exsert and loosely-hung gills without gill-covers, the absence of scales, and the general inferiority in all structural arrangements. The Ganocephs, known only as fossils and generally regarded as Perennibranch Amphibians, have, it is true, a higher grade of organization, both as regards gills and scales, being allied, in these respects, to the highest of Ganoids. And this fact, in view of the above canon, sustains the opinion of Agassiz that the Ganocephs (or Archegosaurs) are actually Ganoids,—having a Reptilian feature in the partial elongation of the limbs, but in little that is fundamental in the structure beyond what belongs essentially to the Ganoid-type.

VII. The lines of gradation between classes, orders and tribes, are only approximating, not connecting, lines, there being often wide blanks of the most fundamental character. The Ornithorhynchus, although Duck-like in some points, leaves still a very wide unfilled gap between the Mammal and Bird, and the Marsupials a still wider. The species are fundamentally Mammalian, and Bird-like only in points of secondary importance. In a similar manner, there are long blanks between the Oötocoids and higher Mammals; between Myriapods and either Insects or Spiders; between Reptiles and Mammals. The intermediate groups belong decidedly to one or the other of the two approximating groups, and are never strictly intermediate.

VIII. Under any *class*, *order* or *tribe*, the lines of gradation run in most cases between the *degradational* subdivision and severally the *gammatypic* and *betatypic* subdivisions, and far less clearly, or not at all, between the gammatypic and betatypic themselves; that is, between D and γ, and D and β, rather than β and γ. For example, in the class of Mammals, the lines run between Oötocoids and either Megasthenes or Microsthenes, and not distinctly between Megasthenes and Microsthenes; in Insecteans, between Myriapods and either Insects or Spiders, and not distinctly between Insects and Spiders; in Crustaceans, between Entomostracans and either Decapods or Tetradecapods, and not distinctly between Decapods and Tetradecapods; etc. There are exceptions to the canon; and still it is a general truth.

IX. Under any *class* or *order* the line of gradation between the *degradational* and the *betatypic* subdivision (or D and β) is often more distinct than that between the *degradational* and *gammatypic*, (or D and γ), although the gammatypic is nearer in grade to the degradational.—Thus, the line between Myriapods and Insects is more distinct than that between Myriapods and Spiders; or that between Entomostracans and Decapods, than than that betweeen Entomostracans and Tetradecapods.

There is an exception in the class of Mammals: the Oötocoids seem to graduate towards both Microsthenes and Megasthenes with nearly equal distinctness.

3. *Coördinate grades and distinctions in Classification.*

X. The coördinate value of subdivisions in the system of classification is brought out to view in the parallel columns of the preceding tables, and evidence is thence afforded as to what groups are rightly designated, classes, orders, etc.

a. We thus learn that the subdivisions of the class of Mammals—Man, Megasthenes, Microsthenes,—are properly *orders*, if we so call the subdivisions Decapods and Tetradecapods under Crustaceans, or Insects and Spiders under Insecteans.

b. Again, we have a solution of the question whether in each of the classes, Mammals, Birds, and Reptiles, the *hemitypic* division, as so-called on page 316, is a *subclass* coördinate with the *typical* division of the same, *or* whether it is an *order* coördinate with the three higher subdivisions of the class. The question appears to be decided, (contrary to former views of the writer,) that it is correctly made an *order*. These hemitypic divisions actually correspond severally to the degradational division in other columns of the different tables; and, therefore, if in the case of other classes, as those of Crustaceans, Insecteans, &c., they are *orders*, so are they in the three classes of Vertebrates mentioned. They have also a relation to the *hemitypic* divisions among Fishes, which are the first and second *orders* of the class.

XI. In an *inferior* or *degradational* group, the distinctions of the subdivisions included are generally much more strongly and obviously exhibited in the structure than among *typical* groups. Thus, the orders of Fishes are based on characters that have nearly a class-value among the higher Vertebrates. In the same manner, Amphibians, or hemitypic Reptiles, differ from true Reptiles more obviously than Oötocoids, or hemitypic Mammals, differ from other Mammals. So, the distinctions among the groups of Crustaceans are very wide compared with those among Insects; and those among degradational Crustaceans far wider than those among the typical subdivisions. The relative force of the life-systems is, in all probability, as great between Oötocoids and typical Mammals as between Amphibians and typical Reptiles, although so unequally expressed in the structure of the high or concentrated groups and the low or lax groups of species. Overlooking this principle has often led authors to allow too great importance to the structural differences among inferior or degradational groups.

XII. Under any class, order, tribe, the *typical* groups are often represented more or less clearly among the subdivisions of the *degradational*. Hence characteristics which separate the typical groups frequently separate only subordinate divisions under an inferior or degradational group. Examples occur in the class of Fishes under Vertebrates, in whose subdivisions the other classes of Vertebrates are partly represented; in the order of Oötocoids under Mammals, which has its megasthenic and microsthenic subdivisions; under Worms, etc.

4. *Distinction between Animals and Plants.*

XIII. This subject well illustrates a fundamental distinction between animals and plants.

a. An animal, as has been stated on page 332, has *fore-and-aft*, or antero-posterior, polarity; that is, it has a fore-extremity and a hind-extremity which have that degree of oppositeness that characterizes polarity.

b. With this fore-and-aft polarity there is also *dorso-ventral* polarity.

c. The dorso-ventral and antero-posterior axes are at *right angles* to one another. In Invertebrates and a large part of Vertebrates the antero-posterior axis is horizontal and the dorso-ventral vertical; and only in Man, the prince of Mammals, is the former vertical and the latter horizontal.

d. An animal, again, has not only oppositeness between the fore-extremity and hind-extremity, but also a *head*, the seat of the senses and mouth, situated at the fore-extremity and constituting this extremity.

e. In addition, the typical animal is *forward moving.*

But in animals of the inferior type of *Radiates,* while there is an anterior and a posterior side, and also, in most species, forward motion, the mouth-aperture—which indicates the *primary centre* in an animal (p. 322)—is not placed at one extremity, but is more or less nearly *central*; and almost precisely central in the symmetrical (and therefore inferior) Radiates. The mouth-extremity and the opposite are at *the poles of the dorso-ventral axis*, and not at those of the antero-posterior; that is, they are at the extremity of the axis which in the inferior animals is normally *vertical.* This is true even in a Holothuria, the mouth of which is not at the *anterior* extremity, but is central, or nearly so, as in an Echinus. A Limulus has been referred to on page 328 as showing an approximation, under the true animal type, to this same central position of the mouth.

We pass now to *Plants.* The plant, in contrast with the fore-and-aft animal, is an *up-and-down* structure, having up-and-down polarity. The axis is *vertical* like the dorso-ventral in the lower animals, to which it is strictly analogous, as is shown from a comparison with Radiates,—Radiates and Plants being alike in type of structure. The primary centre of force is central, in the same sense, in the regular flower and the symmetrical Radiate.

Thus, the structures under the animal-type and plant-type are based on two distinct axial directions, one at right angles to the other: in the *animal-type* the antero-posterior axis being the dominant one, while the two coexist; and in the *plant-type* the axis at right angles to this being the only one.

In the above way, (as well as in its non-percipient nature,) the plant exhibits complete decephalization—a condition to which the Radiate only approximates, as it has generally, if not always, an anterior and posterior side, besides other animal characteristics.

Note to page 327.—The term elliptic, as used on page 327, implies defectiveness or deficiency of parts *through abnormal weakness* in an organ or the general system. The foot of the horse, one of the examples mentioned, is therefore hardly elliptic, since it has its full normal strength in the one toe, this being enlarged at the expense of the others. Paragraph *a* and the second under *b* hence require correction accordingly. In the fifteenth line from the foot of the page, Animal-type should be Mammal-type.

ART. XXX.—*On vibrating Water-falls;* by ELIAS LOOMIS, Professor of Natural Philosophy and Astronomy in Yale College.

IN the year 1843, I published in this Journal, vol. xlv, p. 360, a notice of several water-falls which at times exhibited very strong vibratory motion. I am not aware that any one else had ever published anything on this subject previous to that time. In that article, I proposed an explanation of these vibrations, which was naturally suggested by the only case which I had myself had an opportunity to investigate; viz., that the dam itself was the vibrating body, and that the vibrations were analogous to those of a stretched cord. The attention of several other observers has since been called to the same subject, and Prof. Snell has endeavored to show that the vibrating body is the column of air behind the sheet of water, and that the time of vibration depends upon the length of this column. I have thus been led to examine the subject anew, and have been compelled to modify the views which I first published. I have made a large number of observations on several different water-falls, and have obtained numerous observations made by other persons. These observations have been made chiefly at three places, viz: at South Natick, Mass.; at Holyoke, Mass.; and at Lawrence, Mass.

Observations at South Natick, Mass.

In August, 1859, Mr. William Edwards communicated to me the results of numerous observations which he had made upon a water-fall at South Natick, Mass., about 18 miles west of Boston. The dam over which the water fell, consisted of two separate portions. One of them, the northern, was 98 feet long and 9 feet high, built of hewn timbers generally about 10 inches square, framed together, and banked up with earth to within three feet of the edge. Mr. Edwards, who resided near the dam, had determined that

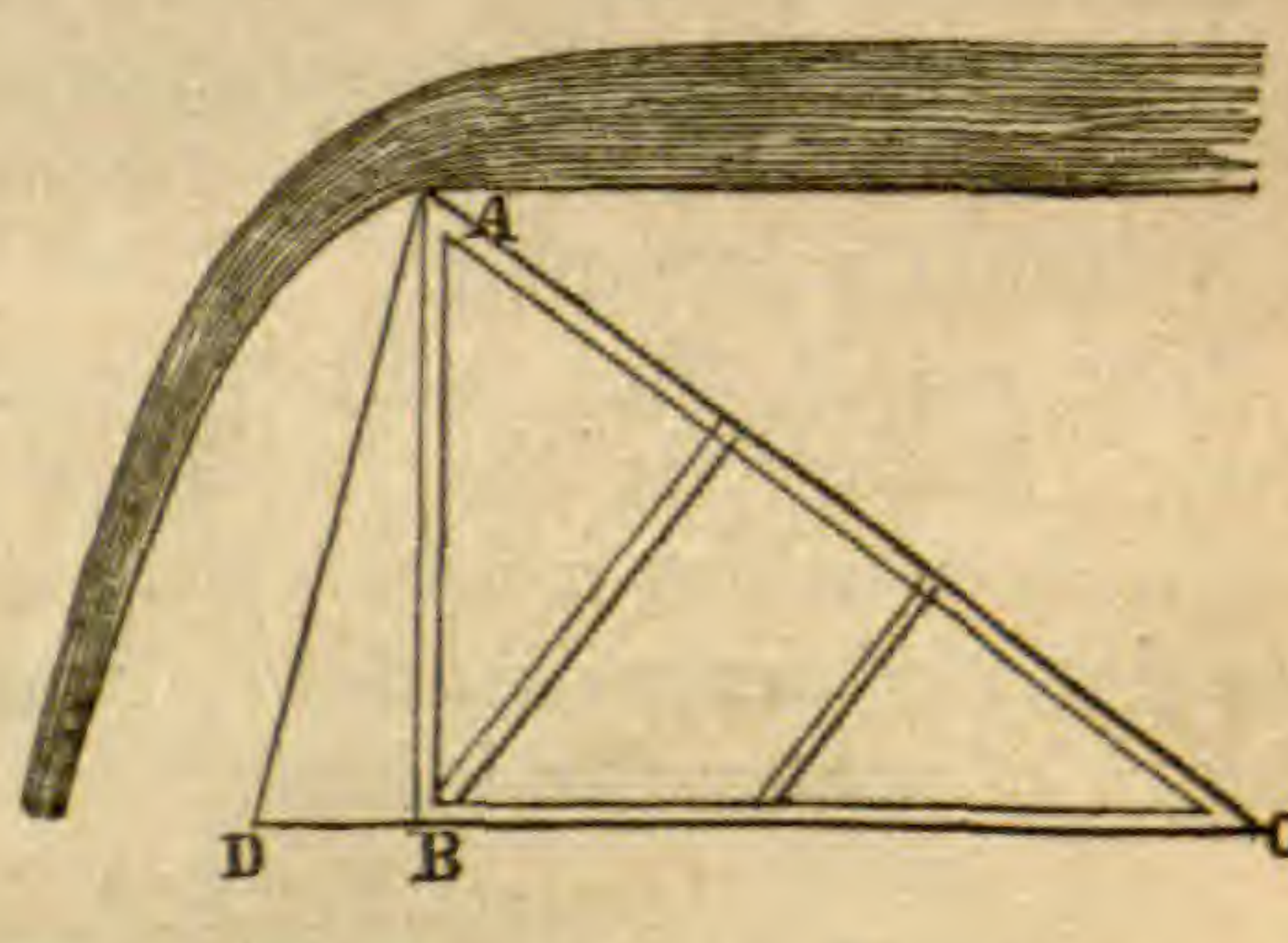
When the height of the water on the edge of the dam was		the number of vibrations was	
	10 inches,		280 per minute.
	8 "		300 "
	7 "		335 "
	5 "		over 400 "

I at once perceived that these numbers indicated a general law; viz., that *the time of one vibration is nearly equal to the time in which a heavy body would fall through a space equal to the depth of water on the dam.* This will appear from the following table, in which column first shows the depth of water on the edge of the dam; column second shows the time of one vibration expressed in fractions of a second; and column third shows the time which a heavy body would require to fall through the spaces in column first, and is computed by the formula $t=\sqrt{\frac{2s}{g}}$. Column fourth shows the quotients obtained by dividing the numbers in column second by those in column third.

Depth of water.	Time of one vibration.	Time of falling through depth.	Ratio.
10 inches.	0s·214	0s·228	0·940
8 "	0 ·200	0 ·204	0·982
7 "	0 ·179	0 ·190	0·940
5 "	less than 0 ·150	0 ·161	less than 0·932

In April, 1861, a portion of this dam, about 70 feet in length, was carried away, and was rebuilt during the following summer. The new part of the dam is of the same height as the old one, and is boarded up tight in front at a slope of about 75°; but the old part, being about 30 feet in length, is open in front as formerly. In the annexed figure, AB represents the height of the dam, AC the slope inclined against the stream, and AD shows how the new part is boarded up on the lower side. Formerly, the space behind the sheet of water was entirely free, back to AC, with the exception of the interruption caused by the beams which supported the dam. Now when the water is only 5 inches deep, the descending sheet strikes AD within about 12 inches of the bottom. There is a square stone abutment on the south side of the sheet, and it is boarded up with planks on the north side, so that the air behind the sheet of water is almost completely enclosed.

1.



The south dam is 100 feet long, and 9 feet high, and boarded up in front at a slope such that with a depth of 8 inches of water, the descending sheet strikes the sloping boards nearly in the middle of their height. The south dam is quite old and its edge is rough, so that the vibrations are never very regular or strong. The north dam is the one upon which Mr. Edwards' observations have all been made.

In the spring of 1863, Mr. Edwards, at my request, made a more extended series of observations, measuring accurately the depth of water on the edge of the dam, and counting the corresponding number of vibrations. The depth of water was measured by inserting a small rod 4 inches from the edge of the dam. Mr. Edwards states that, at a distance of 20 rods above the dam, the water was $1\frac{3}{4}$ inches higher than at the point where he measured it. The following table shows the results of Mr. Edwards' observations in 1863, and is deduced from numerous trials between April 23 and May 4.

Depth of water.	Time of one vibration.	Time of falling through depth.	Ratio.
9·21 inches.	0s·220	0s·218	1·007
8·12 "	0 ·192	0 ·205	·937
7·00 "	0 ·177	0 ·190	·930
6·31 "	0 ·164	0 ·181	·906
5·06 "	0 ·138	0 ·162	·850

These results differ sensibly from those deduced from Mr. Edwards' first series of observations. The difference may be ascribed in part to the greater accuracy of the last observations, since fractions of an inch were disregarded in the first measurements. Perhaps also something is due to the modification in the structure of the dam. I am however inclined to think that the influence of the latter cause is slight, and that the discrepancies are mainly due to the unavoidable imperfection of such observations. Any one who shall attempt to repeat these observations, instead of complaining that Mr. Edwards' results are not perfect, will probably be surprised at the precision which he has actually attained.

The result, then, which we deduce from these observations at South Natick, is that *the time of one vibration is a little less than the time in which a heavy body would fall through a space equal to the depth of water on the dam;* and this deficiency increases as the depth of water diminishes.

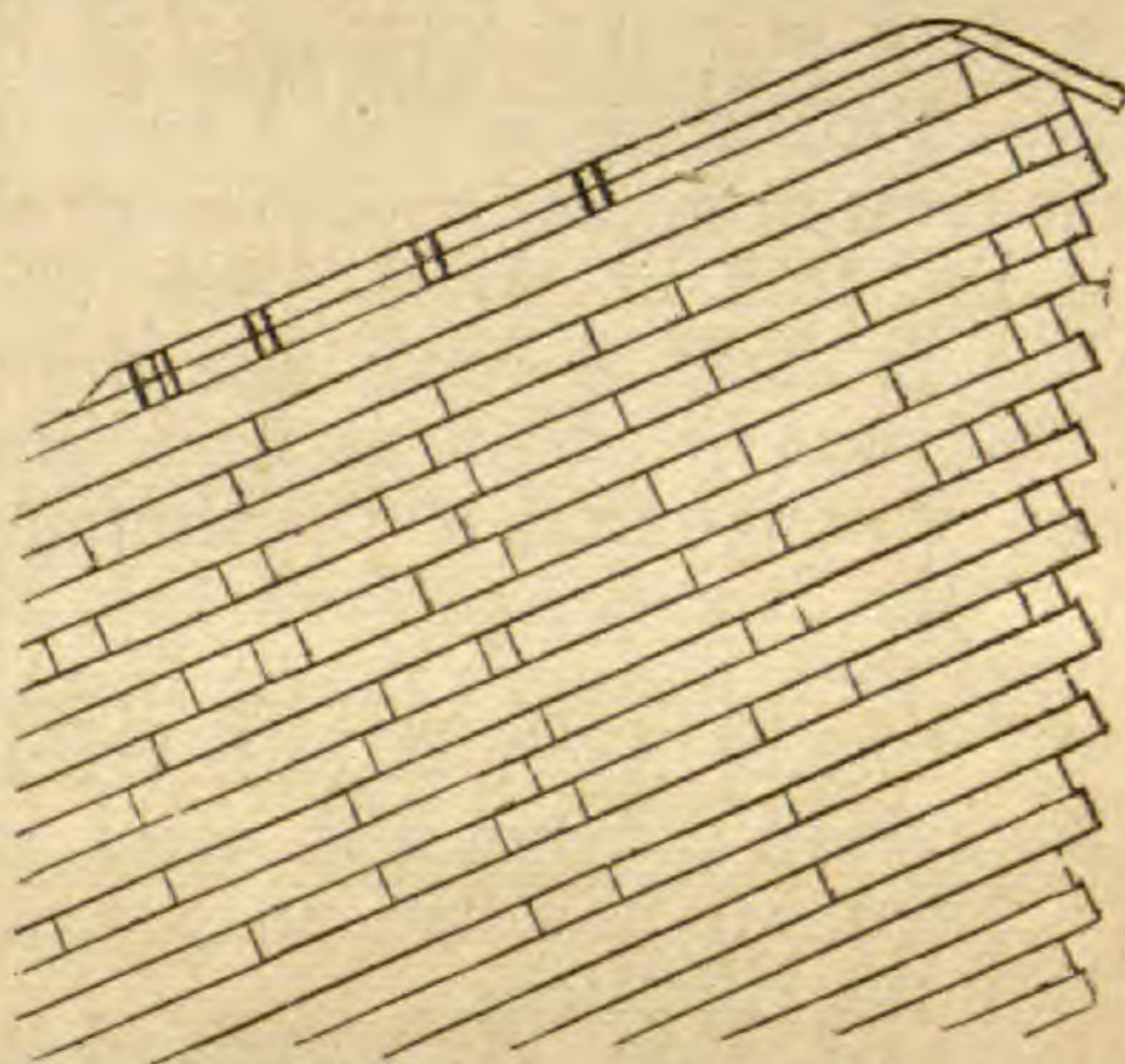
Observations at Holyoke, Mass.

The dam across the Connecticut river at Holyoke, Mass., is 1017 feet long and 30 feet high. It is formed of square timbers, inclined 22 degrees to the horizon, having one end bolted to the rock, and the other resting upon a timber frame-work. From the crest of the dam, the water descends along an apron about four feet in length, which slopes downwards at an angle of 22 degrees. Professor Snell has observed the vibrations at six different times, and with the following average results (this Journal, vol. xxviii, p. 229).

When the water on the edge of the dam is	1 foot in height,	the number of vibrations is	257½ per minute.
	2 feet "		137⅔ "
	5 feet "		82 "

I have spent considerable time at Holyoke, for the purpose of determining the number of vibrations corresponding to different depths of water. I visited the place in August, 1859, but there were only 6 inches of water on the crest of the dam, and the vibrations were not noticeable. I obtained the engineer's plan of the dam, from which the annexed figure has been reduced on a scale of $\frac{1}{12}$ inch to a foot. In July, 1860, I spent several days in observations, during which time the depth of water ranged from two feet to one foot; and again in April, 1861, when I saw the water on the crest of the dam increase from 43 inches to 100 inches. The vibrations ceased altogether after the water had risen to 76 inches. I also made a few observations in July, 1861.

2.



To indicate the depth of water, there is a gauge, graduated to feet and inches, placed 60 feet from the crest of the dam, and firmly secured to the stone abutment. The annexed plan will show the position of the gauge with reference to the dam. The figure represents the stone abutment on the west side of the dam, together with a small portion of the dam. The position of the gauge is indicated by the letter G. At each of my observations, I recorded the depth of water by this gauge; and when the water was not more than two feet deep, I measured it directly on the crest by means of a pole. When the depth was too great to allow this mode of measurement, I measured the difference of level between the surface of the water at the gauge,

3.

and on the crest, by comparing it with the top line of the abutment, which appeared to have been levelled with considerable care. In this manner it was found that when the depth, as shown by the gauge, was 97½ inches, the depth on the crest was 15 inches less. The following table exhibits the average of a considerable number of comparisons.

Depth by gauge.	Depth on crest.	Difference.
14·75 inches.	12·25 inches.	2·5 inches.
16·5 "	13·5 "	3 "
23 "	19 "	4 "
97·5 "	82·5 "	15 "

For depths less than two feet, the depression of water on the crest of the dam is 0·175 of the depth by the gauge; but for a depth of 8 feet, the depression is 0·154 of the depth by the gauge. From these results, I have deduced the depth of water on the crest from the indications of the gauge.

On the 10th of April, 1861, the vibratory motion of the descending sheet of water was through a space of one or two feet, and was synchronous from one end of the dam to the other. Through each of the abutments, and on a line with the edge of the dam, is a passage way, 2½ feet wide and 7 feet high. Through these passages, the air frequently flows with great force. On the 10th of April, at each vibration of the sheet of water, the air rushed through with great violence towards the waterfall. There was however no *outward* motion, but simply a lull between the vibrations.

April 11th, 1861, the motion of the sheet was greatest near the middle, but was very noticeable throughout the whole length. There was no wind of any consequence. Depth of water by the gauge 4 ft. 0½ inch.

April 12th, the vibrations were exceedingly strong, with a fresh breeze from the south. Gauge 4 ft. 3½ in.

April 13th, in the morning, the vibrations were sometimes very strong, and then soon afterward moderate, with frequent variations, the wind blowing in gusts from the south with a drizzling rain. Gauge 4 ft. 5¾ in.

April 13th, toward evening, the vibrations were very strong for 6 or 8 times, then feeble for about as many more; and so on, alternately strong and feeble. Near the middle of the sheet, the vibrations were tolerably regular; but towards either extremity, it appeared inclined to stop, and seemed to be forced into vibration by the power of the middle portion of the sheet. Gauge 4 ft. 9½ in.

April 14th, 1861, the vibrations had ceased entirely. Gauge 6 ft. 8 inches.

April 15th, the sheet of water was extremely smooth, without the slightest sign of vibrations. Through the passage way in the abutment on the east side of the river, the air flowed in a strong and steady breeze without pulsations. There was a slight breeze from the northwest. Gauge 8 ft. 2¾ inches.

April 16th, the sheet of water was tolerably smooth without the slightest appearance of periodical vibrations. Gauge 7 ft. 10¾ inches.

Thus it will be seen that when the gauge indicated 4 ft. 9½ inches, the vibrations were becoming irregular; and when the water rose to 6 ft. 8 in. the vibrations ceased entirely. As this rise of the water occurred in the night, it was impossible to determine the exact depth of water at which the vibrations ceased.

The following table presents a summary of all my observations at Holyoke.

Prof. Loomis' observations at Holyoke.

Date.	Depth of water by gauge.	No. of vibrations per minute.	Temperature of water.	Date.	Depth of water by gauge.	No. of vibrations per minute.	Temperature of water.
1859, Aug. 16	6 inches.	0		1860, July 7	28 inches.	135·4	
1861, July 30	14 "	257·0	73½°	1861, Apr. 10	47 "	81·6	43½°
" July 31	14½ "	256·6		" Apr. 11	48½ "	81·2	
1860, July 10	16 "	254·9	73	" Apr. 12	50¾ "	80·8	43
" July 9	17 "	249·2		" Apr. 12	51½ "	80·9	
" July 9	18 "	256·0		" Apr. 13	54 "	80·0	
" July 8	23 "	134·4		" Apr. 13	57¼ "	78·6	
" July 8	25 "	134·8		" Apr. 14	80 "	0	43

Mr. Joseph P. Buckland, a graduate of Yale College, who resides at Holyoke, has at my request made a series of observations, of which the following is a summary.

Date.	Depth by gauge.		Vibrations per minute.	Date.	Depth by gauge.		Vibrations per minute.
1862	ft.	in.		1862	ft.	in.	
Sept. 22	1	10¼	308 nearly.	Sept. 10	2	5½	128
" 21	1	10½	288 or 290	" 6	2	6	133
" 9	1	11½	296 or 298	" 4	2	7	128
Aug. 31	2	0	280	" 5	2	7½	134
Sept. 1	2	0	280	" 5	2	7¾	133
" 9	2	½	292	" 2	2	8½	133
" 8	2	2¼	273	" 3	2	10½	130

It will be perceived that my observations, for depths from 14 to 18 inches, agree pretty well with Prof. Snell's, as stated on page 355; but Mr. Buckland differs materially from both of us. No one, without trying it, can properly appreciate the difficulty of estimating the number of these vibrations when they exceed four per second; and I am of opinion that Mr. Buckland has estimated the velocities for depths from 22 to 26 inches, considerably too great. For depths a little greater than two feet, we all agree reasonably well.

It must be considered as sufficiently established by these observations, that the sheet of water at Holyoke exhibits three different rates of vibration, viz., about 256, 135, and 81 vibrations per minute, corresponding to depths of 16, 28 and 56 inches of water on the dam. The change from the first to the second rate of vibration, takes place when the depth of water is from 23 to 26 inches; and the change from the second rate to the third, takes place when the depth is from 35 to 47 inches. The vibrations are not noticed when the depth of water is less than about 12 inches, and they also disappear when the depth is as great as 80 inches.

Moreover, for either of these modes of vibration, the number of vibrations per minute is not absolutely constant. On account of the discrepancies in the observations, this proposition may not be considered as fully established for the two more rapid modes of vibration; but for the slower mode of vibration the proof is unequivocal. The observations from April 10th to 13th, 1861, were very numerous, and were made with great care, and the vibrations were so slow that they could be counted with almost as great precision as those of an ordinary seconds' pendulum. The number of vibrations per minute dimishes as the depth of water increases from 47 to 57 inches. This effect is of the same kind as that observed at South Natick, but the quantity of the effect is much less.

In order to reduce, as far as possible, the influence of the errors of observation, and to diminish the labor of calculation, I have divided my observations into four groups, and taken the average of the depths of water, as well as of the times of vibration. The first group includes the observed depths less than 20 inches; group second includes the observed depths from 20 to 40 inches; group third, the depths from 40 to 51 inches; and group fourth, the depths from 51 to 60 inches. The results are as follows:

Depth of water by gauge.	No. of vibrations per minute.	Temperature of the water.	Observed time of one vibration.
15·90 inches.	254·74	73°	0s·236
25·33 "	134·87	73	·445
48·75 "	81·20	43	·739
54·33 "	79·83	43	·752

It is evident, from the motion of the air through the passage way in the abutment at the east end of the dam, that the air behind the sheet of water vibrates simultaneously with the sheet. Is it possible to explain the preceding observations on the supposition that the time of one vibration depends simply upon this column of air? The column of air behind the sheet of water is 1017 feet long; its height April 12, 1861, was about 28 feet, and its average thickness was perhaps 8 feet. The temperature of the air behind the sheet is assumed to have been that of the water, viz: 43°. The velocity of sound at this temperature is 1098 feet per second.

A column of air 1017 feet long, and having a very small diameter, would make one vibration in $\left(\frac{1017}{1098}=\right)0^{s}\cdot926$. According to experiments with organ pipes, the length of the column in this formula should be increased by some function of the breadth, generally about twice its breadth. If we increase the length of the column of air by the sum of the height and thickness, we shall have for the computed time of one vibration $\left(\frac{1053}{1098}=\right)0^{s}\cdot959$. Either of these quantities exceeds the time actually observed, by about one-fourth of the latter quantity.

It appears, from experiments with organ pipes of different forms and with different embouchures, that the time of one vibration varies considerably according to the mode in which the impulse is communicated to the column of air; but I do not know of any established principle which will explain how a column of air, having the dimensions of that at Holyoke, should make one vibration in three-fourths of a second, whether it vibrates as one mass, or as two separate portions. The change in the number of vibrations, when the depth of water increases from 48 to 54 inches, indicates that the time of one vibration does not depend solely upon the dimensions of the column of air; and this motion has the appearance of a constrained vibration, depending upon two causes which separately would produce vibration in unequal times. I have therefore computed the time of one vibration, first upon the supposition that the air is the only vibrating body, and secondly upon the supposition that the vibrations originate in the sheet of water, according to the law indicated by the observations at South Natick.

Depth by gauge.	Depth on crest = D.	Observed time of vibration.	Vibration of air computed.	Time of falling through D.
15·90	13·12	0s·236	0s·232	0s·261
25·33	20·90	·445	·465	·329
48·75	40·63	·739	·959	·459
54·33	45·28	·752	·959	·484

In the preceding table, column I shows the depth of water in inches according to the gauge; column II shows the depth on the crest of the dam, determined according to the principles explained on page 356; column III shows the observed time of one vibration; column IV shows the time of one vibration of a column of air 1017 feet long, computed in the manner explained above; the air in the first case being supposed to be divided into four vibrating portions, and in the second case into two portions; and column V shows the time in which a heavy body would fall through the spaces given in column II.

It will be noticed that the observed time of vibration in each case falls between the times computed by these two methods.

Observations at Lawrence, Mass.

The dam at Lawrence is 900 feet long, and has an average height of 34 feet. It is in the form of an arc of a circle whose radius is 7433 feet; and the versed sine of the arc is therefore nearly 14 feet. It is built of hewn stone, consisting of large blocks, accurately fitted to each other.

Sixty-five feet from the south side of the river, is a fish way 30 feet wide, which effectually divides the falling sheet of water, cutting off 95 feet of the dam. The portion of the sheet of water which is entirely free, cannot exceed 805 feet in length.

Stout bars of iron about three feet long, are inserted in holes drilled in the top stones, at intervals of 3 or 4 feet, and these support a row of planks called flash boards, which raise the water 18 inches higher than the stone work of the dam. These planks are put up in June, and remain until they are carried off by the spring or winter freshets. Until these planks are carried away, the vibrations are never noticed.

I have obtained the coöperation of an excellent observer at Lawrence, viz: Mr. Benjamin Coolidge, engineer of the Essex Manufacturing Company, who has watched the dam for several years, and has made careful observations at my request. The following is a summary of his results:

In the spring of 1860, there was no freshet, and no vibrations were noticed upon the sheet of water.

In the spring of 1861, the vibrations were observed as follows:

March 5th. 100 vibrations in 37½ seconds. Depth of water 3·63 feet.

March 11th. 100 vibrations in 37½ seconds. Depth of water 3·41 feet.

April 1st. 100 vibrations in 37¼ seconds. Depth of water 3·12 feet.

The flash boards were on the north end of the dam, for about 60 feet; thence for 40 feet mostly gone. At the south end of the 40 feet, the vibrations began and continued to the fish weir, which gives about 700 feet of vibrating fall. The vibrations made a distinct noise that could be heard pulsating above the roar of the fall.

April 16th. 100 vibrations in 51 seconds. The depth of water on the edge of the crest-stone was 5 feet. Perhaps there were some 50 feet more of vibrating over-fall than on the 1st of April.

April 20th. 160 vibrations per minute. Water on the crest-stone 3·08 feet. Temperature of water 44° F. Temperature of air 46°. As near as I could judge, the length of the vibrating sheet was very nearly 780 feet, and very much the strongest in the middle of the sheet. The rush of air at the north end of the sheet (the only one open to observation) was very strong, and followed the vibration of the sheet, but with a continuous inward draft.

In the spring of 1862, the following observations were made.

April 18. 162 vibrations per minute. Depth of water 5 feet. Thermometer 44°. These vibrations were for 200 feet at north end of dam, alternating with vibrations in middle of dam.

April 19. 134½ vibrations per minute. Depth of water 5 feet. Therm. 42°.

April 21. 136 vibrations per minute. Depth of water 5 feet. Therm. 42°.

April 26. 156½ vibrations per minute. Depth of water 3·18 feet. Therm. 43°.

May 5. 150 vibrations per minute. Depth of water 3·25 feet. Therm. 49°.

May 6. 154 vibrations per minute. Depth of water 2·95 feet. Therm. 50 deg.

April 18, 1863, Mr. Coolidge observed the number of vibrations to be 122 per minute, and the depth of water 5½ feet measured on the crest of the dam.

The following table presents a summary of

Mr. Coolidge's observations at Lawrence, Mass.

Date.	Depth of water.	Observed time of one vibration.	Date.	Depth of water.	Observed time of one vibration
1862, May 6	2·95 feet.	0^s·390	1861, March 5	3·63 feet.	0^s·375
1861, April 20	3·08 "	·375	" April 16	5·00 "	·510
" April 1	3·12 "	·372	1862, April 18	5·00 "	·370
1862, April 26	3·18 "	·383	" April 19	5·00 "	·446
" May 5	3·25 "	·400	" April 21	5·00 "	·441
1861, March 11	3·41 "	·375	1863, April 18	5·50 "	·492

The first seven of these observations agree with each other about as well as such observations usually do. The mean of these seven observations gives a depth of 3·23 feet, and the corresponding time of vibration is 0^s·381.

The observations of April 16, 1861, and April 18, 1862, differ very widely. I can only ascribe the discordance to the effect of flash boards which had not been carried away, and which created openings in the descending sheet of water. I have therefore thought best to take the mean of the last five observations, which gives the time of one vibration 0^s·452, corresponding to a depth of 5·10 feet.

I visited Lawrence in August, 1859, and again in August, 1861, but the depth of water at these times was not sufficient to produce vibration. In April, 1863, I had the good fortune to witness the vibrations with considerable force. I found it very difficult to count the number of vibrations satisfactorily. For a few seconds, the pulsations would be very marked, and then they would often become so faint as to be noticed with difficulty, so that it was generally impossible to follow them for an entire minute very satisfactorily. The descending sheet of water was very uneven, being marked by numerous ridges, probably caused by the flash boards having been only partially carried away. I spent two days in almost constant attempts to count the vibrations, and my result was an average of 148·68 vibrations per minute, corresponding to a depth of 4·41 feet water by the gauge. If we reduce this depth according to the rule determined at Holyoke, it would give a depth of 3·68 feet on the crest of the dam; but according to Mr. Coolidge's measurements, it would indicate a depth of exactly 3 feet on the crest of the dam.

Comparing all these observations, they seem to favor the conclusion that the time of one vibration increases as the depth of water on the dam increases; but the discrepancies are so great that even this conclusion might be called in question, if it were not confirmed by the observations at South Natick. If we take an indiscriminate mean of all the observations, we find the average time of one vibration to be $0^{s}{\cdot}409$, corresponding to a depth of water on the crest of the dam amounting to 4 feet, and a temperature of the water 44°.

A column of air 805 feet in length, 30 feet high, and 7 feet wide, according to the principles stated on page 359, would make one vibration in $0^{s}{\cdot}766$; or if it vibrated in two portions, the time would be $0^{s}{\cdot}383$. A heavy body would fall through a space of 4 feet in $0^{s}{\cdot}499$. The observed time is intermediate between the times computed by these two methods.

It seems impossible to explain all the preceding observations upon the supposition that the air behind the sheet of water is the sole vibrating body, following the laws which have been established in the case of organ pipes. The observations at South Natick naturally suggest the idea that the vibrations originate in the sheet of water itself; the time of one vibration being nearly proportional to the square root of the depth of water on the dam.

Is there any known principle which will enable us to explain vibrations of this description?

It has been suggested that such vibrations may originate in the unequal velocity of the upper and lower strata of the sheet of water which pours over the dam. If there were no friction, the lower stratum should have a velocity equal to that which a heavy body would acquire in falling through a space equal to the height of the water above the dam. The upper stratum has only the velocity of the stream, combined with that which is imparted by the motion of the lower stratum. The lower stratum (if it could move independently) would describe a parabolic path, which could be computed from the depth of water on the dam. But the upper stratum acts as a load, forcing the lower stratum to describe a path more nearly vertical. The descending sheet of water is therefore in a condition of equilibrium, resulting from the superior momentum of the under part of the sheet, balanced by the weight of the upper part. Suppose now that from any external cause (for example a puff of air) the sheet is forced back beyond the position of equilibrium, the instant this force ceases to act, the momentum of the under part of the sheet forces the entire mass forward, and its inertia carries it beyond the position of equilibrium. The weight of the upper part of the sheet forces it again backward; and thus it has been supposed there might result a series of vibratory movements.

It seems difficult, however, upon this theory, to account for the nearly isochronous, long continued and powerful vibrations which we actually observe.

It has been suggested that this vibratory motion originates in the column of air behind the sheet of water, while the water serves merely as a load, to retard the velocity of these vibrations. The column of air behind the sheet of water at South Natick, is of such dimensions that it should make one vibration in about one-tenth of a second. When the depth of water is 5 inches, the observed time of vibration is $0^{s}{\cdot}038$ greater than the computed time of an air vibration; but with a depth of 10 inches the observed time is $0^{s}{\cdot}114$ greater than the computed time.

When an elastic string is loaded, the time of one vibration varies as the square root of the load. At South Natick, when the depth of water increases from 5 to 10 inches, the increase in the time of a vibration seems to be even more rapid than the increase in the depth of water.

At Holyoke, when the depth of water is 45 inches (if we suppose the air to vibrate as one mass) the observed time of vibration is less than the computed time for a column of air without any load, which appears impossible according to this theory. If we suppose the column of air to be divided into two vibrating segments, the time of an air vibration is $0^{s}{\cdot}480$, while the observed time is $0^{s}{\cdot}272$ greater than this.

When the depth of water is 40 inches, the observed time is $0^{s}{\cdot}259$ greater than that of an air vibration.

When the depth of water is 21 inches, (if we suppose there are only two vibrating segments), the time of an air vibration is $0^{s}{\cdot}465$, while the observed time is a little less than this; which seems impossible upon the present theory. If we suppose there are three vibrating segments, the time of an air vibration is $0^{s}{\cdot}310$; while the observed time is $0^{s}{\cdot}135$ greater than this.

When the depth of water is 13 inches, (if we suppose there are four vibrating segments) the computed time of an air vibration, is $0^{s}{\cdot}232$, while the observed time is only $0^{s}{\cdot}004$ greater than this. If we suppose five vibrating segments, the time of an air vibration is $0^{s}{\cdot}186$, while the observed time is $0^{s}{\cdot}050$ greater than this. If we suppose six vibrating segments, the observed time is $0^s \cdot 081$ greater than the computed time of an air vibration.

At Lawrence, (if we suppose there are only two vibrating segments,) the time of an air vibration is $0^s \cdot 383$, while the observed time exceeds this by only $0^s \cdot 026$. If we suppose there are three vibrating segments, the time of an air vibration is $0^s \cdot 255$, and the observed time exceeds this by $0^s \cdot 154$. If we suppose there are four vibrating segments, the observed time is $0^s \cdot 218$ greater than the computed time of an air vibration. If we suppose there are six vibrating segments, the observed time is $0^s \cdot 281$ greater than the computed time of an air vibration.

If now we compare the results obtained at these three localities, we shall find that we may make such a supposition with regard to the number of vibrating segments into which the column of air divides itself, that the apparent retarding effect of the water shall be nearly proportional to the thickness of the sheet. The following table presents a summary of these results.

	Depth of water = D.	Observed time of one vibration.	Number of vibrating segments supposed.	Computed time of an air vibration.	Retardation produced by water = R.	Quotients. $\frac{R}{D}$
South Natick	5·06 inches,	$0^s \cdot 138$	1	$0^s \cdot 100$	$0^s \cdot 038$	·0075
	10·00 "	0 ·214	1	0 ·100	0 ·114	·0114
Holyoke	13·12 "	0 ·236	6	0 ·155	0 ·081	·0062
	20·90 "	0 ·445	3	0 ·310	0 ·135	·0064
	40·63 "	0 ·739	2	0 ·480	0 ·259	·0064
	45·28 "	0 ·752	2	0 ·480	0 ·272	·0060
Lawrence	48·00 "	0 ·409	4	0 ·191	0 ·218	·0045
	48·00 "	0 ·409	6	0 ·128	0 ·281	·0059

Column fourth shows the computed time of an air vibration, when the number of vibrating segments is assumed as in column third; column fifth shows the difference between the numbers in columns second and fourth, being the retarding effect of the sheet of water according to the theory under examination; and column sixth shows the quotients obtained by dividing the numbers in column fifth, by the numbers in column first. These quotients, with one exception, are nearly constant, indicating that the retarding effect of the sheet of water is nearly proportional to its thickness. The exception to this general rule which occurs at South Natick, may perhaps be ascribed to the small height of the fall, in consequence of which, especially when the water is high, the descending sheet acquires somewhat of the rigidity of a solid body.

The number of vibrating segments of the column of air, is the principal unknown quantity which renders our conclusions so very uncertain. I do not know of any theory which will enable us to compute the precise influence of a sheet of water of given dimensions; but at present, it seems probable that the vibratory motion originates in the column of air behind the sheet of water, and that the descending sheet serves merely as a load, to retard the velocity of these vibrations.

This theory will be fully established or disproved, when we are able to compute the effect produced upon the vibrations of a column of air, by a descending sheet of water of given dimensions.

Why are not these vibrations noticed wherever water pours over a dam?

It is believed that most water-falls exhibit some degree of vibratory motion at certain stages of water; but in order that these vibrations may be powerful and long-continued, the edge of the dam must be horizontal, and quite smooth; otherwise the thickness of the descending sheet will not be uniform; and the sheet will swell into ridges in some places, while other parts become thin. The sheet will divide in some places before reaching the bottom of the fall, and this leaves an opening in the enclosure which contains the column of vibrating air.

This is probably the reason why many water-falls never exhibit this phenomenon in a palpable manner; and why in only a few cases is the vibration so powerful as to cause any annoyance.

Why is a particular height of water requisite to produce these vibrations?

The descending sheet of water must have a thickness of several inches; otherwise it is divided by the action of the air, and the column of air ceases to be enclosed on all sides. With a fall of nine feet, as at South Natick, a thickness of 4 or 5 inches is requisite; and with a fall of 30 feet, as at Holyoke, a thickness of nearly a foot is requisite. At Lawrence, with a fall of 34 feet, the vibrations are not noticed when the depth of water is much less than 3 feet; but this seems to be owing to the inequalities on the top of the dam, resulting from the iron bars being bent over by the spring freshets, and confining some of the flash boards to the top of the dam.

The vibrations cease almost entirely when the water exceeds a certain height, because the thickness of the sheet becomes too great in comparison with its height, and there being some cohesion between the particles of the liquid, the sheet partakes somewhat of the rigidity of a solid body. In order to produce a strong effect, the thickness of the sheet must not exceed about one-sixth or one-eighth of the height of the fall. At South Natick, with a fall of 9 feet, which is somewhat diminished by the back water at the time of a freshet, the vibrations cease when the depth of water much exceeds ten inches. At Holyoke, with a fall of 30 feet, which is also diminished by the back water at the time of a freshet, the vibrations cease when the depth of water much exceeds 5 feet. At Lawrence also, where the fall is a little greater than at Holyoke, the vibrations cease when the depth of water on the crest of the dam much exceeds five feet.

ART. XXXI.—*On the Rocks of the Quebec Group at Point Lévis*, (being a letter to Mr. JOACHIM BARRANDE of France, from Sir WILLIAM E. LOGAN, Canadian Geological Survey.)[1]

Montreal, 15th March, 1863.

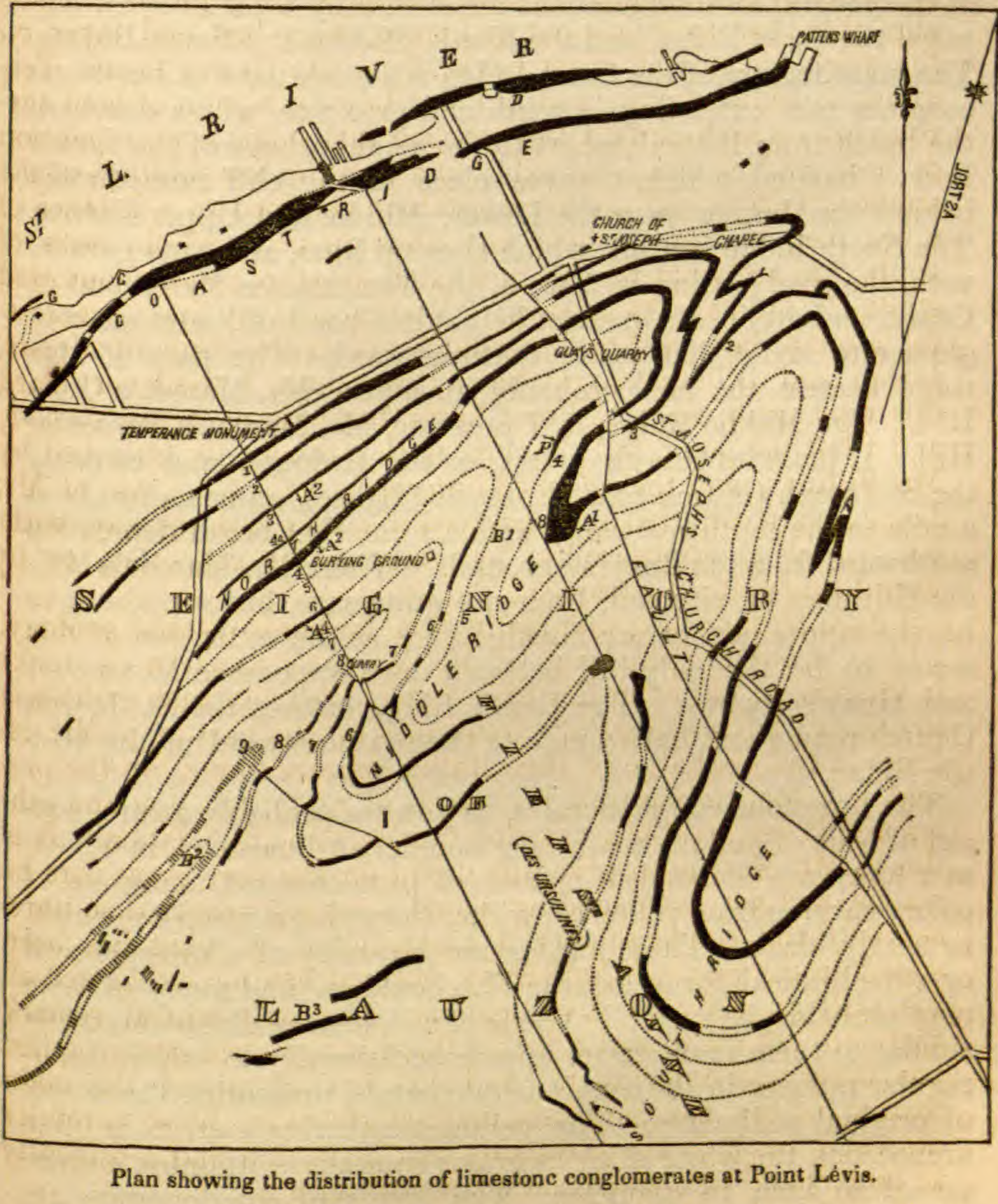
MY DEAR MR. BARRANDE,—*Mr. Jules Marcou* has addressed to you a letter dated the 2d August, 1862, on the Taconic rocks of Vermont and Canada, in which he says, on page 10, "I was able this year to follow out and trace every bed and layer on the whole contour of Point Lévis, from the Grand Trunk Terminus to Indian Cove; and as Point Lévis is a point of land surrounded by high cliffs, I feel satisfied that there is no repetition of beds, and no synclinal axis; and that the few foldings of the strata at Ferry's cliff are mere accident, confined to a distance of a few feet, and are without any effect upon the whole mass of strata, but are what we call in French *structure ployée* (contorted strata)." On page 14 he says: "Fearing that my first unsuccessful attempt last year to understand the explanation of Messrs. Logan and Billings might be my own fault, I tried very hard this year again, when at Point Lévis, but with no better success, and I left the point, fully convinced that the fossils described by Mr. Billings, and the so-called outcrops, A^2,A^3,A^4, &c. of Mr. Logan, were collected and observed in a very careless way, without regard to stratigraphy, by irresponsible collectors, or by unskillful practical geologists."

I have neither time nor inclination for controversial geology. I have never criticised any of Mr. Marcou's remarks on rocks in Canada, or out of it, nor have I suggested any such criticisms to others; but a charge of carelessness on the part of public officers in the discharge of their duties appears to me, on the present occasion, to require a few words of reply, lest you and others might suppose the accusation to have some foundation. It is due to Mr. Marcou to give him credit for the very great care he claims, as I am persuaded he would not have ventured so unreservedly and condemnatory a contradiction of what has been stated on the part of the Survey, without having exhausted all his skill on his own investigation. The only critical remark therefore left me to make, is that this distinguished stratigraphist has been very unfortunate; and that having missed the main feature of the conspicuously marked structure he so carefully searched for, it is not surprising that he should find a difficulty in understanding a statement connected with it.

In 1854 and 1856, a considerable time was expended by Mr. Richardson, one of my assistants, and myself, in ascertaining by

[1] Communicated to this Journal by Sir WM. E. LOGAN.

measurement the position and extent of all the exposures of the limestone conglomerates which characterize Point Lévis. The result of this work was exhibited by me to Mr. Marcou, at the office of the Survey, in 1861, on an unpublished manuscript map, on a scale of six inches to one mile, showing nearly all the known exposures of rocks of the Quebec group for about twenty miles below, twenty miles above, and nearly twenty miles to the south-eastward of Quebec. This map represents an area of 800 square



Plan showing the distribution of limestone conglomerates at Point Lévis.

miles, on which all the exposures are laid down by admeasurements, comprising the work of one member of the Survey for two seasons, and of another for one season. The measurements at Point Lévis I have recently re-protracted on the same scale, with a view of completely separating what is exposed to view, from what is inferred; and a plan reduced from this to one half, by photography, accompanies the present communication.[3] The topographical as well as the geological features are delineated from the measurements of the Survey.

On this plan, the heavy black bands represent the known exposures of the limestone conglomerates; the dotted lines between different exposures represent their supposed connection. Some of the geographical undulations are shown by what I have designated the Coast Ridge, and the North, Middle and South Ridges. The main feature of the Coast Ridge is a thick band of limestone conglomerate extending in a hill and precipice, which overlook the beach from Patten's wharf to the neighborhood of the Lower Ferry; beyond which it gives place to the cliff immediately behind the houses near the Lower, Middle and Upper Ferries. The North Ridge is a hill which rises up from, and runs parallel with, the road passing in front of the Temperance Monument or Cross; and attains its greatest height in a band of limestone-conglomerate about 300 yards southeastward. The part of this ridge nearest the road probably constitutes Mr. Marcou's Cross Hill. The Middle Ridge, is, I presume, Mr. Marcou's Parochial Hill. It includes Guay's quarry, or the Redoute, and crossing the St Joseph Church road (Route de l'Eglise), extends for about a mile to the southwestward, with a somewhat broad depression southward from the Burying-ground. Where Mr. Marcou's Middle Hill may be situated, I am not quite sure, but suppose it to be the upper part of my North Ridge, as the extension of this seems to be the only hill between the Temperance Monument and Guay's quarry. The South Ridge crosses the St. Joseph Church road about half a mile to the southeastward of the Middle Ridge.

The limestone conglomerates, as you are probably aware, consist of beds of yellow-weathering magnesian limestone, in which, as a base, are embedded masses of pure compact limestone, of colors varying from yellowish-white, through gray and brownish, to nearly black. These masses are generally of a sub-spherical or sub-elliptical form, looking like boulders, and many of them may probably be such; but beds of a limestone almost precisely similar to them in character appear occasionally to run in an irregular manner in the conglomerate bands, presenting the aspect of original sediments. The yellow-weathering matrix is often arenaceous, the white silicious grains sometimes attaining a quarter of an inch in diameter. The bands of conglomerate are separated from one another by greenish and blackish slates, which, in many places, are interstratified with strong yellow-

[3] The essential portion of the map referred to is reproduced on the accompanying woodcut without change of scale.—Eds.

weathering gray and black calcareo-magnesian slates, and occasionally with yellow-weathering sandstones. In a few places, red slates are intermingled with the others.

Southeastward from the St. Lawrence, the limestone conglomerates of Point Lévis are distributed over a breadth of more than two miles. In the North Ridge, there are four bands, numbered 1, 2, 3, 4, on the map; on which is represented, in addition, a long lenticular bed (4^a) subordinate to 4, but separated from it by slate. The lenticular bed is composed of brown-weathering magnesian limestone, but appears to contain few or no enclosed masses of the pure limestone. The bands 3 and 4 are, respectively, A^2 and A^3 of a former description. You will perceive that northeastwardly they converge a little; and at the time of that description, it was not determined whether they were to be considered two distinct beds, or one a repetition of the other. They are now taken to be two distinct beds. Followed northeastwardly, they appear to be dislocated by a fault near the St. Joseph Church road; but beyond this they are easily traceable around the extremity of a trough, with a deep channel worn between them in the slate. After passing the axis of the synclinal, the band 4 comes to the limestone of Guay's quarry, which is nothing more than a large lenticular mass of pure limestone, subordinate to the band. Southwestward of the quarry, both bands are seen again crossing the St. Joseph Church road, and again coming against the transverse fault. This fault appears to show an upthrow on its southwest side; since on that side the opposite outcrops of the trough are thrown towards the centre.

Continuing to trace the outcrops on the southern side of the trough, that of band 4 gradually thins, and disappears at P, in less than a furlong; while that of the band 3 becomes more conspicuous, and shows a great development as it folds over an anticlinal axis just eastward of the eastern boundary of the fief Ste. Anne. From this it returns towards the Church road, but becomes concealed about fifty yards before reaching it, after again showing the effect of the fault, in a much smaller horizontal displacement than before. On the northeast side of the anticlinal axis, on both sides of the fault, the dip is to the southeastward, and is therefore overturned; but from the character of the displacement, it is evident that beneath the surface, on the northeast side of the fault, the inversion must be compensated for by a change to the northwest in the slope.

A little above the outcrop of band 4, at P, there occurs a layer of sandstone, which is traceable on the fief Ste. Anne over the anticlinal axis; and a sandstone approaches the outcrop of band 3 at A^1. In the description of 1860, this was supposed to show that possibly the stratigraphical place of the band 4 might gradually approach the band 3, and finally merge into it; but finding farther on, along the outcrops, an exposure of conglomerate at *z*, which will answer for band 4, it is now conceived that there may be two layers of sandstone, one above and the other below the stratigraphical place of band 4; and though this band thins to nothing at P, it may commence again in its relative place farther on.

From the neighborhood of the Temperance Monument, the outcrop of band 2 is traceable northeastward, running not quite parallel with 3, to the fault, and thence across the St. Joseph Church road to the main road. It traverses this obliquely, a little beyond the church, and its turn upon the synclinal axis is seen on the north side of the road, about 400 yards beyond. In the limestone of Guay's quarry there is a small notch-like turn, which serves to augment somewhat its apparent volume; a corresponding twist is more conspicuous in the outcrop of band 3, and in band 2 it assumes a still further prominence at *y*. These successive forms indicate a plait in the stratification, commencing at the quarry, and rapidly augmenting northeastwardly in the space of 350 yards. The importance of its effect on the distribution of the strata would, at this rate of increase, soon become considerable, and it serves to show some of the complications of the neighborhood.

Without going into detail, it is evident from the map that the Middle Ridge is an anticlinal form, and that the South Ridge is another. On this, the exposures of the bands 2 and 3 conspicuously mark the turn on the axis, as they do in the synclinal between the ridges. It will be perceived that, between the synclinal and anticlinal axes, the outcrop of band 2 is represented as showing a very sharp twist. The evidence of this is not quite satisfactory, and the apparent arrangement may possibly be due only to a swelling in the volume of the band, with parts obscured by drift.

The Temperance Monument stands on band 1, with which are associated some layers of sandstone. This band is easily traced to the northeastward across the fief Ste. Anne; but between that and the fault it becomes broken down and obscured, and it will require further investigation. Nothing like it, nor indeed any conglomerate band, has been yet observed following, in its relative place, the sinuosities of band 2, where the strata are affected by the synclinals and anticlinals that have been described. Eastward of the fault and northward of band 2, there is an exposure of conglomerate close upon the southeast side of the main road; the bearing of which would carry it under the church of St. Joseph, and two years ago it was observed in an excavation for the foundation of a house on the northwest side of the road, close by the church. In the strike of these exposures, about 400 yards beyond the church there is a band of conglomerate, which continues in the same strike for about a hundred yards. This strike would carry the band away from those of the North Ridge, and gradually bring it towards those of the Coast Ridge, and it appears probable that the bands of the Coast Ridge may be only a repetition of some of those of the North Ridge. The main band of the Coast Ridge is associated with several beds of sandstone; and, from its great breadth, it may possibly be capable of division into more than one mass of conglomerate. To the southwestward of the extreme point to which this band has been traced, there occurs in the cliff, to the southeast of the Lower Ferry, the band A; one of those referred to in the description of 1860. Its exact relation to the other bands has not yet been satisfactorily determined.

Southward of A^3 you will remark A^4, and you will perceive that these two bands somewhat converge to the southwest, in which direction they are not traceable for over a quarter of a mile. At the time of the previous description, it was left undecided whether these were to be considered distinct bands, or a repetition of one another. They are now assumed to be distinct. On the Middle Ridge, the band 4, at P, is followed by B^1; which is a band of slate with nodules of limestone. On the North Ridge, its place would be between A^3 and A^4. It would therefore be band 5, and A^4 would be band 6. The bands 7, 8, and 9 succeed on the north side of the Middle Ridge, the band 9 being B^2 of the former description; like B^1, it is composed of slate studded with nodules of limestone. This band appears to have a considerable development southwestwardly, in a long shallow trough-like form, extending to the Grand Côte road. From this, its outcrop returns on the south side of the Middle Ridge anticlinal, and points to B^3; which however differs from it in character, having a base of magnesian limestone instead of slate. What is seen of the band B^3 is broken into three portions by transverse faults. It is evidently on the south side of the Middle Ridge anticlinal, and may correspond with band 8, but this has not yet been satisfactorily made out; nor has it yet been found possible to arrange the complicated exposures to the southeast of it, on the South Ridge.

On the southwest boundary of the fief Ste. Anne, near the quarry there indicated, the beds appear to be dislocated on the north side of the Middle Ridge anticlinal, by faults, which do not affect the outcrops on the south side. These faults may be small breaks accompanying twists in the strata, the connecting parts of which may be concealed by drift; but it would require additional facts to make their arrangements certain. Though the number of bands is assumed to be nine, some of them may be repetitions through the effect of plaits suddenly starting up, like that at *y*, or through undetected faults running with the stratification. The distribution of the outcrops in the southwest part of the South Ridge shows the very complicated character of the disturbances, and is a warning against over-confidence in respect to minute details. In regard to the main features of the structure however, there appears to be no doubt; namely, that the Middle and South Ridges are two well marked anticlinals, and that a synclinal, not less so, runs between the Middle and North Ridges, repeating the whole mass of strata.

From the foregoing explanations, you will be able to understand how the fossils enumerated in the description of 1860 are related to the conglomerate bands, as represented on the map. The whole of these fossils were collected by the officers of the Survey, who are all perfectly aware of the importance of observing the exact stratigraphical place of the organic remains, and always most carefully do so. The collectors were Messrs. Billings, Richardson, Bell, and myself; and, from the statements made to me by my colleagues and assistants, I am quite prepared to assert that the specimens referred to B^3, B^2, B^1, A, A^1, and A^3, are from the bands marked on the map by those letters. With the exception of a single specimen of the pygidium of *Bathyurus Saffordi*, obtained by Mr. T. Sterry Hunt from the band 4 (A^3), where it crosses the more northern synclinal axis near the Redoute, the band A^2 afforded to my late regretted and talented young scientific friend, Mr. John Head, and myself, the first collection of fossils obtained by the Survey at Point Lévis. These were taken from the whitish limestone masses associated with the bed, where it crosses the fief Ste. Anne, and the opinion in regard to them expressed by Mr. Billings induced me to instruct Mr. Bell to make a further collection on the same band. In addition to the fossils collected by Mr. Head and myself from the band, there are some by Mr. Richardson, and others by Mr. Bell, all from the fixed rock; but in Mr. Bell's collection there are, in addition, those from the limestones designated by Mr. Billings as Nos. 1 and 3. These limestones were not, like the rest, firmly attached to the band, and as they have been by Mr. Marcou designated as two loose boulders, lying on the superficial soil, while he carries them away from their true site, and approximates their position to the lime-kiln of the Redoute, in order to affiliate them to that mass, it will be necessary for me to describe their mode of occurrence.

On the fief Ste. Anne, the band 3 (A^2) dips to the southeast at a high angle. It is from about twenty to twenty-five feet thick, and in its calcareo-magnesian base it holds a great many masses of yellowish-white limestone, in which fossils are apparent and somewhat abundant. It is underlaid by slates; and in some parts a sudden step to the underlying slates occurs at its northern edge. At the foot of this step, Mr. Bell observed in one place a mass of gray-weathering yellowish-white limestone protruding for a few inches through the soil. This mass, when excavated from its position, proved to be about a foot in diameter, and very fossiliferous. Persuaded that it had fallen from the conglomerate band, he tried farther on in the strike and found another; and, finally, in the distance of about fifty feet along the strike, he obtained five masses, each as heavy as would require a strong man to lift, and twelve smaller masses, each of about twenty pounds weight and upwards. They were all rich in fossils. Some of these gave to Mr. Billings his limestone No. 1, and others that of No. 3. All of these masses, some of which were sharply angular, rested on the slate, just at the base of the conglomerate band; and, with the exception of the small portion of the first one, were wholly covered by the soil, one of them to the thickness of a foot, requiring, before it could be extracted by aid of pick, shovel, and crow-bar, a hole to be made of two feet deep. It appears to me much more probable that these masses should have fallen from the conglomerate band which they touched, than that they should have been transported nearly half a mile from the Redoute, and all laid at the foot of the conglomerate band A^2, in a row in its strike. It is by no means supposed that the stock of these masses was exhausted by Mr. Bell; more may probably be obtained in the strike, and I am persuaded that, if the adjacent parts of the conglomerate band were laid bare, similar masses would be found imbedded in it.

Mr. Marcou states that the limestones Nos. 1 and 3 without doubt come from the Redoute; and that in respect to No. 1, so rich in trilobites, he could almost point out the exact spot from which it came. Soon after the first discovery of fossils at Point Lévis, I spent a good deal of time in endeavoring to obtain specimens from Guay's quarry, but with very indifferent success. Fragments of trilobites were observed, but the only recognizable species obtained was *Menocephalus globosus*. Perceiving that Mr. Marcou had been so fortunate as to meet with upwards of nine species of trilobites in the locality, I last season renewed my attempt; and, with Mr. Billings, made a diligent search of the rock, but with no better luck than had attended my previous researches—*Menocephalus globosus* being again the only species procured. Mr. Marcou states that the stratification is indistinct, and that in consequence of the hardness of the stone, it is difficult to obtain specimens. This perfectly accords with what we observed; but not with the characters of the limestones Nos. 1 and 3; which are not very hard, and in which the fossils occur in layers, marking well the stratification. The limestones split with moderate facility in the direction of those layers, and give considerable planes of surface, with fossils starting prominently up from them. I presume, therefore, that the beds at the Redoute, with which Mr. Marcou compares the limestones No. 1 and 3, are some which he has not yet described, and with which we can make no comparison, as we have not been so fortunate as to find them.

Since 1860, Mr. Devine and Mr. Cayley, both of the Crown Lands Department, have obtained several species at Point Lévis. The latter gentleman discovered *Amphion Cayleyi* (Billings) in band 3 (A^2), on the North Ridge; and Mr. Devine, on the same ridge, has procured *Bathyurus Saffordi* from band 2, *Menocephalus globosus*, and *Cheirurus Eryx* from band 3 (A^2); and from band 4 (A^3) *Bathyurus Saffordi*, *B. Cordai*, and *B. bituberculatus.* But from this band he has made a very important addition to the fauna of Point Lévis, in a perfect specimen of what Mr. Billings agrees with him in considering an *Olenus*, or of a closely allied genus. This was obtained on the North Ridge, just east of the fief Ste. Anne, in a mass of drab-colored limestone, which Mr. Devine thinks is a part of the solid band, although he has not yet tested the matter sufficiently to be positive. The same part of this band here holds *Obolella*, *Orthis Evadne*, *Camerella Calcifera*, *Pleurotomaria*, *Ecculiomphalus Canadensis*, *Orthoceras*, *Agnostus Americanus*, *A. Canadensis*, *A. Orion*, *Arionellus subclavatus*, *Bathyurus capax*, *B. quadratus*, *B. Saffordi*, *Cheirurus Eryx*, *C. Apollo*, *Dikelocephalus magnificus*, *D. megalops*, *D. planifrons*, *D. Oweni*, *Menocephalus Sedgwickii*, and *M. Salteri.* In this collection, the species of *Pleurotomaria*, *Ecculiomphalus*, and *Cheirurus* do not occur in the same hand-specimens of rock with the others. *Bathyurus Saffordi* is in the same specimen with *Menocephalus Salteri.* On the Middle Ridge, he has obtained *Menocephalus globosus* from band 4, at the Redoute. Mr. Billings has obtained in band 2, on the North Ridge, *Bathyurus quadratus;* on the Middle Ridge, in band 6, on the north side of the anticlinal, *Leptæna decipiens;* and the same species in band 7, on the same side of the anticlinal; while band 7, on the south side of the anticlinal, has yielded him a *Pleurotomaria*, allied to *P. Laurentiana*, *Orthoceras*, *n. s.*, *Illænus* ———, and *Asaphus* ———. In a band of conglomerate forming two successive mounds at the water's edge, northwest of the Coast Ridge and running parallel with it, he has met at D with a new species of *Dikelocephalus.*

To make the distribution of the fossils, which we in Canada (including Mr. Devine and Mr. Cayley) have obtained at Point Lévis, more clearly understood, a catalogue of them has been prepared, with the specific names of those which have been described and a separate column for each of the bands, and made a part of the present communication. In this catalogue, no certain stratigraphical place is assigned to the bands D, G, and A, in relation to the others—which, from 1 to 9, are supposed to be in ascending order. With the exception of those otherwise marked, all the determined species have been described by Mr. Billings.

Mr. Marcou, it appears to me, has gone somewhat out of his way to insinuate a discourtesy towards you on the part of the Canadian Survey, in that we have, as he says, distributed fossils of the Quebec group in England to more favored geologists than yourself. Mr. Marcou could not have stated this from his own knowledge, as it is not consistent with fact. The truth of the matter is precisely the reverse of this. We long ago did ourselves the pleasure of transmitting to you a small collection of the principal species; while we have presented none to any other of our geological friends in Europe. On this side of the Atlantic, we have exchanged a few specimens with Col. Jewett, of the New York State Museum, for New York species, of which we stood greatly in want; and we are just now about to make a small exchange with Mr. A. H. Worthen, State-geologist of Illinois, for species from several of the Western States, of which we have long been anxious to possess authentic specimens. Mr. Marcou seems especially aggrieved that he did not obtain a pygidium of *Dikelocephalus magnificus*, asked for, as he states, in your name. This was during my absence in England at the International Exhibition. Mr. Billings cannot call to his recollection that the application was made in your name. Such an application would have afforded him the opportunity of informing Mr. Marcou that you were probably already supplied in the collection sent; but it would not have altered the propriety of what, in conformity with his duty, he found himself under the necessity of replying; namely, that he was not authorized to distribute the specimens of the Provincial Collection.

I am, my dear Mr. Barrande, yours very truly,

W. E. Logan.

Mr. Joachim Barrande, Rue Mezière No. 6, Paris.

Catalogue of Fossils from the Quebec Group, collected at Point Lévis.

	D	G	A	1	2	3	4	5	6	7	8	9
Tetradium ?........................		*										
Graptolithus, *several sub-genera* (Hall)		*					*					
Lingula Mantelli..................						*	*					
" Irene....................		*										
" Quebecensis			*									
Obolella Ida......................						*	*					
" desiderata			*									
Acrotreta *n. s.*						*						
Leptæna decipiens								*	*	*	*	*
" sordida								*				

	D	G	A	1	2	3	4	5	6	7	8	9
Leptæna, *undescribed* 1.								*				
" " 2.						*						
" " 3.							*					
Strophomena, *undescribed*			*									
Orthis gemmicula								*				
" Tritonia								*				
" orthambonites (Pander)								*				
" Euryone								*				
" Electra								*				
" Hippolyte								*				
" Evadne								*				
" Mycale								*				
" Eudocia								*				
" Quebecensis			*									
" *undescribed* 1.						*						
" " 2.							*					
" " 3.							*					
" " 4.												*
Camerella Calcifera						*	*					
Stricklandia? Arachne								*				
" Arethusa								*				
Cyrtodonta? *undescribed*								*				
Ecculiomphalus Canadensis							*	*				
" intortus							*					
Pleurotomaria vagrans							*					
" Postumia							*					
" Quebecensis							*					
" *undescribed* 1.							*	*				
" " 2.							*					
" " 3.											*	
" " 4.											*	
Murchisonia, *undescribed*, 1.							*					
" " 2.							*					
" " 3.							*					
" " 4.											*	
Helicotoma perstriata							*					
Ophileta uniangulata (Hall)							*					
" *undescribed*, 1.							*					
" " 2.							*					
Maclurea Atlantica											*	
Holopea dilucula (Hall)							*					
" *undescribed*							*					
Metoptoma Melissa							*					
" Hyrie							*					
" Orphyne							*					
" Venillia						*	*					
" anomala							*					
Metoptoma Augusta							*					
" superba							*					

	D	G	A	1	2	3	4	5	6	7	8	9
Orthoceras Autolycus.............							*					
" *undescribed* 1.							*					
" " 2.							*					
" " 3.							*					
" " 4.							*					
" " 5.							*					
" " 6.												*
Cyrtoceras Metellus							*					
" Dictys...............							*					
" Alethes							*					
" Mercurius							*					*
" Syphax												
" *undescribed*							*					
Nautilus "												*
Agnostus Americanus						*	*					
" Orion						*	*					
Agnostus Canadensis						*	*					
Amphion Cayleyi...............						*	*					
Ampyx, *undescribed*							*					
Arionellus cylindricus						*						
" subclavatus...........						*						
Asaphus Illænoides						*						
" goniurus						*	*					
Bathyurus capax						*						
" dubius...............						*						
" bituberculatus.........						*						
" armatus..............						*	*					
" Saffordi..............							*					
" oblongus.............							*					
" Cordai...............							*					
" quadratus					*		*					
Cheirurus Apollo							*					
" Eryx.................							*					
Conocephalites Zenkeri						*	*					
Dikelocephalus magnificus........						*	*					
" planifrons						*	*					
" Oweni...........						*						
" Belli						*	*					
" megalops.........						*						
" cristatus..........						*						
" *undescribed*	*											
" (Olenus) Logani (Devine)							*					
Endymion Meeki...............											*	
Holometopus Angelini...........											*	
Illænus, *undescribed*											*	
Leperditia.....................											*	
Menocephalus globosus...........						*	*					
" Sedgwicki.........						*	*					
" Salteri (Devine).....							*					
Nileus, *undescribed*..............							*				*	
Shumardia granulosa............				*								

ART. XXXII.—*Chauvenet's Manual of Spherical and Practical Astronomy.*[1]

[SECOND NOTICE.]

THE publication of this work opens a new era for the student of astronomical science. Henceforth, the study of spherical and practical astronomy is a very different thing from what it has been with all the aids previously existing in the English, French and German languages. That these assertions may not seem exaggerated, we propose to give a somewhat detailed account of those parts of the work which are original with the author, and which we think of sufficient importance to establish the truth of our assertions.

Chapter I, on Spherical and Rectangular Coördinates, initiates the student into the method of applying spherical trigonometry to astronomical problems, in the most general manner. The expressions for the rectangular coördinates of a point in space, in terms of the distance of the point from the origin and of the spherical coördinates, are deduced in a new and extremely simple way.

Chapter II, on Time and the use of the Ephemeris, is unusually complete on various minor points which are apt to embarrass beginners—such as the conversion of mean or apparent solar time into sidereal time; the deduction of local time from the given hour angle of any celestial body, and the inverse problems; the management of interpolation formulas in consulting the ephemeris, etc.—with all of which the young astronomer must be perfectly familiar before he can proceed.

Chapter III, on the Figure of the Earth, does not treat the methods of determining the earth's figure, which is left to geodesy, but gives the geodetic formulas, with their demonstration, which are necessary to the astronomer in reducing observations to the centre of the earth. Bessel's dimensions of the terrestrial spheroid are adopted. The author has not forgotten to notice the "abnormal deviations of the plumb line," which render it necessary to distinguish between the *astronomical* and the *geodetic* latitude or longitude of a point on the earth's surface; deviations which were first pointed out and also accurately determined by our own admirable Coast Survey. Now that the existence of such deviations is established, and the method of eliminating them pointed out, it is certainly desirable that a new measurement of the earth's dimensions should be entered upon by the leading nations of the world. It can hardly be doubted that the earth will be found to differ sensibly from an

[1] A Manual of Spherical and Practical Astronomy. By WILLIAM CHAUVENET, LL.D., Chancellor of Washington University, St. Louis, Mo. Published by J. B. Lippincott & Co., Philadelphia.

ellipsoid of revolution, but it may be expected that, by eliminating as far as possible the anomalous deviations in question, with the aid of the elements determined by Bessel, a very close approximation to the true figure will be obtained.

Chapter IV, on the Reduction of Observations to the Centre of the Earth, another preliminary chapter, is almost exhaustive upon the subjects of Parallax, Refraction, Dip of the Horizon, etc. The author presents the whole of Bessel's theory of refraction (based upon Kramp's and Laplace's) in a most lucid and masterly manner. A complete view of the present state of the refraction theory, however, requires a consideration of Ivory's investigations at least, to say nothing of those of Plana and others; but it would, perhaps, have occupied too large a space in the work before us. Mr. Chauvenet adopts Bessel's Tables, as thus far agreeing best with observation, and therefore limits himself to an explanation of the principles by which that table is constructed. We notice that the author has deduced from theoretical considerations a formula for the dip of the horizon, including the effect of refraction, which agrees remarkably with the results of actual observation. The effect of refraction upon the figure of the discs of the sun and moon is also investigated.

Chapter V, on Finding the Time by Astronomical Observations, brings us fairly into the subject of practical astronomy, to which the preceding chapters are but introductory. This, together with Chapter VI, on Finding the Latitude, and Chapter VII, on Finding the Longitude by astronomical observations, appears to embrace every previously known method of any value, with several new and simple methods, and is accompanied by numerous practical precepts which are designed to inculcate accurate habits of observation and precision in computation. Of the many approximative methods of "working a lunar distance," which have heretofore been given, Prof. Chauvenet gives but one, and that is his own. Any one who will take the trouble to read the investigation of the formulas upon which his method rests, and by which his auxiliary tables are formed, will be convinced that he was entitled to prefer this method to all others. It is in the first place, *accurate* to a degree unapproached by any previous method except Bessel's; in the next place the auxiliary tables are extremely simple and concise, and do not require vexatious double interpolations, for the most part, indeed, requiring no interpolations at all; and, lastly, the form of the computation is quite symmetrical and certainly within the grasp of even the most *un*mathematical navigator. Although the method was given to the world in the first volume of our American Ephemeris in 1855, we do not remember having seen it in any of the nautical works of this country or Europe.

We see the old and demonstrably imperfect methods still reappearing in every edition of Bowditch's Navigator, and the English practical works of Raper, &c. Under the head of moon culminations we notice that the author gives an entirely new formula for combining observations according to *weight*, and explodes the old method of regarding the weight of a moon's transit as proportional to the number of threads on which the transit has been observed. He has also incorporated Prof. Peirce's valuable researches on this subject.

The "American Method" of determining longitudes by the electric telegraph, as developed by the Coast Survey is, of course, fully treated, and illustrated by examples.

We are glad to see that the method of finding the longitude by *altitudes* of the moon—discarded by some recent writers—is here restored to its proper place. By the differential comparison of the moon's limb with a neighboring star, as suggested by Prof. Kaiser of Leyden, the method admits of a very great degree of accuracy, and is practical with portable instruments, and especially with the zenith telescope as now constructed for the Coast Survey.

Chapter VIII, on Sumner's Method of finding a ship's position at sea, and Chapter IX, on the Meridian line, and Variation of the Compass, are both brief and need no especial notice.

Chapter X, on Eclipses, is the most considerable chapter of the work. Besides considering all the questions usually discussed, our author goes into a wholly new discussion of the *Occultation of Planets*, in which the elliptical outline of the disc of a spheroidal planet partially illuminated is taken into account. Prof. Chauvenet's methods of solving the various problems relating to the prediction of solar eclipses for the earth, though based upon Bessel's fundamental formulas, are also original, and combine great accuracy with brevity of computation. Since the elaborate paper on eclipses by Mr. Woolhouse in the British Nautical Almanac for 1836, we believe nothing so complete has been produced on this topic; but our author's discussion is not only more complete than that of Mr. Woolhouse, but is both much more elegant in form and more precise. This chapter fills 166 royal octavo pages, and well deserves to be separately published as a complete monograph on Eclipses; for it embraces a discussion of all the phenomena which are included under that name, *i. e.* solar and lunar eclipses, occultations of fixed stars and planets, and transit of the inferior planets over the sun's disc; together with the best methods of applying the observations of these phenomena to the determination of terrestial longitude, or of the solar parallax.

Chapter XI, on the Precession, Notation, Aberration, and annual Parallax of the Fixed Stars, it is sufficient to say, contains all the matter upon these subjects which belongs to a treatise on Spherical Astronomy, where the physical theory of precession and nutation would be out of place. Chapter XII is equally complete upon the methods of determining the Obliquity of the ecliptic and the absolute Right Ascensions and Declinations of stars by observation.

The first volume concludes with Chapter XIII, on the Determination of Astronomical Constants by observation. This is a brief but clear *resumé* of the methods of determining the constants of the refraction formula, the solar parallax, the mean semidiameters of the planets, the constants of precession, nutation, and aberration, the parallax of a fixed star, and the motion of the sun in space.

Volume II is devoted to the Theory and Use of Astronomical Instruments. Chapter I relates to the Telescope considered apart from any form of mounting, and gives the methods of determining the magnifying power, and a number of practical precepts necessary to the observer.

Chapter II, on the Measurement of Angles, or Arcs, in General, treats chiefly of the errors of graduated circles, of the methods of determining and eliminating eccentricity, periodic and accidental errors, etc., according to the received methods introduced by the German astronomers. The filar micrometer is here also treated, and the methods of determining the value of a revolution of the screw are discussed at length. The investigation of the complete formula for determining the value of a revolution by observations upon a circumpolar star at its greatest elongation, is new.

Chapter III, on Instruments for Measuring Time, embraces clocks, chronometers, and the electro-chronograph, the last being properly regarded as having the same relation to a clock that the micrometer has to the graduated circle. The performance of the various chonographs most in use, is illustrated by full sized specimens of actual work at the Harvard College Observatory and other places.

Chapter IV, on the Sextant and other Reflecting Instruments, contains a full discussion of the theory of these instruments with practical precepts concerning their manipulation. The theory is given in an unusually simple form, and yet appears to be quite complete. The new prismatic instruments of Pistor and Martins, are, of course, not forgotten.

Chapter V, on the Transit Instrument, is, like so many other chapters of this unrivaled work, an exhaustive monograph, and fills upwards of 150 pages. The student who makes himself master of all that this chapter contains on this, the primary and fundamental instrument of the observatory, will be fully prepared to undertake the management of any other instrument.

The author has not only digested into a complete system all the valuable material elsewhere given, but has contributed much original matter. The correction of a transit of the moon or a planet when the defective limb has been observed, which we have looked for in vain in treatises on practical astronomy, is here given in its proper place. The discussion of the probable errors of transit observations is especially valuable. So also are the articles on the use of the portable transit instrument, both in the meridian and in the vertical circle of some circumpolar star. The discussion of an actual series of observations made upon our Northwestern Boundary Survey is full of instruction to the young astronomer. The value of Peirce's Criterion for the rejection of doubtful observations, is here very happily illustrated. Bessel's method of reducing transits over several threads to a single instant, when the instrument is not in the meridian, first given in the *Astronomische Nachrichten*, vol. vi, is here for the time incorporated into a treatise as an essential part of the theory of the instrument. The author has simplified Bessel's method, however, and given a new table which is obviously more convenient than the one given by Bessel for the same purpose.

All the various practicable methods of determining the latitude by the transit instrument in the prime vertical are systematically deduced from a single fundamental formula and their several advantages brought out in a very clear manner, with the aid of full illustrative examples from actual observation.

Finally, the method of determining the declinations of stars with the transit instrument in the prime vertical is given, together with the use of the micrometer in such determinations when the star passes very near to the zenith, a part of the subject usually passed over in silence.

Chapter VI, on the Meridian Circle is likewise complete. The flexure of the telescope is considered. Formulas are given for correcting the observed declination of a planet's limb, both for spheroidal figure and for defective illumination. These formulas are new, it being usual to allow for defective illumination upon the supposition that the planet is spherical.

Chapter VII, on the Altitude and Azimuth Instrument, is also a systematic digest of all that is valuable in this connection. For the reduction of observations over several horizontal threads, the instrument being slowly revolved in azimuth, a method of observation practised at Greenwich with the Altazimuth, we see that the author gives a precise method instead of the rough one used at Greenwich.

Chapter VIII, on the Zenith Telescope, explains in minute detail the method of employing this instrument as practised upon the Coast Survey. We are not sure that Mr. Chauvenet is quite right in calling the *instrument* "the invention of Capt. Talcott." We have supposed it was rather an improved form of the old zenith telescope, the micrometer taking the place of a graduated arc, although the readings of the graduated arc in the old instrument were differential, as well as in the new instrument with the micrometer. The methods of observation, however, and the processes of reduction, belong to our Coast Survey, and are in this work very satisfactorily explained. The method given for determining the latitude by extra-meridian observations with this instrument is original.

Chapter IX treats of the Equatorial Telescope, adopting Bessel's method of introducing the flexure of the telescope and of the declination axis, and giving the most general formulas, even for the case where the pole of the instrument is quite remote from the celestial pole. The mode of determining the actual position of the instrument is illustrated by observations made with the great equatorial of the Pulkowa Observatory.

Chapter X, on Micrometric Observations, treats first of the filar micrometer in connection with the equatorial telescope. The correction of micrometer observations for the errors of the equatorial instrument is investigated, a subject we do not remember to have seen elsewhere discussed. The complete theory of the Heliometer, according to Bessel, is next given in a concise and simple manner, yet with the same extreme accuracy that we find in all the other parts of the learned author's work. The Ring Micrometer is next discussed. The correction of all kinds of micrometric observations, for refraction, is then investigated after Bessel's method, and the necessary table for facilitating the application is given; and finally there is introduced the method of correcting such observations for precession, nutation, and aberration. The formula for the latter purpose is the same as that of Bessel, but the method of investigation is extremely simple, and, we presume, altogether original.

One of the most important features of the work throughout may be found in the frequent application of the Method of Least Squares to those problems which permit of its employment. This comparatively new, but most powerful and flexible, implement of investigation here receives, for the first time in any extended treatise on the practice of astronomy, its due place, and is introduced in its appropriate relations and bearings, as experienced astronomers are accustomed to make use of it.

The principles of this method are fully discussed in an appendix of about 100 pages, in which its essential features are elegantly and concisely developed, in the clearest form in which it has ever been our fortune to see them in any language. The mystery and complication which have been not unfrequently supposed to shroud and confuse the subject are exorcised by a master's hand, and not only the deduction of the fundamental and most convenient formulas, but also the distinctions between the various quantities so easily confounded under the general names of "probable error" or "mean error," are made so clear and manifest that the subject cannot appear to the student as either abstruse or confusing.

This appendix is by no means a compilation; it is an original treatise in which previously known formulas are deduced in a new order from the author's own stand-point. Peirce's Criterion for the rejection of doubtful observations is given, together with a very simple criterion proposed by the author, which agrees nearly with that of Peirce.

In conclusion, we would call attention to the intrinsic elegance of the whole treatise, as including a wide field, and not only assigning to each department its own appropriate degree of prominence, but clearly and beautifully presenting them as parts of one whole; thus rendering the work a systematic treatise as distinguished from an aggregate of discussions of different subjects. In the manifest grasp of both the practical and theoretical relations the master's hand is visible. Refined analysis, appreciation of the most convenient application, theoretical accuracy of processes and formulas, practical knowledge of instruments, in short, the requisite, now fulfilled for the first time in a text-book on Practical Astronomy, that the author should be both a mathematician and an observer, all unite to make this new production of Prof. Chauvenet a classic of the highest order, and an invaluable contribution to astronomical science.

ART. XXXIII.—*Remarks upon the causes producing the different characters of vegetation known as Prairies, Flats, and Barrens in Southern Illinois, with special reference to observations made in Perry and Jackson counties;* by HENRY ENGELMANN, Assist. State Geological Survey.

THE district to which I have reference is peculiarly adapted for the study of the causes which produce the differences in vegetable growth, because we find there different systems of vegetation equally well developed in close proximity to each other. We may especially distinguish the prairies, the post-oak flats, the barrens, the post-oak hills, and the corresponding creek-bottoms, and, on a different soil, the white-oak lands.

The prairies in this district invariably occupy the highest ground, but their relative elevation varies considerably. While the lowest edges of some of them reach to within 40, nay, even

20, feet of the level of the principal water courses, the highest prairies are above the heads of swiftly running tributaries of several miles in length. The surface of the prairies is generally flat or gently undulating, and some of them exhibit considerable difference of altitude in their different parts. They are mostly surrounded by timbered flats of more or less extent, and farther on towards the creeks by broken hills. Where there are extensive uplands not level enough to form prairies, but laid out in more or less broken waves, they assume the character of vegetation known in that section as "barrens," while the more broken hills are covered with forest. The differences depend partly upon the configuration of the surface; but other elements must coöperate to produce them: such are unquestionably the quality of the soil, the underlying geological formations, and to a prominent degree the conditions of moisture, as well in regard to the climate or surrounding air, as in regard to the soil and deeper under ground. The geological formations, especially over the more elevated portions of Perry county, consist principally of arenaceous shales and slates and fine-grained sandstones of the upper Coal-measures in nearly horizontal position. The soils and subsoils of the whole district have been formed mainly from their detritus. They are arenaceous, with only a small admixture of clay, and in a high state of comminution, nearly reduced to an impalpable powder. This physical condition produces some properties which we are wont to attribute only to stiff clay. When quite dry the soil rapidly absorbs water; but after having been moist for some time it becomes hardly permeable, the minute particles of the mass soon filling all the pores between the larger grains and closing them hermetically; also whenever water happens to collect in a depression of the surface, the impalpable mud which it carries in suspension soon forms an impenetrable coating on its bottom and prevents the water from sinking. This is analogous on a large scale to what a chemist may daily observe in his laboratory in filtering certain substances. These pools of water remain standing on the ridges, and dry slowly by evaporation. When for some time saturated with water, and especially when mechanically worked in this state either by agricultural implements, or by the tread of cattle, or under the wheels in the roads—when it is puddled as the technical term is —this sandy loam becomes tenacious in consequence of the great adhesive power of its minute particles, and then appears to be far more clayey than it is in reality. Generally it crumbles easily as soon as dry, especially if any mechanical power is applied, and shows again its sandy character. It is not retentive of moisture; in a dry atmosphere it readily gives off the largest portion of the water which it has absorbed, and it reabsorbs the aqueous vapor from the atmosphere with much less power than clay soil does, neither as rapidly, nor in nearly the same quantity.

Occasionally, we also find as substrata a stiff rough clay, and at some points sand.

The prairies all occupy the broad, more or less flat, dividing ridges between the streams. In digging wells on them, water is frequently struck at shallow depths, say, from 6 to 12 feet, and seldom exceeding 30 feet. It is generally obtained in the quaternary surface deposits above the strata of rocks; the latter are seldom reached in the prairies in digging wells, except near their borders, near the flats and barrens.[1] The underlying formations therefore appear not to exercise any direct leading influence in causing the prairie character of the surface, nor does the fine comminution of the soil by itself, because it does not differ in this respect from the adjoining flats and barrens, which is composed of the same sandy loam. I am inclined to the opinion that the leading cause of the prairie vegetation is to be found in the conditions of moisture of the soil, while I concede that various causes must coöperate to prevent the growth of trees, and that in other districts one or the other of these accessory causes may predominate so far as to seem alone to produce this same result.

The prairies in the district under consideration have a very imperfect surface drainage, in consequence of their configuration. The largest portion of the water which falls as rain or snow is therefore taken up by the soil, besides the large quantity which it absorbs directly from the moist air. The first of these sources, the rain and melting snow, would cover the surface to an average depth of 43 inches each year; the quantity of dew cannot well be estimated, but it is quite considerable in some seasons, and the quantity absorbed directly from the moist air, which we are apt to overlook entirely, plays a most important part in the economy of nature, and is large in soils which are rich in humus, like our prairie soils. Prof. Babo has demonstrated that the absorbing power of some dry soils is scarcely surpassed by that of concentrated sulphuric acid, which is used in the chemical laboratories for the special purpose of absorbing the

[1] *Remark.*—In Washington county, which adjoins Perry county to the northward, the same conditions of vegetation prevail, with the only difference that the prairies occupy a still larger proportion of the surface in consequence of the less broken character of the land. There, rocks, mostly soft sandstones, have been struck at numerous points in digging wells in the prairies; but the water is generally obtained either above these rocks, so that only the well beds are excavated in them, or else underneath a few layers merely of these rocks, which then generally reach rather close to the surface, to only from 10 to 20 feet of it. In most of these cases, then, the permanent sheet of water is still near the surface, underneath a rock which is permeable to it, either throughout or locally in consequence of numerous water-bearing crevices. Rock or no rock then makes no essential difference in the drift of our argumentation, which remains unchanged. Points where water is obtained only by considerably deep wells through the rocks come under the exceptions of which I have treated at the end of this article.

[Compare the remarks on the 'Barrens' and 'Sinks' of Kentucky, by B. Silliman, Jr., in an article on Mammoth Cave, this Journal [2], xi, 333, 1850.—Eds.]

humidity of other substances; those rich in humus, clay, or peroxyd of iron, rank highest in this respect. Tile drains in underdrained land of this kind frequently discharge water when the air is moist, but before any rain has fallen. This water has evidently been absorbed from the air by the soil, and this phenomenon proves conclusively the great importance of this absorbing power.

Let us now enquire what becomes of all the water which the prairie soil takes up from the various sources. A portion of it sinks deeper into the substrata and finds an outlet in springs and creeks, but a large portion is retained by the formations nearest the surface, which become saturated with it, so that water can be obtained nearly all year round in shallow wells. Even where the main water level lies deeper, the underclay is generally of such a kind that it allows the water to percolate but slowly. The consequence is, that during that portion of the year when the evaporation is less powerful, in winter and spring, the soil is perfectly soaked with moisture, and the subsoil remains in that state till late in the season. This excess of moisture which must nearly all be exhausted by evaporation naturally affects the growth of plants. It not merely retards their development in the spring by the chilling influence of the evaporation of so much water, but kills and prevents the growth of all those which cannot live with their roots in stagnant water. It also prevents the access to the soil and roots of the oxygen of the air, which is essential to the healthy development of most plants, and without which no oxydation can take place in the soil, especially no decay, no rotting of organic substances, no eremacausis, as Prof. Liebig terms it, which forms an important source of nourishment for the plants. In the absence or under a limited access of oxygen, the organic bodies in the soil putrefy or ferment, whereby much less elements are produced which sustain the life of the plants, but, on the contrary, whereby the roots and the elements of the soil are deoxydized. Acids are thus developed which are injurious to most vegetation; the peroxyd of iron, a highly beneficial ingredient of the soil, is reduced to protoxyd of iron and then dissolved by carbonic and vegetable acids. It then operates destructively upon vegetation, similar to its combination with sulphuric acid, the protosulphate of iron or copperas.

The deeper a plant roots the more obstacles to its growth it finds in the prairies, because it is longer exposed to these baneful influences, which disappear only late in the season, when it is too late for many plants to begin their annual growth. Then the humidity disappears, the soil is opened to the air, and the consequences of the wet spring are overbalanced by the natural richness of the soil. Later in the season these same prairies which have been excessively wet in the spring suffer considerably from drought, in a great measure certainly because the subsoil is naturally too close, and not opened by deep rooting plants, and therefore does not assist in the absorption of the moisture from the air, nor attract much of it from the water below. Grasses which are not only able to withstand, but even delight in, a considerable degree of moisture, and by their dense growth prevent its early exhaustion by evaporation, and add to it by their attraction of dew, are certainly the plants best adapted to a district like these prairies. By their long continued growth a large amount of vegetable matter has been accumulated in the soil.

I have found the prairie soil in Perry county varying in depth between 10 inches and 2 feet, and underlaid with the white sandy loam with numerous grains of ferruginous matter, from which the upper soil has been gradually formed. Frequently a bed of red or yellow tough clay intervened between the two, and in places formed a regular hardpan which offers much resistance to the passage of water.

The foregoing paragraphs contain my opinion of the principal cause of the absence of trees from the prairies in the examined district. Further investigations may point to other accessory causes which I may have overlooked, but undoubtedly the condition of humidity of the soil exercises here the most prominent influence on its vegetation. The chemical composition of the soil naturally also affects the flora; but as our prairie soils do not appear to have any very peculiar composition, chemistry may account for the absence of certain species of trees, but certainly not for the absence of all trees.

Prof. J. D. Whitney, the distinguished State geologist of California, remarks in his report on the physical geography of Iowa, in vol. i, part 1, of the Geological report of Iowa: "Taking into consideration all the circumstances under which the peculiar vegetation of the prairies occurs, we are disposed to consider the nature of the soil as the prime cause for the absence of forests and the predominence of grasses over this widely extended region; and although chemical composition may not be without influence in bringing about this result, yet we conceive that the *extreme fineness of the particles of which the prairie soil is composed* is probably the principal reason why it is better adapted to the growth of its peculiar vegetation, than to the developement of forests." I have not examined the prairies of Iowa and Minnesota, and therefore confine myself to the suggestion that there too the fineness of the soil may be an accessory cause only, and that the immediate cause may be the conditions of humidity. Several of the proofs which Prof. Whitney adduces may as well, and it seems to me with more right, be claimed as proofs of my theory. For example, the grass-covered bottoms of former ponds are apt to be still considerably moist, in the spring season at least, much more so than the adjoining forest soil; and the gravelly and timbered ridges of the prairies which he mentions are evidently also better drained in consequence of the coarser material of which they are composed. In the middle and northern part of our state too, while forest trees can be grown on elevated parts of the prairies, and after the same have been dried by continued cultivation or deep tillage and under-draining, they will sicken and die in the lower and moister situations where their roots stand, during part of the year at least, in cold water.

The prairies of our western territories are due to other causes; at least not to an excess of moisure, but rather to a deficiency of it, which, besides depriving the tree of a supply of moisture at the time when it would be required for its growth, also prevents a sufficient decomposition of the substrata, and thus the source remains closed from which the roots of the tree would have to draw their nourishment. I have witnessed a very forcible illustration of this latter defect in the sterile regions of western Utah, where whole mountain ranges are covered with sharp angular fragments of rocks, undecomposed for lack of moisture. Little soil could thus be formed on these slopes, which therefore produce but a scanty vegetation, while there is a most luxuriant growth of delicate flowers at the same altitude on every little rivulet trickling down from the snow-clad summits, and even on the moist patches of soil under and between the melting banks of snow. In other regions, again, the absence of trees and the general barrenness of the country seem to be due to an excessive accumulation of various salts in the upper crust of the soil, such as sulphate of magnesia, chlorid of sodium, carbonate of soda, and others, but which accumulate also in consequence of the insufficient drainage, due to the small amount of atmospheric precipitation.

In the district which claims our special attention, the prairie growth is undergoing a considerable spontaneons change with the progressing settlement and cultivation of the country. Since the prairie grass is no longer burnt off annually, as it used to be by the Indians and early settlers for purposes of the hunt, for killing insects and snakes, and in order to free the land from dry stalks, and thus to secure a better pasturage early in the spring, whereby all but the hardiest grasses were destroyed, and those especially remained which propagate by throwing out suckers from the roots, and since the grass is continually cropped close and tramped down by cattle, the former vegetation of the prairies has gradually given way to softer and shorter grasses, and at somewhat broken points even shrubs and trees have began to sprout up; at the same time their surface has become drier, of which more will be said below.

At some points in the prairies no water is obtained at moderate depths, but rocks are struck and have to be penetrated to a considerable depth in order to get water. At such points we might expect to find trees, but such is usually not the case. In the vegetable kingdom also the universal rule prevails that the stronger gain on the weaker ones. The tenacious grasses growing all around such spots will then encroach upon the land which is fit to bear timber, and will not suffer trees to spring up, unless they be assisted by favorable circumstances. Such encroachment of one species upon the territory else occupied by another, even to the extinction of the latter, may be frequently observed.

The "flats" are nearly level stretches of upland, as their name indicates, and are timbered principally with single large and widely scattered post oak (*Quercus obtusiloba*), of a sturdy thickset growth, with stout crooked branches, and a tattered top. Their trunks are generally in part rotten, perhaps in consequence of injuries which they received by fire during the earlier period of their growth, more likely, however, it appears to me, in consequence of the, at times, quite unfavorable condition of the ground upon which they grow, which may produce disease in the tree. The trees of the large post oak stand wide apart, and are interspersed with black jack (*Quercus nigra*) and in places young post oak. The black jack are sometimes well developed with a vigorous growth and well shaped top, but are frequently stunted and scrubby. Not being, here at least, a long-lived tree, they generally do not attain a large size. These woods are quite open, and their white soil is only scantily covered with vegetation. Even where the annual fires are kept out, undergrowth is very slow in springing up in the regular flats. Their subsoil is the finely comminuted white sandy loam mentioned above; it is nearly pure white, with an admixture of small black grains of ferruginous matter, and reaches to a depth of several feet. The upper soil is quite shallow, and seems to be distinguished from the subsoil only by a slight admixture of vegetable mould, not sufficient to color it much darker, and by the smaller number of the ferruginous grains. For the iron gradually disappears from most badly drained surface soils, and sinks deeper into the subsoil, in consequence of repeated reductions of the peroxyd into protoxyd, its solution, and final reoxydation and precipitation. A sharp line cannot be drawn between the upper soil and subsoil of the post-oak flats. They exhibit the peculiar properties which I have above described as characteristic of this kind of soil, which are not obliterated by the small admixture of humus. Its fine comminution makes this otherwise light soil badly permeable to water; and in depressions where rain water collects, the fine particles which have been held in suspension by the accumulating water soon close up the pores of the bottom, and thus shallow pools are formed, from which the water disappears slowly by evaporation. At such points we observe the pin oak (*Quercus palustris*) together with the scaly-bark hickory (*Carya alba*), and sometimes the laurel oak (*Quercus imbricaria*).

The flats extend either round the prairies, between them and the breaks or hill land, and at the same level with the edge of the prairies, or else they occupy the wider ridges without a central prairie. The most obvious difference between prairies and flats, apart from their different vegetation, consists in the different quality of their soils, which in the prairies is deeper and much more charged with vegetable matter; but as the soil is only formed in the course of time, we must search for a deeper seated first cause of the difference. The situation of the flats between the imperfectly drained prairies and the broken hill land gives us a clue. The flats are equally deficient in surface drainage with the prairies, but the permanent water-line appears to lie considerably deeper, and the lower substrata effect a natural drainage wanting to the prairies. I have scarcely any data in regard to the depth at which water is generally struck in the flats, because they have hitherto been shunned by settlers on account of their apparent unproductiveness, which disappears however before a rational system of cultivation. In most cases where wells have been dug near the boundaries of prairies and flats, water has been found considerably deeper than farther in the prairies, and many attempts at wells have been abandoned before it was reached; at the outer rim of the flats we approach, besides, the natural water courses; and we have therefore good reason to suppose that generally the substrata of the flats are drained by the underlying sandstone formation. Early in the spring the surface of the flat is exceedingly wet, on account of the deficiency of the surface drainage and the limited permeability of the soil and subsoil, which are then saturated with water to their fullest capacity. This water will mostly be consumed slowly by evaporation, and that small portion of it which penetrates deeper will be drained off. Vegetation will thus be retarded in spring, and all the bad influences will be experienced of which I have spoken in relation to the prairies. At last the soil remains closely packed and dry. The access of air to it is then still limited in consequence thereof, and when the hot season arrives the soil can absorb only a small amount of moisture from the air, in consequence of its compactness and of its deficiency in clay and humus, which two elements are known to absorb most powerfully the moisture and other gaseous elements of the air which are conducive to a vigorous development of vegetation. The same cause prevents the parched surface from drawing upon the substrata for a supply of humidity from the deep seated permanent water-level; and moreover it is generally assumed that, whenever the depth of the water-level exceeds a few feet, it can exercise a comparatively small influence upon the humidity of the soil in the dry season, during which the main supply of water for the vegetation seems to be absorbed by the leaves and by the soil directly from the atmosphere. The excessive humidity of spring is therefore followed on the flats by an excessive drought, almost without a congenial growing season between. The closely packed condition of the soil presents also an obstacle to the deep-penetrating roots, and the latter are not invited to any exertion in that direction, because the same cause prevents the access of oxygen and of other elements of nutrition. A heavy rain which would be absorbed in a short time by a coarser light soil or by well drained land, and which would only be sufficient to saturate heavy soils, and to supply them with all the water they need for some time after, will be apt to pack the surface of the flats and to flood them without penetrating deep; it will soon be dried by sun and wind, and will suddenly chill the soil, without exerting to a large degree the beneficial influences which it would have on a more favored soil.

While the former vegetation of the prairies seems to have been one suited to wet ground, the flats in their unimproved state will only sustain one which is able to outlive the excessive wet of the spring season, and the sudden change to the dryness of summer and fall. The trees which grow there are such as have shallow spreading roots, and can withstand considerable drought; deeper rooted ones would be killed in the spring, and more delicate plants would wither under the scorching sun of summer and fall if they did survive the spring. Other causes which determine the vegetation of the flats may be found principally in the chemical properties of the soil.

Such is the condition of the typical flats in their uncultivated state, where they present their worst features; there are however gradations to the better, and they are by no means as unproductive as it might appear. They were so, indeed, under the old system of pioneer cultivation, which expected nature to do everything, and man as little as possible; but if the soil is opened by deep cultivation, and vegetable mould is created, and the soil loosened by ploughing under green crops and manuring, it will be found to improve steadily as soon as the roots have once began to penetrate it. The first crop is frequently quite defective, but each succeeding one is better. In this respect the soil resembles stiff clay soil, such as potters clay, which, with all chemical elements of fertility, generally makes a quite barren soil until it is sufficiently opened by cultivation.

The "*barrens*," as the term is understood in the district to which I have reference, are hills covered with a dense growth of tall grasses, without or with only scattering large trees. The progressing cultivation has however changed their aspect considerably, and large portions of them now have a dense growth of young timber. They occupy that portion of the upland the surface of which is too uneven for prairies and flats, partly gently undulating, partly sharply rolling or even moderately broken. Their subsoil is the same white sandy loam mentioned before; but their surface-configuration affords a complete drainage, and they have therefore sustained a better vegetation and have formed some inches of a good soil considerably charged with humus. That of the sharper ridges is however apt to wash down into the the hollows, and is therefore generally shallow, while it has accumulated in the lower places. Their subsoil is frequently, but erroneously, called yellow clay; it is yellow only on exposed surfaces, on cuts in the roads, and similar places, where the iron of the soil, which elsewhere is all concentrated in small grains of dark brown color, is more diffused over the surface and colors the white material yellowish. The drainage of the substrata seems also to be perfect, and the underlying porous sandstones not unfrequently reach to within a few feet of the surface. The barrens become dry early in the spring, and resist drought better than the flats, apparently because their upper soil is better and attracts more moisture from the air, and because it is less packed than that of the flats, and the plants, in consequence of their earlier growth, have progressed farther in their development when the dry weather sets in, and are more vigorous. The subsoil is also very close. Still there seems to have been no absolute necessity for the absence of timber, and it rather appears to have been due to the encroachment of the grasses, which, being well developed in the large prairies, where no timber could grow, spread out and took possession of the barrens. Other lands very similar to the barrens, in regard to soil and situation, are timbered with post-oak forest. The annual fires which swept over the country assisted in keeping down timber, and in giving the grass entire possession; and perhaps the latter was necessary to prepare the soil for the subsequent growth of timber. In some thinly settled neighborhoods we still find the barrens covered with rank coarse grass, but generally a dense growth of small oak is springing up spontaneously, and at many places very vigorously, especially in the more inhabited districts. At some points we find in the barrens single large post oak, as we do on the flats; generally the young growth on the poorer ridges is post oak (*Q. obtusiloba*) and black oak (*Q. tinctoria*) with some blackjack (*Q. nigra*), in the hollows hazel and sumach, and on the finer rolling lands post oak, black oak, barren hickory (*Cayar tomentosa*), hazel, &c. The growth varies considerably according to local circumstances.

The "*post-oak hills*" resemble in most respects the barrens, but are covered with older forest, and are on the average more broken. The prevailing timber is post oak, with some black oak, but we find also blackjack, hickory and some other trees. White oak (*Q. alba*) is found only in the breaks of the creeks. The main difference between this forest and the barrens seems to consist in the more progressed growth of timber, due probably to the more complete drainage of the soil, aided by a more profuse admixture of sand and other materials in the subsoil from the strata which form the hills. In wet spots we find here also some pin oak (water oak, *Q. palustris*) and laurel oak (*Q. imbricaria*).

The *principal creek bottoms* within the region of the barren country and of the sandstones of the upper Coal-measures have a soil very similar to that of the flats, perhaps a little coarser; but its upper portion is considerably mixed with vegetable mould, and about as dark as the prairie soil. They are overflowed by freshets, and very naturally supplied with the necessary moisture from the creeks, even in the driest season, and their growth is thereby regulated. The timber is heavy and very tall, and consists principally of the swamp white oak (*Quercus bicolor*) and the pin oak (*Q. palustris*) with some scaly-bark hickory (*Carya alba*); but where the creeks enter the limits of the underlying limestones and shales the growth is much more varied, and consists, in addition to the above named trees, of bur oak (*Q. macrocarpa*), red oak (*Q. rubra*), laurel oak (*Q. imbricaria*), ash, black walnut, hazel, and many others.

The *white oak* is found in this district only in the steep breaks of the creeks and at a few other points, but it is altogether subordinate. At the southern limit of this region the soil changes entirely, and covering "*the ridges of the Millstone grit formation*," we find a light brownish, decidedly arenaceous, deep, light, and warm soil which supports a splendid growth of white oak, black oak, barren hickory, pig nut (*Carya glabra*), black walnut, black gum (*Nyssa multiflora*), yellow poplar (*Liriodendron tulipifera*), &c. The prairies do not extend into this district, and the Sylva here, beginning in Jackson county, and especially farther south, undergoes a considerable change, which is partly caused by a change in the geological formations and surface configuration, and consequently of the soil, partly by the southern slope and latitude of this part of the country. Of trees which do not extend farther north into the before-described Coal-measure district, I observed here the yellow poplar (*Liriodendron tulipifera*), the swamp cypress (*Taxodium distichum*), the tupelo-gum (*Nyssa uniflora*), the sweet gum (*Liquidambar styraciflua*), the winged elm (*Ulmus alata*), the Spanish oak (*Quercus falcata*), the barren oak (bitter oak, *Q. falcata* var. *triloba* (?)), the beech (*Fagus ferruginea*), the cucumber tree (*Magnolia acuminata*), and others.

Progressing change of the country.—From the foregoing statements it appears that timber is now encroaching spontaneously upon land formerly occupied by tall grasses, while, on the contrary, old forests yield to the axe and ploughshare; at the same time, the rank prairie and barren grasses die out. The effect upon the climate, especially in decreasing the humidity of the country, must be powerful, and may be compared to the change of sensation which we experience, on a clear summer evening, in coming from a sheltered damp creek bottom to the airy top of a dry hill. The effect is similar to that produced in other countries by the clearing of extensive forests. The growth of dense tall grasses, of which untold generations have died and rotted upon the same spot, not only protects the soil from the warming rays of the sun and thus checks evaporation, but it actually increases the precipitation of moisture, especially in the form of dew, by the low degree of temperature consequent upon the humidity of the surface and upon the powerful radiation of heat from the spears and leaves of the grass waving in the night air, which, as can easily be proved by experiment, grow much colder than the bare soil. The grasses also check the surface drainage most effectually. With their disappearance the above effects cease, the soil becomes more exposed to the direct rays of the sun and to the drying breezes, while the succeeding growth does not favor the precipitation of dew nearly as much as the grass. The natural impediments to the speedy abduction of the falling rains are also lessened to a considerable degree, and thus the soil is rendered drier. The artificial works of drainage and even the cuts and ruts of the roads do their share also. The breaking up of the sward and deep cultivation of the soil facilitate the sinking of the water, and expose a greater surface of soil to the desiccating influence of the sun and winds. Every old settler can bear witness to the remarkable and rapid change in the conditions of moisture of the prairies, which is also manifested by the gradual failing of the wells at numerous points. It is a common observation that they must be dug much deeper now than formerly in the same vicinity. The healthiness of the country has thereby improved, and the farmer is enabled to plant much earlier, and at points which were formerly too wet; his loss by the freezing out of the winter crops is much reduced. The droughts in summer and fall are perhaps also more severe at present, but an advantage can seldom be gained without some sacrifice, and a remedy is accessible if only we will apply it.

It is "thorough cultivation and underdraining." Where these are practiced, the roots are enabled to strike deeper, beyond the direct influence of the sun's rays; a much larger quantity of nourishment is presented to them; the humidity of the soil is equalized; its absorbing power for moisture and gases is vastly increased; and the growth of the plants is consequently much invigorated and placed beyond the reach of sudden changes of the weather. If the farmer, instead of superficially cultivating extensive tracts of land with an altogether inadequate laboring force, as I have frequently noticed, would thoroughly cultivate a smaller area, he would not have to complain so much of drought and failure of crops, and of the "giving out" of fields. Instead of exhausting his soil, he would make it richer every year; and, by making heavy and certain crops, he would find himself amply repaid for the increased labor, and reap more on an average on the smaller surface adequate to his laboring force, than before on the larger fields.

Springfield, Ill., August, 1863.

ART. XXXIV.—*On the Earth's Climate in Paleozoic Times;* by T. STERRY HUNT, M.A., F.R.S.[1]

THE late researches of Tyndall on the relation of gases and vapors to radiant heat are important in their bearing upon the temperature of the earth's surface in former geological periods. He has shown that heat, from whatever source, passes through hydrogen, oxygen and nitrogen gases, or through dry air, with nearly the same facility as through a vacuum. These gases are thus to radiant heat what rock salt is among solids. Glass and some other solid substances, which are readily permeable to light and to solar heat, offer, as is well known, great obstacles to the passage of radiant heat from non-luminous bodies; and Tyndall has recently shown that many colorless vapors and gases have a similar effect, intercepting the heat from such sources, by which they become warmed, and in their turn radiate heat. Thus while for a vacuum the absorption of heat from a body at 212° F. is represented by 0, and that for dry air is 1, the absorption by an atmosphere of carbonic acid gas equals 90, by marsh gas 403, by olefiant gas 970, and by ammonia 1195. The diffusion of olefiant gas of one inch tension in a vacuum produces an absorption of 90, and the same amount of carbonic acid gas, an absorption of 5·6. The small quantities of ozone present in electrolytic oxygen were found to raise its absorptive power from 1 to 85, and even to 136; and the watery vapor present in the air at ordinary temperatures in like manner produces an absorption of heat

[1] Communicated to this Journal by the author.

represented by 70 or 80. Air saturated with moisture at the ordinary temperature absorbs more than five hundredths of the heat radiated from a metallic vessel filled with boiling water, and Tyndall calculates that, of the heat radiated from the earth's surface warmed by the sun's rays, one-tenth is intercepted by the aqueous vapor within ten feet of the surface. Hence the powerful influence of moist air upon the climate of the globe. Like a covering of glass, it allows the sun's rays to reach the earth, but prevents to a great extent the loss by radiation of the heat thus communicated.

When however the supply of heat from the sun is interrupted during long nights, the radiation which goes on into space causes the precipitation of a great part of the watery vapor from the air, and the earth, thus deprived of this protecting shield, becomes more and more rapidly cooled. If now we could suppose the atmosphere to be mingled with some permanent gas, which should possess an absorptive power like that of the vapor of water, this cooling process would be in a great measure arrested, and an effect would be produced similar to that of a screen of glass; which keeps up the temperature beneath it, directly, by preventing the escape of radiant heat, and indirectly by hindering the condensation of the aqueous vapor in the air confined beneath.

Now we have only to bear in mind that there are the best of reasons for believing that during the earlier geological periods all of the carbon since deposited in the forms of limestone and of mineral coal existed in the atmosphere in the state of carbonic acid, and we see at once an agency which must have aided greatly to produce the elevated temperature that prevailed at the earth's surface in former geological periods. Without doubt, the great extent of sea, and the absence or rarity of high mountains, contributed much to the mild climate of the Carboniferous age, for example, when a vegetation as luxuriant as that now found in the tropics flourished within the frigid zones; but to these causes must be added the influence of the whole of the carbon which was afterwards condensed in the forms of coal and carbonate of lime, and which then existed in the condition of a transparent and permanent gas, mingled with the atmosphere, surrounding the earth, and protecting it like a dome of glass. To this effect of carbonic acid it is possible that other gases may have contributed. The ozone, which is mingled with the oxygen set free from growing plants, and the marsh gas, which is now evolved from decomposing vegetation under conditions similar to those then presented by the coal-fields, may, by their great absorptive power, have very well aided to maintain at the earth's surface that high temperature the cause of which has been one of the enigmas of geology.

Montreal, August 1st, 1863.

Note to paper on the Earth's Climate in Paleozoic Times.—Since the above was in type, my attention has been called by Prof. Dana to a paper published in 1849 by the late Major E. B. Hunt, U. S. Engineers, on Terrestrial Thermotics. In this paper, which appears in the *Proceedings of the American Association* for that year, page 153, and is referred to by Dana in his *Manual of Geology*, page 363, Major Hunt argues that the temperature at the earth's surface increases with the barometric column; and that, as the atmospheric mass must have been greater in the earlier geological periods, by the amount of carbon and carbonic acid since abstracted from it, the temperature must have been higher. To this effect of the carbonic acid, is to be added, as Mr. Dana has remarked, the greater moisture of the insular climate of the Carboniferous period, when the excess of aqueous vapor would have contributed to increase the weight of the atmospheric mass.

The slight augmentation of the barometric column dependant upon these additions to the atmosphere would however probably be inadequate to produce the considerable difference in climate which it is proposed to explain, and it is only in the light of Tyndall's recent discovery of the great absorptive action exerted by certain gases and vapors on radiant heat, that the chemical constitution of the atmosphere of that time enables us to solve the problem of the warm climate of the Paleozoic period, towards which, however, Major Hunt made as great a step as the existing state of science permitted, when in 1849 he attributed it to atmospheric conditions.

ART. XXXV.—*Correspondence of Jerome Nicklès, dated July 5th*, 1863.

Obituary.—Of scientific men, deceased in France since my last correspondence, the following are leading names:—the physicists Despretz and Bravais, the botanist Moquin-tandon, the geologist Marcel de Serres, the chemist Péan de St. Gilles. A few details may be here appropriate touching each of these savants, some of whom, as Bravais and Péan de St. Gilles, have died in the prime of life and maturity of talent, without having been able to produce all that we could have expected from their attainments and zeal.

César Mansuète Despretz died at six in the morning, March 15th, 1863. Born in Lessines, (Hainault), on the 13th of March, 1789, of poor parents, he was, to use an expression peculiar to France—the *son of his works.* Active, studious, very intelligent, and eager for instruction, he came to Paris, attended there the course of physics and chemistry, and speedily brought himself to the notice of Gay-Lussac, who took especial note of the patience and perseverance of his young scholar, and chose him as assistant professor in his course of chemistry at the Polytechnic school.

In 1824 he became professor of physics, in the college of Henry IV, where he had for some time acted as tutor, and in 1837 was appointed to the same chair in the Faculty of Science in Paris, in the place of Dulong. In 1841 he was appointed to fill the vacancy in the Institute caused by the death of the illustrious physicist Savart, and in 1858 was called to the presidency of the Academy.

His first publications date from 1818, when he began with important researches upon the latent heat of vapors. In 1819, he was occupied with the elastic force of vapors. At that time, physicists agreed with Watt that *the total amount of heat contained in a given weight of saturated vapor of water is constant at all pressures and at all temperatures.* Despretz, on the contrary, concluded from his own experiments, that *the latent heat increases with the pressure and the temperature, but to a less amount than the increase of the temperature.* His results are now universally adopted.

The researches which he published in 1822 on *the causes of animal heat*, were "crowned" by the Academy, which had proposed this subject for investigation:—"Compare the heat developed by a warm-blooded animal, in a given time, with the heat developed by the CO^2 and HO formed in the respiration of the same animal during the same time." On this occasion Despretz gave his attention to a series of researches upon the composition of the air and the respiration of reptiles; perceiving that, besides the carbonic acid formed in the respiration of these animals, there was also some nitrogen.

He also discovered that fishes have a higher temperature than that of the water in which they live;—that new-born infants have a temperature about two degrees higher than that of man, and that the same is true with animals, etc.

His researches upon the conductibilities of bodies were also productive of results which have been accepted by science, and which are almost entirely confirmed by corresponding results obtained by Mr. Forbes of Edinburgh. In this line of research, Despretz was preceded only by Ingenhousz.

In his researches upon the compressibility of liquids, he was the first to perceive that this compressibility was in a decreasing ratio (1823).

In 1825, he was occupied with the study of the heat developed in the combustion of carbon, hydrogen, phosphorus, and many of the metals, in the investigation of which, he, for the first time, burned metals in a calorimeter. Out of many interesting facts resulting from these researches, we note this one:—tin and the protoxyd of tin emitted the same quantity of heat for the same volume of oxygen absorbed.

While studying the density of gases at different pressures, in 1827, he discovered that all gases follow Mariotte's law, while they are strongly compressed, and that they possess variable compressibilities.

In 1828, he studied combustion under different pressures; in 1829, the modifications which metals suffer, under the combined action of heat and ammoniacal gas, discovering the fact that iron will combine directly with nitrogen, forming a nitrid. But only recently, and as one result of the great labors of St. Claire Deville and Wöhler, has it become certainly known that nitrogen has a very strong affinity for many of the metals, and is fitted to combine with them.

But space would fail us to speak particularly of all the works published by Mr. Despretz, even in the last years of his life. We have before[1] spoken of some of these, especially of his researches upon the nature of simple bodies (1859), which occasioned a discussion on the part of Mr. Dumas, so interesting and at the same time so profitable to science. We have also spoken of that interesting memoir in which, under the modest title, "Observations upon charcoal," he shows the possibility of making artificial diamonds.

The works remaining for especial mention are those "upon the variations of the zero in the thermometer in the same course of experiments;" "upon the luminous power of the pile, with reference to the number and disposition of the elements;" "upon the limit of the chemical power of the pile;" "upon the chemical work of the pile;" "upon the law of currents;" "upon the tangents-compass;" "upon the constancy of the pile;" etc.

But let us look at the principal results of these researches,—results which have taken their place in science, and which it is well to call to mind, now that the grave has forever closed over the humble and venerable savant who was their author.

Mr. Despretz has furnished experimental demonstration,—1st of the decrease of the compressibilites of liquids with the pressure; 2d, of the unequal compressibilities of gases; 3d, of the increase of the compressibilities of gases with the pressure; 4th, of the law of the propagation of heat in liquids; 5th, of the generality of the law of the propagation of heat for all bodies, whether good or bad conductors; 6th, of maximum density as a property appertaining to all aqueous solutions, and of the existence of this maximum at a temperature lower than that of congelation, for a great number of solutions; 7th, of the constancy of the quantity of heat disengaged, whatever the pressure may be, in the phenomena of combustion, where the volume of oxygen is not changed; 8th, of the fusibility and volatility of all bodies, even of carbon; 9th, of the non-influence either of the tension or quantity upon the rays of the voltaic spectrum; 10th, of the influence of the position of the poles, in the vertical voltaic arch, upon the length of this arch; 11th, lastly, of the artificial production of microscopic diamonds by the electric current, whether wet or dry.

Such are some of the discoveries which belong solely to this zealous experimenter.

Despretz did not believe in grand theories, and never founded a school. His genius consisted in patience for every investigation. By persevering labor and a strong will, he attained to that superiority which others owe to natural talent.

His manner of life was very austere; he always wore clothes of sombre hue, which caused him to be often mistaken for a priest, and which he did not change even when preparing for the chase, of which he was passionately fond. He was not married, and took his meals every day at the same hour, at a restaurant which owes to him its popularity among chemists and physicists of all parts of the world; for it was a great pleasure to him to invite to his modest repast such men of science as might arrive from abroad. From this circumstance I take the liberty

[1] This Journal, [2], xxviii, 121.

of giving some details of these repasts, now become classic, and which Mr. Despretz did not renounce until, having lost his entire fortune in 1847 in a railroad enterprise, he believed he ought to impose upon himself very severe privations. The repast in question was composed almost invariably of *soup á la Julienne ; oysters ; Normandy sole ;* in summer, *beefsteak*, in autumn and winter, a *roast* from the results of his hunting; lastly, *fruits* and *coffee*. When he had no guests, he took for his morning meal a cup of milk, while receiving calls.

He left numerous manuscript notes, and a library well supplied with French, English and German works.

Marcel de Serres died at Montpellier, July 22d, 1862, at the age of 82, in the midst of scientific labors, and almost, as it were, without laying down his work. * * *

[A notice of the life and labors of de Serres will be found in vol. xxxiv, p. 303 of this Journal.—Eds.]

Horace Benedict Alfred Moquin-tandon was born at Montpellier, May 7th, 1804, and died on the 15th of last April. Called to the Faculty of Sciences of Toulouse in 1833, as professor of botany, in 1850, together with Mr. Montagne, he was appointed by the government to make a thorough examination of the flora of the island of Corsica. After the death of the botanist Richard, in 1853, Moquin-tandon was appointed to fill his place as professor of botany to the Faculty of Medicine of Paris, and director of the botanic garden belonging to that institution. The following year he was elected an acting member of the Academy, having previously been a correspondent. He was vice-president of the Society of Acclimation.

He published various researches upon both zoology and botany; among them, the following:—"Upon the manner in which the officinal leeches first enter the skin, and on the wound which they inflict;" "Monograph of the family of the Hirudines," 1 vol. 8vo, with another of 44 plates—"Researches upon the Ancylus," "Essay upon the duplication of organs of vegetables"—"Elements of vegetable teratology," (1841), &c.

As a professor, Moquin-tandon was endowed with a happy ease of manner, and united to the clearness of verbal demonstration a great facility of representing upon the board, in a simple and rapid manner, the form and structure of the object which he was describing.

Auguste Bravais was born in 1811, at Anonnay (Ardèche). While serving in the capacity of midshipman, he made a voyage to the Polar regions, in 1836, and, upon his return, published a report upon numerous meteorological and geological observations. Having attained the rank of lieutenant, he left the service and accepted a professorship in the Faculty of Sciences at Lyons. In 1846 he was appointed to the chair of Physics at the Polytechnic school, and in 1854 was elected to the Academy, to fill the place made vacant by the death of Admiral Roussin.

Among his published works, we may mention the following:—"Essay upon the general disposition of rectiseriate leaves"—"Memoir upon the old coast-lines of the sea of Finmark"—"Upon the crepuscular phenomena"—"Upon symmetrical polyhedrons"—"Studies upon crystallography"—"Upon parhelia"—"Upon the white rainbow"—"Upon halos"—"Influence which the rotation of the earth exercises upon the movement of the conical pendulum"—"Researches upon feeble double refractions."

Having suffered many years from a disease of the brain, he was at length compelled to give up all scientific labors: in his last years he was reduced to childishness. He died in March last.

Léon Péan de St. Gilles was born at Paris, January 4th, 1832, and died March 22d, 1863, at the age of 31.

Though always in delicate health, he chose a career little compatible with his constitution, viz: that of a chemist. Possessing a large fortune, he fitted up a fine laboratory, where he delighted in passing his whole time, by this close confinement, doubtless, hastening his death.

The following are a few of his works deserving mention:—"Upon many new sulphites of the bases Hg^2O and Cu^2O"—"Upon the hydrate and acetate of iron." These two memoirs have been the subjects of very favorable reports from Messrs. Balard and Thénard.—"Various experiments relating to the comparative action of nitric acid and mercury upon sulphur in its insoluble and its crystalline states"—"Upon the oxydizing properties of permanganate of potash;" lastly, "Researches upon affinities," undertaken in concert with Mr. Berthelot, which his death suddenly broke off.

Discovery of fossil man.—We have before mentioned[2] (1860) facts relating to this subject, in reviewing the long continued efforts of Mr. Boucher de Perthes to establish the fact that man was cotemporaneous with the larger animals whose remains we find fossilized. Perthes founded his opinion upon the presence, in the Post-tertiary deposits, of wrought articles, and, in general, of the products of human industry, associated with bones of *Ursus spelæus*, *Elephas primigenius*, &c. These articles consist mostly of flint wrought to the form of hatchets or daggers, together with bones bearing evident signs of having been worked. One thing remained to be accomplished—the discovery of man himself in a fossil state; for one could reasonably say, that, if man were cotemporaneous with the *Ursus spelæus*, we ought to find his fossil remains in connection with those of the larger animals.

This discovery was made at the commencement of the present year, by Mr. de Perthes, upon the same formation with his previous discoveries in an open gravel pit, called Moulin Quignon.

We will not here repeat all the particulars of the authentication of this discovery which was immediately carried out by Messrs. de Quatrefages and Prestwich. English paleontologists having expressed doubts (*London Times*, April 25th, 1863), a sort of international congress was decided on to settle the question. Under the presidency of Milne-Edwards, uniting great learning and a conciliatory spirit, and after many sessions at the Museum, they adjourned to Abbeville, where, after taking great care to prevent any deception, they made new excavations, and, at the depth of four metres, in a bed apparently identical with that from which the jaw had been extracted, found many hatchets of flint every way similar to those previously examined whose authenticity had been doubted by the English savants. The details of this remarkable congress can be found in the interesting report which Milne-Edwards presented to the Academy, at the sitting of May 18th.

[2] This Journal, [2] xxix, 269.

Finally, the question takes a new phase, for Elie de Beaumont does not admit that the deposit at Moulin Quignon belongs to the Post-tertiary, but classes it with "deposits laid down upon hill-sides," and considers it, consequently as more recent than the diluvium.

Milne-Edwards, without wishing to discuss with E. de Beaumont, the stratigraphical question, which is not in his province, persists in considering it very probable that the jaw from Moulin Quignon is cotemporaneous with the fossil bones obtained from the same quarry. Both geologists and paleontologists, in general, share the opinion of the illustrious geologist, but the point is still in discussion. [For a notice of this discovery, see this vol. p. 123.—Eds.]

The manufacture of alcohol by means of illuminating gas.—The industrial world has been, for some time, much interested in a process for the production of alcohol by means of illuminating gas, at a very low cost (25 francs per hectolitre), and one litre of alcohol so prepared is mentioned among the principal curiosities at the London Exhibition. Moreover, it has been said by some journals, both in France and elsewhere, that the manufacture is going on at St. Quentin, and that the apparatus which receives coal upon one side pours out alcohol on the other.

These are exaggerations of certain results obtained by a company which has been organized at St. Quentin for undertaking the application of a patent obtained by Mr. Cotelle, a manufacturing chemist. The patent is founded upon the experiment by means of which Berthelot, in 1855[3] accomplished the synthesis of alcohol, by causing the absorption of olefiant gas, C^4H^4, by sulphuric acid, thus converting it into sulpho-vinic acid, a compound readily turned into alcohol by processes long since known.

This experiment, made known by Hennell, thirty years ago, has now been repeated with C^4H^4 prepared from alcohol. Mr. Cotelle employs mostly illuminating gas, which, as we know, contains from 4 to 12 per cent of C^4H^4. Separating this by means of sulphuric acid, there remains a gaseous mixture, composed of C^2H^4, CO, H, &c., very suitable for burning, so that this first material ought to cost very little, especially if the manufacture be undertaken at the mines, so as to take advantage of the gas which issues from the coke furnaces.

To produce one hectolitre of alcohol of 90 per cent, Mr. Cotelle uses not more than 40 cubic metres of C^4H^4, which corresponds to about two tons of the northern coal used at St. Quentin..

But the difficulty is not solely in the production of C^4H^4; there is also needed a large amount of concentrated sulphuric acid, (10 parts of $HO\,SO^3$ to 1 of alcohol). This, used at 66° of Beaumé's areometer, remains, after the completion of the work, at from 20° to 25°. It is necessary, then, either to concentrate it again for a new process, or to utilize it in its diluted state; from this we see the necessity for either concentrating apparatus or leaden chambers; for a hectolitre of alcohol requires for its production 1500 kilometres of sulphuric acid at 66°.

Thus we perceive a series of difficulties which are not yet overcome, but which are vanishing, day by day. Still, Cotelle's process is interest-

[3] This Journal, [2], xx, 111, 264.

ing and we will give it in a few words. Starting with the purification of gas, we free it from sulphydric acid and ammonia, then desiccate it by passing it over $HO\,SO^3$. Drawn along by suction like that of a pump, the dry gas is directed to a column of glass or sandstone furnished with trays or diaphragms pierced with small holes, from which descends $HO\,SO^3$ in a finely divided state to meet and dissolve the C^4H^4. This solution takes place slowly, so that the apparatus needs as many as forty trays to distribute enough sulphuric acid to absorb the gas and be saturated with it.

The sulpho-vinic acid thus obtained is next treated with five times its volume of water, and the mixture submitted to the action of a stream of vapor which carries over the alcoholic product. The vapors are condensed; the alcoholic liquid thus obtained is re-distilled over a little lime, to separate any sulphuric acid which may have distilled over, and the liquid condensed from this distillation is rectified to produce alcohol of 90°.

The residue of this operation is, as we have seen, sulphuric acid of 20° to 25°, and a gaseous mixture representing the gas from ordinary coal less HS, NH^3, and C^4H^4 : this latter can be advantageously used for fuel.

New method for the concentration of mineral waters.—Sea-water in freezing, forms flakes of ice consisting of nearly pure water, and an extremely saline liquid which in Northern countries is utilized in the production of marine salt. Very recently, Dr. Robinet, a physician of Paris, has discovered that the same process can be applied in the purification of fresh water. In freezing water from the Seine, from wells, and from springs, he found the ice produced to be so entirely free from the salts of lime and magnesia which were contained in the water, that, thus purified, it may be considered as nearly equal to distilled water. So it is now proposed to procure water on board ships, no longer by distillation but by congelation, by means of the apparatus of Mr. Carré, of which mention is made below.

The same fact is made use of in the concentration of mineral waters, a problem which has offered itself for a long time, but which the employment of heat could not solve, on account of the gas originally in solution, which the heat expelled. Cold works better. Dr. Ossian Henry, of Paris, has experimented with forty different varieties of water, and finds that it is possible by congelation, to reduce mineral waters to one-eighth, one-tenth, one-fifteenth, or even one-twentieth of their original volume, without producing any alteration in the gases contained in them.

100 litres of mineral water can thus be reduced to 5, giving great economy in transportation ; moreover, the ice itself is also valuable. But we do not believe that the therapeutic properties of the extract will be identical with those of the water in its original state, because of the changes which manifestly take place in the contained salts, changes so evident that Mr. Balard has been able to base upon them a manufacture of sulphate of soda, by exposing to a temperature sufficiently low the waters containing NaCl and MgO, SO^3, which result from the manufacture of sea-salt by the evaporation of sea water.

The publication of this process has given occasion for a protest on the part of Mr. Tichon, an apothecary of Aix les Bains (Savoy), according to which the same process has been used since 1856, by him and a Mr. Melsens, who was staying at Aix for his health. The mineral water which he drank here, and which is sulphurous, proving disagreeable to his taste, he undertook to remove part of the odor by submitting it to a freezing mixture. In this way he was able, not only to mask the disagreeable odor but also to concentrate the mineral ingredients.

Mr. Tichon adds that congelation will not suit all mineral waters, inasmuch as it alters the organic matter therein dissolved.

Manufacture of ice.—If freezing mixtures were the only means of obtaining ice, or a degree of cold corresponding thereto, the various applications of which we speak would still be far from having received a practical solution, but we already know of important services daily rendered to science by the apparatus of Carré which excited so much interest at the last London Exhibition. As we cannot here describe this interesting machine, for lack of room, we are obliged to refer for particulars to "L'Année Scientifique et Industrielle," by Figuier, 1863, page 457, and Supplement, but the principle is as follows:—It is based upon the great quantity of heat which ammonia, liquefied by condensation, absorbs in becoming again gaseous, as this body contains an immense amount of latent heat.

Ammonia in the gaseous state is readily obtained as is well known, by boiling the ammoniacal liquid known in commerce as "volatile alkali," to reproduce which it is only necessary to expose ammonia in the presence of water; to liquify the gas, simple pressure is adequate. As it so readily takes the gaseous form, it is sufficient to simply remove the pressure which retains it as a liquid, and as this change of state is possible only on the condition that the liquefied ammonia retakes the heat lost in its liquefaction, we perceive that it will rapidly cool the vessel which contains it and, consequently, the neighboring material.

To undertake the method of putting these principles in operation we have only to suppose an apparatus composed of two retorts soldered together by the necks, the whole perfectly close and without communication with the outer air. In the larger of these retorts we place a concentrated solution of ammonia in water and heat it. Driven off by the heat, the gaseous ammonia cannot escape without becoming liquefied in the small retort. But, when the apparatus is restored to the ordinary temperature, the liquefied ammonia reassumes its gaseous form and becomes redissolved in the water of the first retort.

Two forms of apparatus are used, 1st, intermittent, 2d, constant: they are beginning to be introduced into various branches of industry. Brewers use them to freeze the wort of beer destined to undergo fermentation; coffee-house keepers for making ices and sherbets; vine growers to concentrate wine, and in the preceding article we have noticed other uses to which this apparatus has been or can be applied.

Mr. Carré started in this line of business in the first place, by perfecting an apparatus of American invention, [that of Prof. A. C. Twining,] in which the volatilization of sulphuric ether served to produce a considerable degree of cold, and to obtain, in a short time, blocks of ice.

Still, this apparatus presented a great fault, viz: the difficulty of maintaining the vacuum: not being able to remedy this, conveniently, he chose to change the system; thus he was brought to operate his machine with ammonia.

Building Materials—Preservation by means of the residuum of coal-tar.—In France, the residuum obtained by the distillation of tar, for the purpose of extracting the oils and hydrocarbons, is called *brai* (*coal-tar pitch* in English). Upon immersing bricks in this resin, melted at 200°, they become fit to use with success in the construction of chlorine chambers, also of condensers for chlorhydric acid. Mr. Kuhlmann, of Lille, has applied it hot to the exterior walls of his kilns for the decomposition of sea-salt, he also impregnates with it the tiles with which his workshops are covered, especially on those in which acid exhalations are produced. He also employs it for giving consistency and a black color to vessels and tiles of porous earthen ware. Plaster acquires a strong consistency, and does not crack as it does after being dipped in silicate of potash or soluble glass: by virtue of its porosity, it is thoroughly penetrated by the resin, and becomes permeable to all other substances; while objects moulded in plaster retain their form without the least alteration. This is so true that crystals of gypsum (natural hydrated sulphate of lime) become, in the resin, of a shining black color, the crystalline form not being changed, but the water of hydration, being replaced by the resin: it is a pseudomorph. Alabaster acts in the same way.

Stones covered with coal tar, or even with a greasy or resinous coating, resist the action of wind bringing salt spray from the sea better than do bare stones.

Mr. Kuhlman saw a striking example of this in the Bay of Biscay upon a building erected in 1858. Upon the more exposed points, the wrought stones were deeply corroded; but what is most remarkable of those stones which before being put in place, were numbered in black with oil, the parts covered by the color have been so protected from alteration that the numbers are now presented in considerable relief and with great sharpness.

This resin is not the only substance which readily penetrates plaster, stone or masonry. Mr. Kuhlmann has discovered that resins, greasy matter, and various other substances act in the same way, and that it is also the case with all liquids and bodies in fusion, when they *wet* the body which is to be penetrated. In the case of plaster it is not a simple effect of permeability, but rather of displacement of the water; the plaster, becoming anhydrous, is thoroughly penetrated by the tarry matter, and its consistence increases, but the form of the object moulded is preserved unaltered, even when the bath of resin is raised to the temperature of 400° C. At from 150° to 200° C., stearic acid acts like resin; the plaster becomes impregnated with it, and at the same time loses its water of hydration, which is recognized from the boiling which it occasions in the bath.

With liquids which do not wet the plaster, this penetration does not take place. Mr. Kuhlman has tried in vain to effect it with melted sulphur or with mercury.

But it is not only water which can be displaced by coal tar; other substances are similarly affected: e. g., crystallized peroxyd of manganese loses, in the boiling resin, one atom of its oxygen, and takes up coal tar; its crystalline form remains unchanged; after the operation, the crystals thus treated no longer give any trace of chlorine with chlorhydric acid.

In all these experiments, the temperature of the solid body should be raised very gradually.

Bibliography.

Recent Publications by HACHETTE & Co., Paris:

Leçons faites à la Société Chimique de Paris, par M. Verdet et M. Berthelot. 8vo, 1863. These lectures bear partly upon the great question of the "mechanical equivalent" of heat, which Mr. Verdet has set forth at two sittings and in which he has summed up the state of the question of which the fundamental formula was given forty years ago by Mr. S. Carnot.

Mr. Berthelot, in his lectures, explains all the facts which relate to the polyatomic alcohols, in the discovery of which he has taken so large a part; he then treats thoroughly of the sugars, and the other alcohols whether derived or polyatomic, of which he has equally contributed to increase the list and elucidate the history.

L'Année Scientifique et Industrielle, par L. Figuier, 7th year.—This popular work contains the principal scientific facts made known during 1862. Among other important articles we notice the following:—upon the cutting of the Isthmus of Suez; upon the Mount Cenis tunnel; upon the artesian wells in the Sahara; the metallurgy of platina; textile substances substituted for cotton; machine for making ice; &c., &c.

L'Année Géographique, par Vivien de St. Martin.—1st year, 1863.—The author has begun for geography that which Figuier has accomplished for the sciences. The voyages of discovery, the ethnographical studies, the topographical works, the antiquarian researches of the year are all recapitulated; the excavations at Nineveh and Babylon, the search after the source of the Nile, the journies across central Africa, the triangulation of India, with many other subjects, are treated in a very clear and very complete manner.

Histoire des temps modernes, depruis 1453 *jusqu' à* 1789, *par V. Duruy.* 1 vol. 12mo, 1863. This volume forms part of a collection which is often inquired after, and which appears under the direction of Mr. Duruy, at present the Minister of Public Instruction in France. The present volume is entirely by Mr. Duruy himself, and will give some idea of the liberal as well as philosophical spirit of its author.

This book is a continuation of a series of works by the same author, who is especially celebrated for his philosophical spirit and the independence of his ideas and opinions. Among his collaborators we find such scholars as Chéruel, Guillemin, Preysz and Zeller, from whom we have previously had many publications.

Les marines de France et d'Angleterre—1815–1863, *par X. Raymond.*—The principal chapters of this interesting work are: Steamers—Floating batteries and iron clads—Iron armor and cannon—The conditions of naval power, &c.

Abd-el-Kader; sa vie politique et militaire, par Alex. Bellemare, 12mo. The author, who is a sincere admirer of the Emir, has given careful study to the events in which this great man has taken part. Being attached to his person in the capacity of interpreter, Mr. Bellemare has been able to make a valuable book and one which historians my consult with profit.

Association polytechnique.—Entretiens populaires; 3d series, 1863. 1 vol. 8vo. This new series, following those which we have previously announced, contains lectures upon the Plurality of Worlds, by Babinet; upon the Suez ship-canal, by de-Lesseps; upon Work and its Influence upon Health, by Dr. Bouchardat; then, the lectures upon the London Exhibition; these last were given especially in view of the "Universal Exhibition" which is to take place in Paris in 1867, and which will be much more *universal* than any of its predecessors, in that all branches of human activity will be admitted thereto.

Souvenirs d'un prisonnier de guerre au Mexique, 1854–1855, *par Vigneaux.*—The author was one of the adventurers belonging to the party of Rousset-Boulbon, and served as its secretary. The book is therefore founded on accurate knowledge of this adventurous campaign, at the end of which Mr. Vigneaux found a prison.

L'Intelligence des bêtes, par Victor Rendu, 12mo.—The author, being a great lover of bees, and having viewed his subject with affection, brings up especially such facts as testify in favor of the intelligence of these useful insects and other Hymenopters. He naturally concludes in favor of the intelligence of animals in general, and gives, in support of his opinions, some very interesting examples.

L'Année Littéraire et Dramatique, par Vappareau, 12mo.—A work of the character of *L'Année Scientifique* noticed above; it treats of all the new literary and bibliographical works which appeared in France in the year 1862.

Fraudes et Maladies du vin, par Jacques Brun. 8vo, 125 pp.—This work, conscientiously written, indicates the ways in which we may discover adulterations and diseases of wines, as well as the process for their analysis, among which are many new ones, some of them invented by the author.

Le Verrier du XIX^{e} *siècle, par Flamen.* 8vo, 500 pp.—This work is a complete treatise upon the manufacture of crown and flint glass, and enamel. The author, who has for a long time directed a large glass manufactory, includes in this work his practical observations and also the secrets of manufacture which have secured the success of his glass as well as of his establishment. Mr. Flamen is not learned—we see it in his book, but he is a practical man, and especially an observer. The processes not before published take up a large part of the book; of these he has invented some and perfected many more.

Nancy, July 5, 1863.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS.

1. *On the density of vapors at very high temperatures.*—Deville and Troost have communicated the results of their very interesting and valuable investigation of the subject of vapor-densities. If we consider the combining volume of hydrogen as **2**, the volume occupied by an equivalent of chlorid of ammonium is, as is well known, **8**, while the volumes occupied by single equivalents of most compounds appear to be represented by **4**. Many chemists assume that at high temperatures chlorid of ammonium is decomposed into equal volumes of ammonia and chlorhydric acid so that the **8** vols. of the former are really a mixture of **4** vols. of each of its constituents, and the volume **8** is therefore *apparent* only. At lower temperatures, in cooling for instance, the constituents recombine. A similar explanation has been given for other cases of **8**-volume vapors, and Pebal, Wanklyn and Robinson have endeavored to show by the direct application of Graham's method of diffusion that a separation actually does take place at high temperatures inasmuch as small quantities of the **4**-volume constituents were obtained in a free state. Deville has now proved, first, that at the temperature of 350° C. ammonia and chlorhydric acid combine with evolution of heat, so that it is impossible that dissociation can take place at this temperature. Secondly, that chlorid of ammonium does not decompose at a temperature at which ammonia is already in large part decomposed. Thirdly, that a mixture of chlorhydric acid nitrogen and hydrogen, passed through a tube heated to low redness, does not combine to form chlorid of ammonium, even in presence of platinum sponge. Similar experiments made with iodid and bromid of ammonium lead to the same results. Cyanid of ammonium, which is formed at very high temperatures by the contact of ammonia and carbon, occupies **8** volumes at 100°. Even if this salt did decompose it would not give a mixture of cyanhy-

dric acid and ammonia, because both of these are decomposed much sooner than the cyanid. Chlorhydrate of ethylamine is decomposed in very small quantities into ammonia and chlorid of ethyl at high temperatures, but the gases do not recombine under the circumstances. Deville explains the results of Pebal, Wanklyn and Robinson very simply by suggesting that the process of diffusion produces a decomposition of compound gases exactly as of liquids and salts in solution. Deville finds further that the number of substances corresponding to 8 vols. of vapor is much larger than has been supposed. His recent results confirm the view already received that sulphur, selenium, tellurium, phosphorus and arsenic correspond to 1 volume (H=2). The vapor density of chlorid of niobium, $NbCl_2$ (?), corresponds to 2 vols. as well as that of chlorid of tantalum, $TnCl_2$ (?). Neutral sulphid of ammonium and monohydrate of sulphuric acid give 4 vols. of vapor. The authors in conclusion call attention to the danger of basing theoretic speculations on the atomic constitution of bodies upon vapor-densities alone.—*Comptes Rendus.*

W. G.

[*Note.*—It is easy to see that if the existence of 8-volume vapors be admitted, the *molecular* weights of the elements must be made to correspond to 8 volumes instead of to 4, provided that we assume, with the followers of Gerhardt and Laurent, that the molecules of all substances in the form of vapor, occupy the same volume. The molecular weight of oxygen thus becomes 64 instead 32, and the molecule must consist of 4 atoms, each having a weight represented by 16. While the progress of science may lead to the adoption of the new atomic weights, it is not to be denied that the advantage of simplicity still remains with the older view, which admits the possibility of a difference in the atomic volumes of different gases. W. G.]

2. *On the work of elastic forces.*—In a very interesting and suggestive note on the course to be pursued to discover the only truly universal principle in physical nature, Lamé asserts that he has recently demonstrated, with an unhoped for degree of rigor and simplicity the following propositions. The velocity of propagation of a plane wave in any solid diaphanous medium must diminish with the length of the wave, this diminution being zero when there is no free ether; insensible in all cases of sonorous waves; very sensible on the contrary in the case of a luminous wave, if the ether exists and if the distances which separate the ponderable particles are comparable to the breadth of the wave. In other words, in the formulas employed the coefficients of elasticity, instead of being constant, must contain the length of the wave and diminish with it; this modification is necessary and sufficient. From this second theoretical extension we obtain, first, the only rigorous proof of the existence of free ether in diaphanous bodies, hitherto admitted *à priori* and not demonstrated; secondly, the only completely rational explanation of the phenomenon of dispersion; and thirdly, a whole series of new consequences on dispersive powers, on the coloration of transparent media, on the real distances which separate the ponderable particles and on other points.—*Les Mondes*, 4 *Juin*, 1863. W. G.

3. *On the atomic constitution of liquids.*—Wiener has given an explanation of the motion of small particles of inanimate matter in liquids,

long since observed by the English botanist, Robert Brown. The explanation depends upon the author's views of the constitution of matter and of the relations between material and etherial particles. These views are in themselves worthy of attention as presenting some new points and perhaps overcoming difficulties long since recognized. The author's propositions are briefly as follows:

(1.) Matter consists of material atoms which mutually attract, and of etherial atoms which mutually repel each other. Material and ethereal atoms repel each other.

(2.) The heat of a body consists in a condition of vibration of its ethereal atoms and material molecules.

(3.) With an increase in extent of vibration is connected;

a. an increase of living force in the position of equilibrium.

b. a diminution of the time of vibration.

c. an expansion of the body. For if after the position of equilibrium has existed, vibrations are excited around this, the mean distance of two vibrating atoms is still the same as in the position of equilibrium, but the mean of the varying forces is greater than the force in the position of equilibrium, because the forces diminish in some inverse ratio of the distances. The mean force therefore increases with the extent of vibration, and expansion must ensue.

(4.) The temperatures of two bodies are equal when the times of vibration of the atoms in them are equal.

(5.) The quantity of heat conveyed to a body is the increase of the living force of the vibrating atoms and the work done by the change in the position of the atoms. The living force is here defined as $\frac{1}{2}mv^2$ and not as mv^2.

In the process of fusion heat is absorbed; the work done is expended in changing the physical condition and not in raising the temperature. This work must be commenced either in overcoming internal forces with a change in the position of the atoms, or in producing an increase of living force, and must be accumulated in the body in one of these forms. The first case cannot occur because an increase in the distance of the molecules often does not occur, as for instance in the case of ice, which contracts on melting. The work commenced must therefore be expended in increasing the living force of the vibrating atoms, but here the difficulty arises that there is no change of temperature during fusion.

To overcome this difficulty, Wiener suggests that the difference between the two physical states may consist in two different directions of the vibrations of the ponderable molecules as compared with the directions of the vibrations of the ethereal particles. Thus in the one case the two sets of particles may vibrate in the same, and in the other case in opposite directions. In solid bodies the direction of the vibration of the molecules is opposite to that of the ethereal particles; in fluids it is in the same direction; the latent heat of liquefaction is expended in increasing the living force which is necessary to keep the time of vibration or temperature unchanged during the inversion of the direction of vibration. When the temperature becomes sufficiently high the distance between two molecules may become so great that the range of stable equilibrium is exceeded, so that the active force has its direction inverted. The molecules then vibrate in the direction of the atoms, and the condition of fluidity is reached while the temperature becomes that of fusion. The new state of vibration does not admit of stable equilibrium, and when two molecules separate beyond the position of unstable equilibrium the intervening ether must dilate, its repulsion for the surrounding ether must diminish, and this will rush in to supply what may be called the partial vacuum. The outer atoms and molecules will follow, and since the cause of this motion, namely the great amplitude of vibration, constantly acts, there will be a constant motion of the particles with respect to each other. In other parts of the liquid the molecules are for an instant within the limits of stable equilibrium, and there is therefore a certain amount of cohesion. The author proceeds to apply this theory to the motions of solid particles observed by Brown: he shows satisfactorily that these are not due to infusoria, to mechanical agitation, to the attraction and repulsion of the vibrating solid particles for each other, to differences of temperature or to evaporation, and ascribes them therefore to internal motions peculiar to the condition of fluidity. In the case of particles whose diameter was between 0·0006 millimeter and 0·0014 millimeter, the mean velocity was 0·0016 millimeter per second. With larger particles the motion was less; thus, particles 0·0023 millimeter in diameter had a velocity of 0·0005 millimeter per second. With particles from 0·0014 to 0·0023 millimeter the motion varied greatly, but was usually considerably greater than 0·0005 millimeter. With a greater diameter than 0·0023 millimeter the motion was still less. The author infers from these measures that the diameter of the similarly moved masses of water is so small as to correspond nearly with the wave-length of red light and still more nearly with that of radiant heat, and considers this as a strong confirmation of his theory.—*Pogg. Ann.*, cxviii, 79. W. G.

4. *On the absorption of gases by charcoal;* by Dr. R. ANGUS SMITH, F.R.S.—The following is a summary of the author's observations:—

(1.) Charcoal absorbs oxygen so as to separate it from common air, or from its mixtures with hydrogen and nitrogen, at common temperatures.

(2.) Charcoal continues the absorption of oxygen for at least a month, although the chief amount is absorbed in a few hours, sometimes in a few seconds, according to the quality of the charcoal.

(3.) It does not absorb hydrogen, nitrogen, or carbonic acid for the same period.

(4.) Although the amount absorbed is somewhat in the relation of the condensibility of the gases by pressure, this is not the only quality regulating the absorption, of oxygen at least.

(5.) When it is sought to remove the oxygen from charcoal by warmth, carbonic acid is formed, even at the temperature of boiling water, and slowly even at low temperatures.

(6.) Charcoals differ extremely in absorbing power, and in the capacity of uniting with oxygen, animal charcoal possessing the latter property in a greater degree than wood-charcoal.

(7.) Nitrogen and hydrogen, when absorbed by charcoal, diffuse into the atmosphere of another gas with such force as to depress the mercury three-quarters of an inch.

(8.) Water expels mercury from the pores of charcoal by an instantaneous action.

(9.) The action of porous bodies is not discriminate but elective.

Theoretical Considerations.

(1.) The elective nature of porous bodies may be closely allied to three properties:—

a. The condensibility of the gases.

b. The attraction and perhaps inclination to combine.

c. The capacity of combination.

(2.) In either case, the attraction which results in condensation of the gas is exercised at distances greater than the distances of atoms or molecules in combination.

(3.) The gases in porous bodies lie in strata, the outside and more distant being less attracted than the atoms nearer the solid body.

(4.) We cannot separate chemical from physical attraction; but attraction may exist without its ultimate result (combination), which is distinctly chemical.

(5.) It is exceedingly probable that as physical attraction moves onwards to chemical combination it produces the phenomena which have been attributed to so-called masses.

Chemical affinity is supposed to involve an attraction which is purely chemical; we have no proof of any such attraction as a separate power, we have only a proof of the combination. Attraction may exist without the capacity of combining chemically, or, in other words, without chemical affinity. Chemical affinity (a very inappropriate term) is only known by combination; the previous attraction has never yet been shown to be of two kinds; and it seems more in accordance with Nature to diminish than to increase the number of original powers.—*Proceed. Royal Soc'y,* Feb. 5, 1863, p. 424.

[JOHN HUNTER, of Belfast has published in the *Philosophical Magazine,* (1863) p. 364, the preliminary results of a series of researches on this subject, in which he follows the method of Th. de Saussure, by introducing heated charcoal into the dried gas over mercury and noting the absorption. This part of his paper is devoted to the absorbing power of different kinds of charcoal on ammonia, carbonic acid and cyanogen.

He found that prolonged heating of charcoal diminished its power of absorption, hence all the pieces of charcoal were heated as nearly equally as possible. We condense below his main results, giving the mean of all the trials reduced to 0° C. and 760 mm.

	Ammonia.	Carb.acid.	Cyanogen.
Logwood,	111·3	54·6	87·3
Ebony,	106·7	47.0	89·6
Camwood,	91·2	45·4	
Green Ebony,	90.3	40·8	
Fustic (Cuba),	89·6	58·0	
Lignum vitæ,	89·0	47·2	
Boxwood,	85·6	31·2	28·8

	Ammonia.	Carb.acid.	Cyanogen.
Logwood, (Jamaica)	69·5	33·3	33·3
Sapan wood,	69·9	32·2	32·2
Beach,	58·		
Rosewood,	50·6		
Wistaria Sinensis,	44·03		
Vegetable ivory,	50·5	57·3]	

II. CHEMISTRY.

1. *On Anilin dyes.*—HOFMANN has made the unexpected observation that chemically pure anilin does not yield coloring matters with oxydizing agents. The same is true of pure toluidin. On the other hand, a mixture of anilin and toluidin yields the characteristic colors with great facility, and it therefore appears certain that the coloring matters contain both the phenyl and tolyl molecules. Anilin blue has been found by Hofmann to have the formula $C_{76}H_{31}N_3$; its rational formula is $C_{40}H_{16}(C_{12}H_5)_3N_3$, so that it is to be regarded as rosanilin in which three eqs. of hydrogen are replaced by three of phenyl. Triphenyl-rosanilin, or anilin blue, is prepared by heating rosanilin with an excess of anilin; its formation is represented by the equation,

$$C_{40}H_{19}N_3, HCl + 3C_{12}H_7N = C_{40}H_{16}(C_{12}H_5)_3N_3, HCl + 3NH_3.$$

The free base is a white amorphous substance: the chlorhydrate has a bluish-brown color and dissolves in alcohol with a magnificent blue. The author describes several other salts, all of which are uniacid. Reducing agents convert triphenyl-rosaniline into triphenyl-leucaniline, which has the formula $C_{40}H_{18}(C_{12}H_5)_3N_3$. The iodids of methyl, ethyl and amyl act readily upon rosaniline, forming new coloring matters analogous to aniline blue and containing three equivalents of methyl, ethyl or amyl in place of three of hydrogen. The author promises further communications on the nature of anilin-green, anilin-violet and azulin.—*Comptes Rendus*, lvii, 25. W. G.

2. *On the constitution of American Petroleum.*—PELOUZE and CAHOURS have studied the oil obtained at present in very large quantities in Pennsylvania, and find that it consists essentially of homologues of marsh-gas, the lowest term of the series obtained being hydruret of butyl, C_8H_{10}, which boils at a little above 0° C., while the highest term yet studied is $C_{30}H_{32}$. The authors have obtained from these hydrurets the corresponding chlorids and in many cases the alcohols. They consider it probable that paraffin is a mixture of still higher terms in the series. Similar results have been obtained by Schorlemmer, who however finds benzol and toluol in the American oils, while Pelouze and Cahours explicitly deny the presence of these substances.—*Comptes Rendus*, lvii, 62. W. G.

3. *On Cæsium, separation from Rubidium.*—BUNSEN (*Pogg. Ann.*, cxix, 1) has made further investigations on this rare element. He could not apply Allen's method to the preparation of pure cæsium compounds because of the small quantity of the element obtainable from the sources at his command. He discovered that cæsium could be separated from rubidium by another plan, which is as follows: In a mixture of pure RbCl and CsCl the Cl is determined, and from its amount that of Rb is calculated. The chlorids are converted into carbonates, and to the latter salts a little more tartaric acid is added than is necessary to produce neutral tartrate of cæsium and bitartrate of rubidium. The mixture dried and pulverized is brought upon a funnel whose neck is stopped by a small filter, and the whole is placed in an atmosphere saturated with moisture. The neutral cæsium salt deliquesces and passes the filter while the acid rubidium salt remains behind.

Equivalent.—The cæsium tartrate was converted into chlorid and the latter was purified from traces of KO and LiO, which it acquired from the tartaric acid, by conversion into platin-chlorid, washing, &c. This operation was repeated six times, the salt being each time analyzed. The last three results coincided very closely, and from their average the equivalent 132·99 was deduced, which agrees with the number obtained by Johnson and Allen, viz: 133·03, and fully authorizes the use of the round number, 133, as expressing the combining proportional of this element.

Deliquescence of CsCl.—Bunsen further maintains the accuracy of his original assertion that CsCl is deliquescent. Johnson and Allen had stated that it was "not only not deliquescent but scarcely hygroscopic." The observations of both are entirely correct for the circumstances under which they were made. We have since observed pure CsCl to deliquesce in a few moments on an August day; while at this writing, Sept. 24th, the same specimen has been exposed all night to the air in a room with open windows, temp. 55° F., and is in appearance perfectly dry. Bunsen rightly remarks that "Johnson and Allen appear to have made their experiments in a comparatively dry atmosphere, in which as is known many deliquescent salts are unaltered, and therefore overlooked the deliquescent character of CsCl." The experiments of Johnson and Allen were made in the cloudy and foggy but cold weather of November, weather which is commonly designated as *damp*, but the real moistness of the atmosphere they only judged of from sensation.

CsCl much resembles urea in its hygroscopic relations. The latter substance is stated by most recent authors, including those who have had occasion to observe it especially, *to be unaltered in the air.* Thus Lehmann, *Physiological Chemistry;* Gorup Besanez, *Zoochemische Analyse;* Mitscherlich, *Lehrbuch;* Gerhardt, on authority of Pelouze, *Traité de Chimie Organique;* and Neubauer, *Analyse des Harns*, assert that it is unchanged by exposure to the air. Gorup Besanez, *Lehrbuch der physiologischen Chemie*, 1862, states that it is "permanent in the air but rapidly absorbs moisture." Gmelin mentions that "according to earlier statements it deliquesces in moist air." We have repeatedly observed that in very moist air urea rapidly deliquesces, though usually, except in summer weather, it is not visibly affected by atmospheric moisture.

Spectrum of Cæsium.—Johnson and Allen, in the paper above referred to, observed that "Kirchhoff and Bunsen, in the figure given by them (*Pogg. Ann.*, 1861, and *Fres. Zeitschrift für analyt. Chemie, Heft* 1, 1862), represent 11 lines. We find without difficulty 7 more lines, and observe further that some of those figured by K. & B., are not mapped in their correct position."

In relation to this, Bunsen remarks, "we regret that Messrs. J. & A. appear to have neglected to make an accurate comparison of their spectrum with ours, otherwise they would have easily convinced themselves that the lines given by them agree with ours as closely as is possible: that on the other hand three lines are figured on our plate which do not occur at all in the cæsium spectrum. These lines have been printed on a number of lithographs by fault of the lithographer, in consequence of a misunderstood proof that was not sent to us for revision, and lie near and to the right of the lines designated by J. & A. as VI, X and XV."

Johnson and Allen could but conclude that the colored plate in *Poggendorff's Ann.*, accompanying the paper of Kirchhoff and Bunsen, was a copy of their spectrum as far as the important, i. e., the characteristic and brilliant, lines were concerned. They are therefore scarcely more in fault in having "neglected to make an accurate comparison" than K. & B. are in permitting incorrect spectrum plates to be circulated. Fresenius uses the uncorrected chromo-lithograph in the new edition of his *Qualitative Analyse*, and we have never yet had the good fortune to see the lines of this element correctly mapped on any colored print.

Bunsen notices at length the inaccuracies of the diagram of J. & A., which, as the latter stated distinctly, was intended to give *approximatively* the position of the lines on Kirchhoff and Bunsen's scale, and which from the construction of the spectroscope they employed would not be exact.

The remark of J. and A. that their "line II, nearly coincident with α lithium of Kirchhoff and Bunsen, and not figured by them, is as bright as their γ cæsium, our VI (?),"—Bunsen passes over without notice. The observation is certainly not an unimportant one, for any person occupied in preparing the new alkalies is likely to be embarrassed by a line that in small instruments is practically coincident with α lithium, if it be not credited to cæsium by the authorities.

Johnson and Allen rightly state that "the yellow line VIII, is hardly less characteristic of the spectrum of *pure* cæsium than the two blue lines. It also is nearly as distinct as any of the green lines when sodium is not present in too large quantity, and is much more readily made out than the extreme red line δ of rubidium." This line was wanting in the original spectrum plate of K. & B. Chemists will be glad that Bunsen has now given, in connection with the paper we refer to, a diagram of the spectra of all the alkalies and alkaline earths, as well as of thallium, which is quite complete, and which, by following his simple directions, is readily and exactly comparable with the spectra of any instrument. In this he figures 16 lines for cæsium. S. W. J.

4. *Preliminary Notices of a New Metal;* by F. Reich and Th. Richter.—The authors have found a new metal in two Freiberg ores, which were composed principally of arsenical pyrites, blende, and some galena, together with silica, manganese, copper, and a small proportion of tin and cadmium. The ores were first roasted to get rid of the greater part of the arsenic and sulphur, then mixed with chlorhydric acid, evaporated to dryness and distilled. The impure chlorid of zinc obtained was examined with the spectroscope for thallium. No green line was seen, but the authors remarked an indigo-blue line, which was before unknown.

The authors succeeded in isolating the conjectural substance, necessarily in very minute quantity, partly in the form of chlorid, partly as hydrated oxyd, and partly in the metallic state. On submitting these, moistened with chlorhydric acid, to the spectroscope, the blue line was seen so brilliant, sharp, and persistent, that they did not hesitate to conclude that it belonged to a hitherto unrecognized metal, to which they accordingly gave the name *Indium*.

The line mentioned has a perceptibly greater refrangibility than the blue line of strontium, and there appears besides a much weaker line, of still greater refrangibility, which almost, but not quite, reaches the blue line of calcium.

The authors add that as far as they have examined the chemical properties of *Indium* they may safely assert that it is not precipitated from an acid solution of the chlorid by sulphuretted hydrogen; that from the same solution it is precipitated by ammonia as a hydrated oxyd; that the chlorid is extremely deliquescent; and that the oxyd heated on charcoal with soda gives lead-gray metallic beads, which are ductile and very soft; these heated again before the blowpipe give a yellowish coating, which on further heating takes no characteristic color with cobalt solution.—*Journal für Praktische Chemie*, lxxxix, 441, *from Chemical News*, Sept. 12, 1863.

5. *On some new volatile alkaloids given off during putrefaction.*—Dr. Crace Calvert communicated under this title to the Royal Soc'y (Feb. 1860) the preliminary results of some investigations on the products of putrid wounds, with reference to the contagion known as hospital gangrene. We have waited the conclusion of his research, but as it has not yet appeared we give the main points of his first paper. Failing to obtain a sufficient supply of the products emanating from putrid wounds, he arranged, in each of a number of small casks, twenty pounds of meat and fish, mixed with pumice stone to prevent clogging. Two tubes were adapted to the top of each cask, one of which supplied air, which was drawn through the other from near the bottom of the cask by an aspirator. The air in its passage passed through two bottles containing chlorid of platinum, which was soon made turbid by the production of a yellow amorphous powder. This precipitate collected, washed with water and alcohol, and dried, was found by analysis to contain carbon, hydrogen, and nitrogen, but what was remarkable sulphur and phosphorus also entered into its composition. The amount of these elements determined quantitatively was 11 per cent for the sulphur and 6·01 per cent of phosphorus, of the whole precipitate. By heating a quantity of the platinum salt with a strong caustic ley, a liquid, volatile and inflammable alkaloid was obtained, whilst sulphur and phosphorus remained combined with the alkali, and were easily detected. The author is satisfied from twelve months research that no sulphuretted or phosphoretted hydrogen was given off, and his researches tend to prove that the noxious vapors given off during putrefaction contain the nitrogen, sulphur, and phosphorus of the animal substance, and that these elements are not liberated in the simple form of ammonia, and sulphuretted and phosphoretted hydrogen. As putrefaction proceeds, different volatile bodies are given off. The platinum salts, heated in test tubes, give off vapors, some acid, some alkaline, of a most noxious and sickening odor, resembling putrefaction, while a white crystalline sublimate is formed, which is not salammoniac. These researches are still in progress and will occupy several years.

Photography.—

6. *Selenocyanids in Photography.*—Mr. Emerson J. Reynolds has studied the action of selenocyanids in photography, and finds that it is similar to that of the sulphocyanids. An aqueous solution of seleno-

cyanid of potassium gives a white curdy precipitate in a solution of nitrate of silver soluble in an excess of the precipitant, and the same solution will remove the trace of silver compounds present in the whites of albuminized prints after fixing with hyposulphite. While, however, the dilute acids were without action on the sulphocyanids of the alkaline metals, they decompose the corresponding selenium compounds. Selenocyanid of silver is insoluble in water, nearly so in solution of ammonia, and almost unacted on by dilute acids. It is less sensible to light than chlorid of silver, but more so than sulphocyanid of silver.—*British Journal of Photography*, x, 223.

7. *A process for the reduction of silver waste* has been discovered by Millon and Commaille which promises to be of practical value. It is based on the fact first observed by these chemists, that ammoniacal subchlorid of copper precipitates completely perfectly pure silver from a solution of nitrate of silver to which a slight excess of ammonia has been added. In the first place, the ammoniacal subchlorid of copper is prepared by dissolving 5 parts of black oxyd of copper and 4 parts of finely divided metallic copper in chlorhydric acid. When the whole is dissolved, an excess of the strongest liquid ammonia is added, which produces a clear and colorless or pale blue solution. If it be used to reduce an old bath, it is only necessary to add ammonia until the oxyd of silver is redissolved, then pour in an excess of the copper solution, and collect, wash and dry the pure reduced silver. The same reagent may also be used for reducing the silver of an old hyposulphite bath, only in conducting this process it is important to observe the following precautions: 1st, not to add too much ammonia; 2d, to have the mixture greatly diluted; 3d, to allow sufficient time for the finer metallic particles to settle completely to the bottom.—*London Journal of Photography*, x, 246.

8. *Sulphocyanid of ammonia as a fixing agent.*—Experiments have been made by G. W. Simpson and Mr. Lewitsky on sulphocyanid of ammonia as a fixing agent, employing a saturated solution of the salt for the purpose. The results seem to indicate a marked superiority over hyposulphite of soda, both as to the sharpness and the permanency of the print. The comparatively high price of the salt must, however, for a long time prevent its general use, but Messrs. Gavin and Cie announce that they can furnish it for four francs the kilogram (about thirty-seven cents a pound), and it seems highly probable that with a large demand the price may be still further reduced.—*British Journal of Photography*, x, 247, 264, also *Le Moniteur de la Photographie*, 3[e] Année, No. 5, p. 35, and No. 7, p. 51.

9. *Redevelopers.*—(*a.*) Mr. Blanchard recommends for redeveloping negatives:

Green vitriol, ...	1 gramme.
Citric acid, ...	2 "
Water, ...	100 "

This intensifier is used just like the pyrogallic acid solution, adding to it a small amount of solution of nitrate of silver, and pouring it on the negative already developed with the usual iron developer. The advantages claimed over pyrogallic acid are that the solution keeps better (improves, indeed, with age), and does not stain the negative, and that the silver is deposited in a finer condition.

(*b.*) Mr. Simpson recommends the following formula:

Green vitriol, ...	5 grammes.
Tartaric acid, ...	1 "
Water, ...	160 "

A mixture of this solution with nitrate of silver remains clear until full intensity is obtained.

(*c.*) Mr. Linsay uses for developing the negative the usual iron developer, and then adds to it, for intensifying, a small quantity of the following solution:

Citric acid, ...	3 grammes.
Nitrate of silver, ...	3 "
Distilled water, ...	100 "

—*Le Moniteur de la Photographie*, 3e Année, No. 9, p. 67.

[We have used a formula similar to the last with the best results.—J. P. C., JR.]

(*d.*) To these we may add the following formula from Mr. Liefert, a French photographer residing at San Francisco, of a developer which at once gives to negatives on a bromo-iodized collodion all the intensity desired. Dissolve together

Green vitriol, ...	30 grammes.
Purified nitre, ...	7·50 "
Water, ...	420 "
Add Acetic acid, No. 8, . . .	30 "
Alcohol at 32°, ...	7·50 "
Silver bath at 10 p. c., . . .	15 "

—*Le Moniteur de la Photographie*, 3e Année, No. 7, p. 49.

(*e.*) Mr. Charles Waldak recommends the following developers for positives on glass, the first rendering the whites dead, and the second making them brilliant and metallic:

For the dead whites.

Green vitriol, ...	5·30 grammes.
Water, ...	177·00 "
Acetic acid, No. 8, ...	7·00 "
Alcohol, ...	5·00 "
Pure nitre, ...	2·00 "

For brilliant or metallic whites.

Green vitriol, ...	4·50 grammes.
Water, ...	350·00 "
Acetic acid, No. 8, ...	3·50 "
Alcohol, ...	5·30 "
Pure nitre, ...	1·75 "
Solution of nitrate of silver (ordinary bath),	1·75 "
Nitric acid, ...	20 drops.

The second, of course, acts less rapidly than the first.

5. *Miscellaneous facts.*—Among the other facts worthy of notice in recent photographic journals are :—

(1.) Use of formic acid in developer for instantaneous views, but with somewhat discordant testimony as to its value.

(2.) Methods of separating the albuminous film from the positive print and applying it to the surface of porcelain or other ware, when the gold and silver image may be burnt into the enamel. Similar effects have also been obtained with carbon prints.

(3.) Substitution of the double sulphate of iron and ammonia for green vitriol in the developer (5 parts of the salt, 20 parts of No. 8 acetic acid, to 100 parts of water), which is said to have important advantages over the usual developer.

(4.) In the wet process, after removing the plate from the silver bath, sinking it in a large bath of distilled water, and, just before using, dipping again in the silver bath. The sensibility is said to be thus increased 50 to 100 per cent, and freedom from pin holes and stain to be better insured.

(5.) A new process for printing positives is said to have been invented by Mr. Wothly, in which chlorid of silver is replaced by less expensive materials, and the cost reduced 70 to 80 per cent, with the very great increase of sensibility; but the details were not given, as the author wishes to secure his interest by patent.

(6.) The use, in the tannin process, of acetic acid in the silver bath (2 to 4 per cent of crystallizable acid). The same photographer, Mr. Teissure, recommends the use of a large amount of acetic acid in the developer, which protects the white of the image, and also dipping the plate in the silver bath just before developing, and after it has been dipped into distilled water. J. P. C., JR.

III. AGRICULTURAL CHEMISTRY.

1. *On the fertilizing action of Gypsum.*—Dehérain, in a note presented to the Academy of Sciences at Paris (*Comptes Rendus*, 18 May, 1863), gives the results of some new studies on this much discussed question. The idea that plaster favors the production of nitre in the soil is not sustained by Dehérain's experiments, as he found that nitrification went on as slowly in plastered soil as in soil destitute of gypsum.[1] He also found that gypsum is without influence in assisting the formation of ammonia in the soil, and unlike caustic lime, does not favor the solution of phosphates.

Convinced that the fertilizer in question operates almost exclusively by its effects upon the soil, and observing that those crops which are most benefitted by plastering, viz: the Leguminosæ, contain a large amount of potash, Dehérain was led to inquire whether gypsum exercised any solvent effect on this substance in the soil. For this purpose a variety of soils were examined, 1st, in their natural state, and 2d, after mixture with 10 per cent of gypsum,—this large quantity being employed to bring

[1] The practice of farmers in some parts of Switzerland (Berne) sufficiently indicates that gypsum has a preservative instead of an oxydizing effect on organic matters. By its use, stable dung, it is said, may be kept quite fresh and unaltered for a whole year, though it acquires an odor of sulphuretted hydrogen. S. W. J.

out any result the more sensibly. Some of the soils were also limed, but, while in the plastered soils a decided quantity of potash was made soluble in cold water, in the limed soils no such effect was produced.

The subjoined table includes Dehérain's results:—

Grammes of potash extracted by cold water from 1 *kilog. of air-dried soil.*

Soils.	From the normal soil.	From the plastered soil.	Difference.	Duration of the experiment.
Black earth of Russia, *Tschernosem,*	0·048	0·136	+0·089	4 months.
"	0·048	0·140	+0·092	15 days.
"	0·048	0·288	+0·240	1 month.
"	0.048	0·428	+0·380	1½ "
Black earth, another specimen,	0·128	0·138	+0·010	1 "
Soil from Chapelles,	0·017	0·115	+0·098	1 "
" Verclives (asparagus bed),	0·487	0·556	+0·069	1 "
" Rio-Parana,	0·003	0·067	+0·064	1 "
" Sologne,	0·192	0·202	+0·010	1 "
" Jardin des Plantes,	0·046	0·355	+0·309	24 hours.

The above experiments were made on such soils as happened to be at hand. Other trials were afterwards instituted on such soils as experience had taught were either considerably benefitted or quite unaffected by plastering. The results were as follows:—

Grammes of potash extracted by cold water from 1 *kilog. of air-dried soil.*

Soils.	Potash in the normal soil.	Potash in the plastered soil.	Difference.	Duration of the experiment.
Soil from Eragny—never plastered.	0·084			
" Alfort "	0·082			
" Gueritaude—plastered with great advantage,	traces.	0·105	0·105	12 hours.
" Gueritaude, another specimen,	traces.	0·192	0·192	" "

Dehérain remarks that these experiments fully establish the fact that gypsum acts by liberating potash, and explains why wood-ashes are often substituted for plaster to advantage, for they afford a direct supply of potash: finally, he gives as a reason of the utility of sowing plaster on the growing crops, that the potash is rendered soluble at a time when the plant can take it up, and is not washed out of the soil as it might be (?) if the latter were not occupied by vegetation.

In regard to the mode in which gypsum favors the solubility of potash, Dehérain refers to the experiments of Huxtable & Thompson, Way, and Brustlein, on the absorptive power of soils, who found that the soil, while it retains in solution, certain substances that may be brought in contact with it, allows others to pass through more or less completely. In repeating these experiments, Dehérain found that plastered soils in general permitted the filtration of saline matters more readily than normal soils, and this effect was especially true of carbonate of potash. In a comparative experiment made on plastered and unplastered soil from Indre-et-Loire, put in contact with carbonate of potash, the result was that the normal soil retained all but traces of alkali, while the plastered soil allowed 0·472 grm. to pass it.

Other researches led to the discovery that *bicarbonate of potash* is scarcely more retained by normal than by plastered soils.

Note.—We venture to assert that the action of gypsum upon the soil is more complicated than appears from Dehérain's experiments, and it is greatly to be regretted that his analyses were not extended to embrace all the changes effected by gypsum. Dietrich, in an extended study of the solvent influence of various agents on soils and rocks, has anticipated Dehérain in the discovery of the general fact that gypsum liberates alkalies from some of their combinations which may occur in the soil. He found that sulphate of lime kept in moist contact with powdered basaltic rock for a few days makes soluble a considerable amount of alkalies.

In his experiments, 100 grms. of powdered basalt were mixed with 10 and another equal portion with 20 grms. of gypsum, and the mixtures were kept moist at the ordinary temperature for ten days. Each mixture was then treated with its own weight of water and immediately filtered. In the first solution were found 0·125 grm. and in the second solution 0·175 grm. of alkali-chlorids. In another experiment 100 grms. basalt-powder were boiled 9 hours in 300 c. c. water with 10 grms. gypsum. In the filtered liquid were found 0·337 grm. alkali-chlorids.—*Jour. für Prakt. Chem.*, lxxiv, 129, 47.

The mode in which gypsum acts in liberating potash doubtless belongs to that class of substitutive reactions which Way and Eichhorn first distinctly brought to notice. The writer, in an article on "Some Points of Agricultural Science" (this Jour., July, 1859, pp. 71–85), gave a notice of the researches of Way and Eichhorn, and adduced other instances of similar actions. Wolff found that the ashes of buckwheat-straw which was raised on a soil manured with common salt contained less chlorid of sodium and more chlorid of potassium than ashes grown on the same soil without this addition, a displacement of potash by the soda having occurred in the soil. Eichhorn found a similar substitution of soda and ammonia for potash in a soil by treating it with solutions of common salt and carbonate of ammonia, (*Wilda's Centralblatt*, 1858, ii, 171). These results were confirmed and extended by Dietrich, *loc. cit.*, by Voelcker (*Jour. Roy. Ag. Soc. of Eng.*), and by all other investigators who have made a thorough study of the absorbent power of soils. From these and other facts mentioned in the paper referred to, the writer concluded as follows: "However complicated and obscure these reactions may be, it is plain, that, henceforth, *the effect of a solution of one base in displacing other bases from native hydrated aluminous and ferric silicates, and of one acid upon the compounds of other acids with oxyd of iron and alumina, must be considered in the theory of the action of saline manures.*"

In a course of lectures on Agricultural Chemistry, delivered at the Smithsonian Institution, the writer remarked that—"the facts brought to light by the researches of Way, Eichhorn, and Voelcker, already described, indicate another general mode by which fertilizers, especially soluble saline bodies, may operate indirectly. The investigations referred to show that the bases (and acids?) may replace each other in insoluble or slightly soluble combinations, *i. e.*, soluble lime may displace insoluble potash, making this soluble and becoming insoluble itself. Soda may, in the same manner, displace lime or potash, or ammonia, the rule being that the body *in excess* goes into combination, and expels those before combined. We observe here a tendency to bring all the bases into what we may designate as an equilibrium of solution. This principle appears adapted more than any other yet discovered to generalize the phenomena of indirect action, and to enable us to foresee and explain them."—*Smithsonian Report* for 1859, p. 192.

Further investigation will very likely demonstrate that the solvent effects of gypsum are not confined to potash, but also extend to magnesia and in some cases also to phosphoric acid.

There is another mode in which gypsum acts that in many cases is perhaps no less influential than the one just noticed. In the writer's 4th Smithsonian Lecture (p. 191 of the *Report*) may be found the following passages:

"Gypsum, common salt, carbonate of lime, nitrates of potash and soda, and in fact all the saline compounds which are incorporated with the soil in manures, may exert important physiological effects on the plant in addition to their mere nutritive function.

The transpiration of water through the plant is very remarkably hindered when lime, potash, or the salts just named are present in the absorbed liquid. This fact, observed for the first time by Mr. Lawes, in 1850, and recently brought again more strikingly into notice by Dr. Sachs, of Tharand, Saxony, appears to be of great importance in the theory of manures. Dr. Sachs experimented on various plants, viz: beans, squashes, tobacco, and maize, and observed their transpiration in weak solutions (mostly containing one per cent) of nitre, common salt, gypsum, (one-fifth per cent solution) and sulphate of ammonia. He also experimented with maize in a mixed solution of phosphate and silicate of potash, sulphates of lime and magnesia, and common salt, and likewise observed the effect of free nitric acid and free potash on the squash plant. The young plants were either germinated in the soil, then removed from it and set with their rootlets in the solution, or else were kept in the soil and watered with the solution. The glass vessel containing the plant and solution was closed above around the stem of the plant by glass plates and cement, so that no loss of water could occur except through the plant itself, and this loss was ascertained by daily weighings. The result was that all the solutions mentioned, except that of free nitric acid, quite uniformly retarded transpiration to a degree varying from 10 to 90 per cent, while the free acid accelerated the transpiration in a corresponding manner.

As the processes of elaboration—the chemical and structural metamorphoses going on within the cells of the plant—require time for their performance, we can easily perceive that a too rapid upward current of liquid, by diluting the juices, might measurably interfere with the assimilation of the food, and that the presence of a body may be no less useful by its regulating influence on the circulation of the water than by contributing an ingredient necessary for the formation of the substance of the plant itself.

It is also obvious that if a substance added to the soil retard the transpiration of water through vegetation, a given store of hygroscopic moisture in the soil will serve the needs of vegetation longer—will reach further into time of drought than it otherwise could. Dr. Sachs found that gypsum exerted the greatest effect in preventing loss of water, and this observation gives a scientific ground of evidence to the opinion long maintained among farmers, but rejected by men of science (and very properly, as no cause could be discovered for such an effect, and the effect is not capable of measurement in field culture), that gypsum has the influence of a body that attracts moisture."

Since the above was written, we find that Liebig has also anticipated Dehérain in the discovery that potash is liberated from the soil by the action of a solution of gypsum. From 1000 grms. of soil from a wheat field, 3 liters of pure water extracted ·0243 grm. of potash, while the same amount of saturated solution of gypsum removed from the same quantity of soil ·0436 grm. or nearly twice as much potash. *Die Naturgesetze des Feldbaues*, p. 360, 1862. Liebig also observed (*loc. cit.*) that *magnesia* is liberated by gypsum to a considerable degree. His results are given in the following table.

From 300 grms. of soil are dissolved by a liter

	of distilled water.	of gypsum water.
	grms. of magnesia.	
Soil of Bogenhausen,	0·0302	0·0706
" Schleisshein,	0·0316	0·0878
Subsoil of Bogenhausen,	0·0122	0·0842
Soil of Botanical Garden,	0·0454	0·1686
" Bogenhausen, No. I,[2] unplastered,	0·0266	0·1016
" " No. II,[2] plastered,	0·0382	0·0480
" Schornhof,	0·0086	0·0634
" Alabama cotton field,	0·0019	0·0038

Liebig contents himself with communicating the simple results of his experiments without venturing to draw any definite conclusions from them. We remark that they fully confirm our views above expressed.

S. W. J.

IV. MINERALOGY AND GEOLOGY.

1. *On the phosphatic (or guano) rock from the Island of Sombrero, W. I.* —A specimen of the phosphatic rock from the Island of Sombrero has been described by Dr. T. L. Phipson[1] as a new mineral species under the name "*Sombrerite.*"

The characters given by Phipson are as follows:—

"Sombrerite presents itself in nature as a white, yellowish white, or reddish colored rock, having a straight fracture, and, in some portions, a peculiar horny aspect; its appearance is, in general, compact, though in reality the rock is very porous. It shows no signs of crystallization whatever, but appears like an amorphous, gelatinous phosphate that has been submitted to a high temperature. It is thought to be of comparatively recent geological origin, as it encloses fossil bones (of mammalia?) and several kinds of shells. In a mineralogical point of view, sombrerite is a compound of phosphate of lime with phosphate of alumina; in some specimens a portion of the alumina is replaced by a corresponding proportion of sesquioxyd of iron; in others, where little or no iron is present, the mineral adheres to the tongue like other

[2] This soil is one whose productiveness is greatly enhanced by plastering.

[1] *Journal of the Chemical Society*, xv, 277.

aluminiferous minerals. Sombrerite is not phosphorescent by heat like apatite; before the blowpipe, when moistened by sulphuric acid, it colors the flame pale green. It contains no fluorid or chlorid of calcium.

The specific gravity of sombrerite is 2·52. A well chosen specimen has given me the following composition:—

		Atomic ratio.	
Water,	9·00	1·00	20
Phosphate of lime $\dot{C}a^3\overset{\cdot\cdot\cdot\cdot\cdot}{\bar{P}}$,	65·00	0·41	8
Phosphate of alumina $\overset{\cdot\cdot\cdot}{\bar{A}}l^2\overset{\cdot\cdot\cdot\cdot\cdot}{\bar{P}}^3$,	17·00	0·05	1
Carbonate of lime,	5·00		
Chlorid of sodium,	1·44		
Sulphate of lime,	1·36		
Silica,	1·00		
Crenate of ammonia, &c.	0·20		
	100·00		

The formula of sombrerite is therefore $8\dot{C}a^3\overset{\cdot\cdot\cdot\cdot\cdot}{\bar{P}}+\overset{\cdot\cdot\cdot}{\bar{A}}l^2\overset{\cdot\cdot\cdot\cdot\cdot}{\bar{P}}^3+20\dot{H}$."

Dr. Phipson asserts that this compound forms a large portion of some small islands in the West Indies, especially of Sombrero Island. As this statement is at variance with the facts known to those who have had the opportunity to see any considerable amount of the phosphatic rock from Sombrero, we here insert the following observations on Dr. Phipson's article, communicated to us by Mr. Alexis A. Julien, *resident chemist at Sombrero.*

2. *Observations on the Sombrero Guano;* by A. A. Julien.—In anticipation of a paper on the subject of the guano Island of Sombrero, which I hope soon to present, it is proper for me, from my peculiar advantages, to point out some of the errors into which Dr. Phipson has fallen in this proposal to distinguish the Sombrero guano as a new mineral species.

He commences by saying: "This mineral forms a large portion of some small islands in the West Indies, especially of Sombrero Island." The only other islands in the West Indies, besides Sombrero, on which the rock guano has been found to occur, are Les Monges, El Roque, etc., off the coast of Guiana and Venezuela. The composition of the rock guano, however, from these islands is very variable, and, as the published analyses show,[2] differs materially (except in general character) from that of the Sombrero guano, and altogether from the representative analysis for "Sombrerite" which Dr. Phipson offers further on.

In the course of the brief and very imperfect description of the physical characters of the Sombrero guano given by Dr. Phipson, he says: "It shows no signs of crystallization whatever, but appears like an amorphous gelatinous phosphate that has been submitted to a high temperature." I have recently, however, discovered in certain localities small but perfect crystals of bone phosphate of lime, (together with a small proportion of carbonate of lime and phosphate of magnesia,) in the examination of which I am now engaged. Again, there is a natural division of the Sombrero guano into two varieties. One of an *oolitic* structure, of a great variety of colors, and containing, in addition to the bone ($3CaO, PO^5$) and neutral ($2CaO, HO, PO^5$) phosphates of lime, the

[2] This Journal, [2], xxii, 299, 1856. *Proc. Phil. Acad. Nat. Sci.*, March, 1857. This Journal, [2], xxiii, 120, 1857. *Appleton's Encyc.*, article "Guano."

phosphates of alumina, iron, and magnesia, organic matter, silica, etc. The *other* variety, generally of a broad *concretionary* structure, is of a white or yellowish-white color, containing a little carbonate of lime, sulphate of lime, etc., but especially abounds in bone phosphate of lime. From these and other evidences, it is almost certain that the former more nearly resembles the original deposit and is the older of the two; while the latter is far more uniform in composition. These varieties are so characteristic that it would be difficult to find a block of this guano of a cubic foot in dimensions in which one or both of them would not be perceptible. The term "amorphous" can only be applied to those comparatively rare varieties in which these characteristics are mixed, or entirely indistinguishable on account of the uniformity in color of the oolitic grains and their cement. Again, as the guano is interlaminated with ordinary coral limestone, and I have found all the phenomena it presents to be plainly attributable to atmospheric agents, there is no call for the conjecture of a "high temperature," nor for the following strange theory proposed in the latter part of the article: "I look upon this rock as having found its way to the surface at a high temperature, in contact with water or steam, and under great pressure."

The "several kinds of shells," which Dr. Phipson then mentions among the fossils enclosed in the guano, belong entirely to the coral limestone which is very generally converted into phosphate of lime, wherever it is in contact with the masses of guano, or has been exposed in the veins to percolating guano solutions. The only synchronous fossils of the guano, yet discovered, are a variety of bones, the shells of a crab, rootlets, and a small coral.

As a representative analysis of "Sombrerite," Dr. Phipson offers that of "a well chosen specimen,"—the absurdity of which expression will be understood from what has been already stated. The Sombrero guano is as variable in composition, particularly in its content of earthy phosphates, as any other phosphatic guano, and as little entitled to rank as a new species. Dr. Phipson cannot possibly have examined with any care a single cargo,—I venture to say, not even a single *ton;* for there is no natural standard by which a representative specimen could be "well chosen" or chosen at all.

Further, Dr. Phipson does not inform us of the methods used by him in his analysis. This omission is unfortunate when we take into account the prevailing uncertainty among chemists as to the most exact mode of estimating phosphoric acid, in the presence of lime, iron, and alumina, also on account of some other curious results arrived at. Firstly, as to the 9 per cent of water: it is to be presumed that the "well chosen specimen" was carefully dried to remove moisture, which varies several per cent in different varieties. It is also to be presumed, from the analysis, that "Sombrerite" contains no neutral phosphate of lime, with which a part of this 9 per cent of water may be combined,[3] and that the two equivalents of water, combined in the "sulphate of lime," have been deducted from the gross amount. The variety of this guano which I

[3] Notwithstanding that the presence of phosphoric acid in the filtrate, after long washing with hot water, has proved the existence of this salt in at least some common varieties.

have described as of concretionary structure especially predominates throughout the southern part of Sombrero Island, and over one-half our cargoes are composed of it. It contains on an average 83 to 85 per cent of bone phosphate of lime—*less than one per cent of phosphate of alumina and often none.* We may very properly ask if this fact is to be entirely ignored in the consideration of the composition of "Sombrerite"?

The article concludes with the following objection to the use of this guano in the manufacture of superphosphate of lime: "In producing the latter, Sombrerite gives rise to a certain quantity of sulphate of alumina, and this salt, being deliquescent, attracts and retains so much moisture that the product can be dried only with great difficulty,"—an objection applicable with the same force to the 1·44 per cent of similarly deliquescent chlorid of sodium. But every practical chemist is aware that in this manufacture only a portion of the phosphate of lime of the material is ever intentionally and actually converted into superphosphate. It cannot be that Dr. Phipson supposes that this "sulphate of alumina" can be formed and exist in the presence of an excess of bone-phosphate of lime. Finally, he has but to consult that same "manufacturer of superphosphate of lime" to learn how utterly groundless is his objection *in fact* as well as in theory.

Such of these observations as depend merely upon my own statement I shall hereafter more fully elucidate. We may safely conclude that a material so heterogeneous as the Sombrero guano does not possess the uniformity of composition requisite for distinction as a mineral species.

2. *On the nature of Jade, and on a new mineral species described by Mr. Damour*; by T. Sterry Hunt, F.R.S. (From the *Comptes Rendus* of the French Academy of Sciences, June 29, 1863).—"The name of jade was given by the elder De Saussure to a compact whitish mineral from Switzerland, which, as it occurs in rolled masses on Lake Leman (Geneva), was called by Delameter, *lemanite*, and had been described by Haüy as a variety of feldspar, forming with smaragdite the rock to which the founder of crystallography gave the name of euphotide. This jade, according to De Saussure, scratched quartz and had a specific gravity of 3·32–3·40. It was not attacked by acids, and yielded De Saussure by fusion a soft glass, having a density of 2·8. The younger De Saussure afterwards gave to the jade described by his father the name of Saussurite. Subsequently to this, Boulanger, and afterwards Delesse, submitted varieties of euphotide for examination. These chemists mistook for saussurite a white mineral often presenting the cleavages of a feldspar attackable by acids, and having the composition of labradorite, with which the jade of De Saussure was henceforth confounded, so that the name of euphotide became a synonym for a variety of diorite or hyperite. (Rose, D'Halloy, Senft)."

"Mr. Damour showed some years since that certain ornamental stones from the East, known by the name of jade, had the composition of a compact hornblende or tremolite, and a density of 2·97. This substance was however far removed alike from the jade of De Saussure and the feldspars of Delesse, and I was led, about four years since, to make an examination of authentic specimens of the euphotide of Switzerland (from the Valley of Sass, Mont Rose), which were kindly furnished by

Prof. Guyot. The results of my researches appeared in March, 1859, in a memoir upon Euphotides, in the American Journal of Science, [2], vol. xxvii, p. 336. I there showed that the jade, which forms the base of the true euphotides, possesses all the characters ascribed to it by De Saussure. It has a density of 3·33–3·38, a hardness equal to quartz, and is only attacked by acids after being heated to whiteness. This mineral is always compact, tough, with a somewhat conchoidal fracture. Its color is whitish, with shades of green, blue or red. It sometimes encloses lamellæ of a triclinic feldspar, which is attacked by acids and resembles labradorite; its presence serves to explain the error of those who have confounded this species with the true jade or saussurite of euphotide. The saussurite was also accompanied, and often penetrated, by a silvery talc, sometimes with actinolite. The smaragdite of the euphotide is a grass-green pyroxene, sometimes mixed with hornblende, and yielding, by analysis, chrome, nickel, and traces of cobalt."

"The analyses of two specimens of the jade from the euphotide of Monte Rosa (density 3·33 and 3·38) showed it to be a silicate of alumina and lime, with two or three hundredths of soda, giving for the oxygen ratios of the silica, alumina, and protoxyd, nearly 3 : 2 : 1, which are those of meionite (a species belonging to the family of the wernerites) and also of zoisite. The saussurite is however by its specific gravity, its hardness, and its chemical characters, far removed from the wernerites, and related to the family of the garnets, epidotes and zoisites. I therefore referred it to this last species, the jade of De Saussure."

"In the memoir from which the foregoing results are cited, I insisted upon the relation of isomerism, or rather of polymerism, which exists between meionite and zoisite, and I remarked that the augmentation of hardness, of density, and of chemical indifference which is seen in this last species, is doubtless to be ascribed to a more elevated equivalent, or in other words, to a more condensed molecule. These different degrees of condensation, which are constantly kept in view in the study of organic chemistry, are besides, as I have already elsewhere shown, of great importance in mineralogy, and will form the basis of a new system of classification, which will be at the same time chemical, and natural-historical. (*Comptes Rendus*, 1855, xli, 79). The different rhombohedral carbon-spars, kyanite and sillimanite, hornblende and pyroxene, offer in like manner examples of different degrees of condensation, and by their chemical composition belong to series, the terms of which, like those of the hydrocarbons nC_2H_2, are both homologues and multiples of the first term. At the same time each one of these carbonates and silicates belongs to another possible series, the terms of which differ by nM_2O_2, corresponding to more or less basic salts."

"Meionite, with the ratios 3 : 2 : 1, is the most basic term known of the series of the wernerites. The proportion of silica in these minerals augments until we find in dipyre the ratios 6 : 2 : 1, with density which does not exceed 2·66. We might then expect to find a silicate which should be to dipyre, what zoisite or saussurite is to meionite, and Mr. Damour has recently had the good fortune to meet with such a mineral in a specimen of jade from China, of which he has recently given the description and the analysis (*Comptes Rendus*, May 4, 1863). This substance closely resembles in its physical and chemical characters the saussurite or jade of Monte Rosa, of which it has the density (3·34, Damour). It is a silicate of alumina, lime and soda, and gives the same empirical chemical formula as dipyre. We may then expect to find between saussurite and this new species, to which Mr. Damour gives the name of *jadeite*, other jades having formulas which will correspond with the wernerites intermediate between meionite and dipyre. Mr. Damour has just enriched the science of mineralogy with a new species, which from the considerations above given assumes a high importance. By its hardness, its specific gravity, and its indifference to acids, jadeite is completely separated from the wernerite group, and takes its place along side of zoisite or saussurite, with the garnets, idocrase and epidotes. The following table will serve to show the relations of the new species:

Density, about - - -	2·7	3·3
Ratio 3 : 2 : 1, - - -	Meionite.	Saussurite.
Ratio 6 : 2 : 1, - - -	Dipyre.	Jadeite."

To the above translation we add from the original paper of Mr. Damour, already cited, the following details. The new mineral, which is brought from China in the form of carved ornaments, has a fine green color, like that of chrysoprase, bordering on an emerald-green. It is somewhat translucent, with a scaly fracture, and a texture which is finely lamellar and occasionally somewhat fibrous. Its hardness is between that of orthoclase and that of quartz. Before the blowpipe it fuses readily into a transparent blebby glass. Its analysis by Damour gave

Silica, - - - - - - - - - - -	59·17
Alumina, - - - - - - - - - - -	22·58
Soda, and a trace of potash, - - - - - -	12·93
Lime, - - - - - - - - - - -	2·68
Magnesia, - - - - - - - - - - -	1·15
Protoxyd of iron, - - - - - - - -	1·56
	100·07

In his published note Mr. Damour was disposed to class this new mineral with the wernerites, but subsequently, in a private letter of the 9th July, he agrees with me in placing it near the epidotes, and adds the important observation recently made by Des Cloizeaux, that jadeite presents two axes of polarization. This species, unlike the saussurite, is not attacked by acids after fusion, a fact which is to be ascribed to the greater proportion of silica which it contains. Mr. Damour found for the specific gravity of three specimens of jadeite the numbers 3·33, 3·34 and 3·35.

Montreal, Aug. 12, 1863.

3. *Geological Survey of Canada,—Report of progress from its commencement to* 1863; illustrated by 498 wood cuts in the text, and accompanied by an atlas of maps and sections. Montreal: Dawson, Brothers, 1863.—The annual Reports of the Geological Survey of Canada have from time to time been noticed in this Journal. The large volume just now issued consists of a general review of the results thus far obtained in its various departments; in stratigraphical geology by Sir Wm. E. Logan, and his assistant Alexander Murray, in paleontology by Mr. E. Billings, and in mineralogy, lithology and chemistry by Prof. T. Sterry Hunt. As

should be expected from such a corps, every part of the volume is the result of thorough and laborious research. The volume commences with a geographical description of the country, and of its relations to the features of the continent. It then takes up the geology of Canada, and, commencing with the Laurentian system, gives full details as to the structure, distribution, etc., of the formations in order. Numerous figures and descriptions of fossils are introduced in connection with the account of the successive fossiliferous strata, making the work one of great value as an exhibition of general American geology. The Laurentian and Huronian systems, so beautifully worked out in the course of the survey, and also the Quebec group, so rich in its contributions to our knowledge of the ancient life of the globe, are described with full details and many illustrations.

At the 454th page commences the Lithological part of the volume, which occupies 220 pages. Professor Hunt here treats of the special characters of the rocks, minerals, and mineral waters, gives numerous analyses, and discusses, with many original views, the formation of minerals, rocks, and mineral veins, and also the general subject of metamorphism, which, as the volumes of this Journal have shown, his researches have done much to elucidate. Economic geology is next taken up by him, and a vast amount of information embodied in the following 160 pages, commencing with the ores of iron and other metals, and continuing with the subjects of phosphates, marls, mineral paints, peat, bitumen, plumbago, clays, building stones, &c.

The volume closes with a table of the equivalents of American and foreign rocks, a list of Lower Silurian Fossils, indicating by asterisks the formations in which the species occur, a list of Graptolites, and a number of pages of additional figures of fossils.

It is announced that the following maps will form an Atlas to be issued to accompany the volume.

1. A colored geological map of British North America, and of the adjacent parts of the United States; on a scale of 125 miles to an inch. This is reduced from a map on a scale of 25 miles to an inch; which is already engraved, and will be published separately.

2. A detailed map of the Laurentian rocks in the counties of Ottawa, Terrebonne, Argenteuil, and Two Mountains; on a scale of seven miles to an inch. This map shows the distribution of the limestone, anorthosites, and intrusive rocks of the Laurentian system, and also the outcrops of the adjacent Lower Silurian formations.

3. A detailed map of the Huronian series, along the north shore of Lake Huron, showing also the relations of the Laurentian and Silurian systems; on a scale of eight miles to an inch.

4. A detailed map of a portion of the Quebec group, from Stanbridge, C. E., to St. Albans, Vermont, showing also its relations to the Potsdam and Trenton groups; on a scale of two miles to an inch.

5. A map of the limestones of the Quebec group at Point Lévis; on a scale of a mile to three inches.

6. A map of the Superficial Geology, which will be upon the same scale as number 1.

All of these are engraved on steel or copper, and, with the exception of 5, will be printed in oil colors by chromo-lithography.

The sections, which will be about ten in number, it is proposed to give on a scale of five miles to an inch.

This Atlas will not be ready before the end of the present year.

The Canadian government has already made appropriations for the continuation of the Geological Survey another five years.

4. *Air-breathers of the Coal Period:* a descriptive account of the remains of land animals found in the coal formation of Nova Scotia, with remarks on their bearing on theories of the formation of coal and of the origin of species; by J. W. Dawson, LL.D., Principal of McGill University. With illustrations. Bailliere, New York and London, 1863. Montreal: Dawson, Brothers.—This is a highly important memoir on the Air-breathers, or terrestrial species of animals, of the Carboniferous age, found, and mostly by the author, Dr. Dawson, in the rocks of Nova Scotia. It runs through four recent numbers of the "*Canadian Naturalist*" of Montreal, and is here presented entire in a pamphlet of 82 pages, illustrated by seven plates, one of them photographic.

Dr. Dawson differs from Owen, and justly we believe, in his conclusions respecting the Archegosaurian nature of some of the species. We cite a brief abstract of the general results from pages 65–67 of the memoir.

VERTEBRATA.—Reptilia. Order Microsauria. Genus *Hylonomus.*—Reptiles or batrachians; with simple teeth in one series; biconcave vertebræ with arches anchylosed to them; ribs long and bent; limbs developed for walking; cranial bones smooth or nearly so; body protected below with oval or ovate bony scales, and above with horny scales and other appendages.

1. *Hylonomus Lyelli*, Dawson.—Teeth elongated, conical, thirty-six in each side of the jaw; larger toward the anterior part of the lower jaw; length of the lower jaw ·7 inch; limbs well developed, especially the posterior pair; bony scales oval; body above with imbricated horny scales, and rows of angular and bristly points.

2. *Hylonomus aciedentatus*, Dawson.—Teeth of maxillary and mandible thick wedge-form, or nearly round at base and flattened to an edge at top. Teeth of intermaxillaries cylindric, bluntly pointed, and with spiral furrows at the point. Number of teeth about forty on each side of jaw; length of lower jaw about 1 inch. Size more than twice that of *H. Lyelli.* Dermal covering, so far as known, similar, but the parts large in proportion.

3. *Hylonomus Wymani*, Dawson.—Teeth obtusely conical, about twenty in each side of the jaw; length of lower jaw about ·25 inch. Vertebræ elongated; size much smaller than that of *H. Lyelli.* Bony scales small and rounded, body above probably clothed in imbricated horny scales.

Order Labyrinthodontia. Genus *Baphetes.*—*Baphetes planiceps*, Owen. —Teeth conical, hooked, striated longitudinally, and with inflected and convoluted cement; in two series; the inner of larger size. Cranial bones much corrugated. Head broad; breadth in front of orbits 6 inches; length from this line to front of snout 3½ inches. Probably a dermal covering of corrugated bony scales.

Genus *Dendrerpeton.*—Batrachians with a double series of teeth; the outer simple and flattened conic, the inner conical with inflected folds of cement. Teeth also on the vomer. Bones of skull corrugated; body protected below with long ovate or rhomboid bony scales, and above with imbricated horny scales. Form elongated, fore limbs largest, tail natatory, vertebræ biconcave, neural arches and bodies ossified.

1. *Dendrerpeton Acadianum*, Owen.—Inner teeth straight conical; outer teeth short and obtuse. Length of head 2·75 inches, breadth at orbits about 2 inches, distance of orbits ·7 inch. Length one to two feet.

2. *D. Oweni*, Dawson.—Teeth slender and hooked, and cement of inner teeth more perfectly inflected. Length of skull 1·2 inch, distance of orbits about ·5 inch; length one foot or less.

Order ARCHEGOSAURIA. Genus *Hylerpeton.—Hylerpeton Dawsoni*, Owen.—Teeth simple, bluntly conical, with large pulp cavity; about 13 in one side of the jaw. Two of the anterior teeth of the upper jaw twice as large as the others, and deeply sunk in the jaw. Length of lower jaw 1·3 inch. Bones of skull puncto-striate. Limbs unknown, probably natatory.

SEDIS INCERTÆ. Genus *Eosaurus.—Eosaurus Acadianus*, Marsh.—Known by two biconcave vertebræ 2·4 inches in diameter and much resembling the caudal vertebræ of *Ichthyosaurus*. See paper by Mr. O. C. Marsh, *Silliman's Journal*, vol. xxxiv.

ARTICULATA.—MYRIAPODA. Genus *Xylobius.—Xylobius Sigillariæ*, Dawson.—Body crustaceous, elongate, one to two inches in length, articulate; when recent, cylindrical or nearly so, rolling spirally. Feet small, numerous; segments 30 or more; anterior segments smooth, posterior with transverse wrinkles, giving a furrowed appearance. In some specimens, traces of a series of lateral pores or stigmata. Labrum? quadrilateral, divided by notches or joints into three portions. Mandibles two-jointed, last joint ovate and pointed. Eyes ten or more on each side.

MOLLUSCA.—GASTEROPODA. Genus *Pupa.—Pupa vetusta*, Dawson, Cylindrical, tapering toward the apex; surface shining, minutely marked with longitudinal ridges; whorls 8 or 9, rounded, width of each equal to half the diameter of the shell; aperture rather longer than broad; outer lip regularly rounded and somewhat reflected; pillar lip straightened above, rounded below. Edentulous or with faint ridges on columella?. Length ·3 inch or a little more.

On the concluding page there is also the following note:

While these sheets were passing through the press, I have for the first time been enabled to study von Meyer's plates of the coal reptiles of Germany. They confirm my previous impression of the generic and probably family distinctness of *Dendrerpeton* and *Hylonomus* from *Archegosaurus*. The former of the two genera named is however that which approaches most nearly to von Meyer's genus. The arrangement of the teeth in *A. latirostris* much resembles that in *Dendrerpeton*, and the scales on the throat and belly are similar in form and arrangement. The form of the skull, and the proportions of its bones, are, however, quite different in the two genera. The vertebræ of *Dendrerpeton* are also much more perfectly ossified, its ribs very much larger and more bent, and its limbs much larger and adapted for supporting the body on land. *Archegosaurus* must have been in all respects more ichthyoid and aquatic than any of the species of *Dendrerpeton* or *Hylonomus.*

The skull figured by von Meyer under the name of *Sclerocephalus Hauseri* may have belonged to an animal more nearly allied to *Dendrerpeton* than were the species of *Archegosaurus.*

If the animals of the type of *Archegosaurus* existed in the coal period in Nova Scotia, their remains would not be likely to occur in such repositories as the erect trunks of Sigillariæ, but only in strictly sub-aqueous deposits.

V. BOTANY AND ZOOLOGY.

1. *Origin of Varieties in Plants.*—According to the Gardeners' Chronicle, quoting from *Les Mondes*, " M. Decaisne, the very able Professor of Cultivation in the Garden of Plants, Paris, has lately brought under the notice of the Academy of Sciences some very interesting remarks upon the varieties of cultivated plants in general, and of Pears in particular." Decaisne remarks :—" The almost unlimited and ever increasing number of varieties in fruit-trees, vegetables, and all useful plants is a fact to which science has at present given too little attention. . . . These new forms, what are they ? Can they be, as has been recently asserted, true species which have remained unknown up to the time when they were first subjected to cultivation ; or are they merely modifications of ancient known species, assuming various appearances according to climate or situation ? It may appear strange that such a question should be brought before the Academy, so natural does it seem for a species to be subject to change." . . . Botanists in the present day may be divided into two schools. The more ancient, which may be called the Linnæan, admits the changeableness of species ; within certain limits, no doubt, though it is not always easy to define them. Hence those large polymorphous species, sometimes vaguely defined, but generally easy to characterize by a short descriptive phrase. The other school, which belongs especially to our own time, and which may be designated as the school of immutability, denies most positively any tendency to variation in the vegetable kingdom. According to it, the forms of species never alter in the slightest degree ; and when two plants of the same genus present any palpable difference, however slight it may be, these two plants form two species, radically distinct from the beginning of things. According to M. Jordan of Lyons, a very eloquent advocate of the modern school, all the races and all the varieties admitted by the ancients become so many species. In his eyes, all our races and all our varieties of fruit-trees, of Pears amongst others, are distinct, unchangable species, always preserving their own characteristics from generation to generation. Hence it follows that these trees did not proceed, as is commonly believed, from a single or even from a few specific types, which cultivation has caused to vary, but from as many original types as there are perceptible varieties."

Although the absurdity of the latter view seems evident enough, Decaisne proceeds to refute it, by the record of his observations and experiments. He has raised seedlings from four very different varieties of

Pears, and each of the four gave rise to a considerable number of new varieties, as different from each other and from their mother, as she was from the greater part of our old varieties. "It is not only in the fruit that trees raised from the same seed differ; but also in the time of their ripening, general appearance, and in the form of their leaves. . . . Some have spines, some have none, some have slender wood, in some it is stout and coarse. Upon some of the seedlings of the old *Poire d'Angleterre* the variation went so far as to produce lobed leaves, like those of the Hawthorn or of *Pyrus Japonica.* Everything varies in the Pear-tree, even to its sap. As proof of this observe the very different success of the graft, according to the stocks employed. All the varieties and races of Pear-trees bear grafting upon a Pear-tree, that is to say, upon the wild Pear-tree; but all will not take upon the Quince, as for example the *Rance*, *Clairgeau*, *Bosc*, *Duchesse de Mars*, *&c.*" The flowers also showed very striking variations. As to size of fruit the transition is said to be perfect from the wild Pear of Blidah, which is only the size of a pea, to the *Belle d'Anguine* and *d'Amour*, which vie with a melon of middling size.

That these extreme forms belong to different species which have crossed and recrossed thousands of times with each other, producing fertile hybrids, &c., Decaisne will not allow to be probable. But it would be difficult to disprove it. He admits the crossing, but maintains that the constant fertility, after every conceivable cross, argues identity and not diversity of species.

"Does the graft, as some people maintain, alter the character of the variety?" He concludes it does not. "The notion that fruit-trees degenerate because they are propagated by grafting is an error which must be exposed. There is no single fact to prove it. Those which have been cited depend upon totally different causes, first and foremost among which are climate, unsuitable soil, and very often bad cultivation or a neglect of pruning, so common now-a-days. Our ancient pears, which a century or two ago were so justly esteemed, are now exactly the same as they ever were; they ripen at the same time and keep good just as long. . . . The pretended degeneracy of ancient races is really nothing more than one of the clever devices of the present day."

"On the other hand, can it be true, as Van Mons and many pomologists believe, that the pips of a good fruit produce wild austere fruit, and thence return to what they suppose to be the specific type? I do not hesitate to declare the contrary, and I defy any one to bring forward an example of a good fruit, whose flowers were fertilized by their own pollen, or by that of any of their own race, whose seed has produced wild fruit. . . . It may be considered certain, that all superior varieties of the Pear-tree, and I may say of all fruit-trees, if they are fertilized by themselves, produce good fruit. They may vary, and will probably do so, sometimes in one peculiarity and sometimes in another, according to the variety; but none will become wild, any more than our seedling Cantaloupe melons return to the form and flavor of the little wild melons of India, or than our Cabbages and Cauliflowers return to some one of the wild races that grow on the sea-shore. Whatever the advocates of immutability may say, the species of plants are really subject to great variation; and there is much truth in the theory which refers to the same specific type, races and varieties which, though very different in appearance, have the same morphological organization, and which, like the members of the same family, are capable of crossing with one another . . . Take any one of our races of Pear-trees, and transport it to all the regions of the globe; wherever it can exist, it will struggle to adapt itself to the situation, and after a few generations it will have given birth to new and numberless varieties. This fact, which takes place under our own eyes, in the case of every cultivated plant that is much distributed over the world, gives the key to those polymorphous species which perplex botanical classifiers, and which have only become what they are by nature herself having spread them over an immense expanse of country."

We remark, 1, that a view which we have more than once referred to, *i. e.* that variation is a result of domestication, and that there are no wild varieties (a view which essentially coincides with that of Jordan, as stated above), is sufficiently refuted by Decaisne's experiments. For one of the four Pears of his experiments, the *Sauger* is a wild variety, or nearly so, and its seedlings were not behind those of the other sorts in amount of variation. And 2, although no varieties may not have yet been shown to *degenerate* while duly cared for, under continued propagation from buds, it does not follow that they would *continue to exist* in perpetuity. It can hardly be otherwise than that the *existence of a species* is prolonged by sexual reproduction as it would not be by budding, or sexless propagation. A. G.

2. *Review of Mémoires et Souvenirs de Augustin Pyramus DeCandolle.*—The conductors of this Journal are rather pleased than otherwise when articles are reprinted from its pages. Nor can they at all object to abridgments of such articles, when the omissions are indicated in the customary manner. But in this respect they have somewhat to object to in the reprint, from our January number, of the article above-mentioned, abstracted in Dr. Seemann's Journal of Botany, without anything to mark the *lacunæ*. This, however would not be worth notice, except for the statement that the article is thus reproduced "with corrections by the author." This seems to imply an agency in the matter on the author's part, which surely it could not have been intended to assert. It may, therefore, well enough be stated, that the corrections in the author's copies sent to the Editor of the Journal of Botany, as to other correspondents, amounted, we believe, only to the change of a single letter in a misprinted word. So that there is some mistake or inadvertance in the matter, which we are not able to explain. A. G.

3. *On Welwitschia, a new genus of Gnetaceæ*, by JOSEPH HOOKER, M.D., F.R.S., etc. pp. 48, tab. 14.—This is a separate issue, in folio form, of a memoir in the current (24th) volume of the Transactions of the Linnæan Society, illustrated by fourteen superb and elaborate plates, the expense of which has been mainly defrayed by the Royal Society, from a parliamentary fund placed at its disposal for the promotion of scientific research. By the coöperation of these two learned societies, the fruits of Dr. Hooker's admirable researches are given to the scientific world in a form and manner worthy of them and of the wonderful subject. Dr. Welwitsch's discovery of this strange plant, and what was then known of it, are briefly adverted to in this Journal, vol. xxxii, p. 289, two years

ago. The present memoir was published in the spring of the present year, and has been duly noticed in the principal scientific journals. It accidentally happened that the copies addressed by the author to his correspondents in the United States were only recently received. Though late, some notice of the plant and of Dr. Hooker's investigations will still be welcome, no doubt, especially to many readers who may never see the memoir itself.

A good idea of the vegetable wonder in question is given in the following brief account of its appearance and prominent characters, drawn partly from the descriptions of its discoverer, and partly from specimens sent to England :

"The *Welwitschia* is a woody plant, said to attain a century in duration, with an obconic trunk about two feet long, of which a few inches rise above the the soil, presenting the appearance of a flat, two-lobed, depressed mass, sometimes (according to Dr. Welwitsch) attaining 14 feet in circumference (!), and looking like a round table. When full grown it is dark brown, hard, and cracked over the whole surface (much like the burnt crust of a loaf of bread); the lower portion forms a stout tap-root, buried in the soil, and branching downward at the end. From deep grooves in the circumference of the depressed mass two enormous leaves are given off, each six feet long when full-grown, one corresponding to each lobe; these are quite flat, linear, very leathery, and are split to the base into innumerable thongs that lie curling upon the surface of the soil. Its discoverer describes these same two leaves as being present from the earliest condition of the plant, and assures me that they are in fact developed from the two cotyledons of the seed, and are persistent, being replaced by no others. From the circumference of the tabular mass, above, but close to the insertion of the leaves, spring stout, dichotomously branched cymes, nearly a foot high, bearing small erect scarlet cones, which eventually become oblong, and attain the size of those of the common spruce fir. The scales of the cones are very closely imbricated, and contain, when young and still very small, solitary flowers, which in some cones are hermaphrodite (structurally but not functionally), in others female. The hermaphrodite flower consists of a perianth of four pieces, six monadelphous stamens, with globose trilocular anthers, surrounding a central ovule, the integument of which is produced into a styliform sigmoid tube, terminated by a discoid apex. The female flower consists of a solitary erect ovule, contained in a compressed utricular perianth. The mature cone is tetragonous, and contains a broadly winged fruit in each scale. Its discoverer observes that the whole plant exudes a resin, and that it is called *Tumbo* by the natives, —whence he suggests that it may bear the generic name of *Tumboa*; but this he withdrew at my suggestion, for reasons which I shall presently give. It inhabits the elevated sandy plateau near Cape Negro (lat. 15° 40′ S.) on the S. W. coast of Africa."

Welwitschia mirabilis, Hook. fil. was also detected and made known—indeed the first actual materials, with a drawing of the plant, were sent to England—by Mr. Baines from the Damara country, in lat. 24° or 25° S. about 500 miles south of Cape Negro. Mr. Baines is an artist, and his original colored sketch of a plant in fruit is reproduced on the first plate of the memoir. It appears as if five-leaved; but probably one of the two original leaves is split into two, the other into three segments. As might be inferred from the form and structure, the *Welwitschia* inhabits a dry region. Mr. Monteiro writes to Dr. Hooker :—

"— about thirty miles distant from the coast, I passed a plain about three miles across, on which this plant was growing abundantly; that is to say, I saw about thirty specimens on my line of march. The plain was perfectly dry, and bare of other vegetation than the *Welwitschia* and a little short grass. The ground was a hard quartzose schist. The *Welwitschia* was generally growing near the little ruts worn in the plain by running water during the rainy season."

And from Damara Land, Mr. Anderson writes that,—

"Rain rarely or never falls where this plant exists. [Yet the night dews are heavy, as other authorities mention.] I have crossed and recrossed Damara Land throughout its entire length and breadth, but only found the plant growing on that desperately arid flat, stretching far and wide about Walvisch Bay."

We are familiar with plants of very diverse orders of Dicotyledons and Monocotyledons which are adapted to arid regions by great restriction of surface. Here a similar plan is adopted by a Gymnosperm; for the resemblance to *Coniferæ* and *Casuarineæ* indicated by Dr. Welwitsch is shown by Dr. Hooker to import a close affinity, the author referring the plant to *Gnetaceæ* near to *Ephedra.* Its permanently abbreviated ascending axis—of which the greater part consists of the first internode, below the cotyledons,—increases in thickness but hardly in length, developes no other than the seminal pair of leaves, above which the disciform bilobate axile portion or "crown," gradually produced, bears year by year only leafless inflorescence.

Hæmanthus equally bears a pair of leaves; but these die off as the season of drought advances, when the plant is reduced to a minimum of surface in its spherical bulb,—which outspreads a new pair of leaves when the rainy season returns. But in *Welwitschia* the two leaves are permanent. Wherefore they are firm and coriaceous, and, increasing by basal growth from year to year, the older parts doubtless become inactive at length, while fresh surface below is annually renewed, under the shelter (as Dr. Hooker describes) afforded by the deep grooves which in old plants separate the growth of the hypocotyledonary stock from that of the crown above, and is filled by the tender growing bases of the leaves.

Having given a detailed generic character of *Welwitschia*, and a comparative view of the Gnetaceous genera, now three in number, Dr. Hooker proceeds to describe at length the trunk, leaves, inflorescence, flowers, fertilization, embryogeny and seed of this curious subject,—comparing it in the latter respects with *Coniferæ* and *Cycadaceæ* on the one hand, and with *Santalum* and *Loranthus* on the other, and closing with a general summary of the results.

An abstract or analysis of this most important paper is beyond our present reach or space. But we may refer to some of the special points.

The most obvious peculiarity of *Welwitschia* is, that "it appears to be the only perennial flowering plant which at no period has other vegetative organs than those proper to the embryo itself,—the main axis being represented by the radicle, which becomes a gigantic caulicle, and developes a root from its base and inflorescences from its plumulary end, and the leaves being the two cotyledons in a very highly developed and specialized condition." It is an excellent case, accordingly, if any such were still needed—for showing the nature of the radicle as stem, or ascending axis (not root,)—a view which we supposed observation had long ago demonstrated. Dr. Hooker, in a note, refers to this view as expressed by Adr. Jussieu in his *Cours Elémentaire* (which appeared in 1843 and 1844), and in Gray's Introduction to Botany (*Bot. Text Book*), 1858. But the same view is taken in all the earlier editions of the latter work; even in the first (1842) the radicle is spoken of as the first internode of the stem (p. 29, note); and probably the idea will be found distinctly expressed in works of an earlier date. Dr. Hooker, in the note referred to, assents to the proposition that "the radicle is rightly regarded as an axis" i. e. an ascending axis, "and not a root," but does not agree that it is an internode. To us, the one implies the other. Conceiving, as we do, the fundamental idea of the morphology of the phænogamous plant to be, that the ascending axis consists of a series of superposed internodes, each crowned by a leaf-bearing point or ring (the node), the first internode must needs be that which is crowned by the first leaf or pair of leaves, the cotyledons; and its whole developement confirms this view.

Dr. Hooker notes the curious fact that in *Welwitschia* flower buds are occasionally produced on the stock below the insertion of the leaves, that is, on the radicle or caulicle itself; and Dr. Masters pointed out to him analogous cases of shoots thus originating, one of which was described by Bernhardi thirty years ago. It is simply the case of adventitious buds; these might seem as likely to occur on the first internode as on any later one.

Welwitschia, having a dicotyledonous embryo, has also essentially an exogenous stem, i. e. "the vascular system is referable to the exogenous plan, but its arrangement into concentric wood wedges is very rude." But the superadded isolated and closed vascular bundles of the stock and root, and especially their losing themselves in the periphery of the stock, are endogenous analogies. So also is the strictly parallel and free venation of the leaves; yet, as there are no cross veinlets, thus favoring the splitting up of the leaf into laciniæ, this looks as much or more towards *Cycadaceæ* and broad-leaved *Coniferæ*.

The total absence of anastomosing veinlets in the leaf, each nerve representing a single and independent vascular axis, extending, in *Welwitschia*, from the axis of the trunk to the apex of the leaf, causes such leaves as these and those of *Dammara*, &c., to "resemble more closely a series of parallel uninerved leaves united by cellular tissue, than a foliar expansion of parenchyma traversed by one system of inosculating vessels, and the frequent presence of many linear cotyledons in these plants seems to favor this view, as does the mixed character of the foliage of *Podocarpus*, of which some species have uninerved and others many-nerved leaves. The numerous flower-buds along the periphery of the crown also further favor this view." That is, in *Welwitschia*, where this ingenious surmise carries a plausibility, which it does not when applied to *Podocarpus*.

The binary symmetry of *Welwitschia*, beginning with the cotyledons, is carried through the inflorescence up to the decussating pairs of bracts of the cone and the two leaflets in each whorl of the hermaphrodite perianth. But the stamens are six, at first sight a monocotyledonous analogy; yet they may be regarded three sets of two, notwithstanding their monadelphy. The flowers are diœcio-polygamous, i. e., of two sorts, one female, the other structurally hermaphrodite, but the gynæcium sterile, though well-developed, except that no embryo-sac appears. The hermaphrodite cones and their flowers accord in many respects strikingly with the male cones of *Ephedra ;* but the anthers are trilocular, which is remarkable. The simple ellipsoidal pollen is the same in both. In *Ephedra* the stamens vary from two to eight, and the column is solid, there being no rudiment of a gynæcium.

The female fruitful cones are about three inches long, and bright red when fresh.

The integument of the ovule, as in *Gnetum*, is prolonged at the summit into a style-like body, thus closely simulating a pistil; and the apex of this styliform tube, which is thin and merely erose in the fertile flower, in the structurally hermaphrodite flower is dilated into a broad papillose disk, exactly imitating a highly developed stigma—a marked instance of a highly developed organ which is functionless; for no pollen has been detected upon it, and no embryo-sac in the nucleus. Here Dr. Hooker speculates upon "the possibility of *Welwitschia* being the only known representative of an existing or extinct race of plants, in which such a stigma-like organ was really capable of performing the function of a stigma. And, when we see this organ occurring in a hermaphrodite flower, it is easy to suppose that we have in *Welwitschia* a transition in function, as well as in structure, between the gymnospermous and angiospermous Dicotyledons; and that the ideal race consisted of hermaphrodite plants, in which the office of the stigma of the carpellary leaf was performed by a stigmatic dilatation of the ovular coat itself."

This assumes that the gymnospermous theory established by Brown is correct (whatever be the nature of the cone-scales, rameal or carpellary, simple or compound), and applicable to the Gnetaceous as well as to the Coniferous type. This view, lately much questioned, Dr. Hooker maintains, and enforces, as respects *Gnetaceæ*, by very convincing and in part wholly original arguments, drawn from his own researches upon the present plant and its allies. We refer to pp. 28 to 31, which we could not readily condense, and have not room to copy. The same is to be said in regard to the resemblances or analogies in gynæcial structure between *Gnetaceæ* and *Loranthaceæ*, &c.—a subject upon which we await expectantly Prof. Oliver's investigations. Moreover, as Dr. Hooker remarks upon another page, the decisive or final comparative view of the structures in question cannot be had until the homology of the ovule itself is settled. In cases where the flower is so simplified that the nucleus of an ovule directly terminates the floriferous axis, and is surrounded by few and simple, or peculiarly specialized, investments, the discrimination of these must be difficult enough, and must ultimately depend upon the theory adopted as to the nature of the ovular coats. If these be regarded as foliar (as a rigid application of adopted morphological principles will require), then a complete transition from gymnospermy to angiospermy is probable, and may be expected to be demonstrable.

The fertilization and embryology of *Welwitschia* have been wonderfully worked out, considering the materials, by Dr. Hooker, and the two most elaborate and valuable plates of the memoir are filled with the details. Suffice it to say, that it appears that the pollen must be brought by insects to the ovule of the female flowers, at an early period, before the nucleus is covered by the ovular coat or by the perianth, and before the former has produced its styliform apex, down which it would be nearly impossible to convey the grains of pollen which were bodily found on the nucleus, with their tubes there produced. So that, notwithstanding the carpel-like form of the ovule, the impregnation is absolutely gymnospermous. As to embryo-formation also, "there is a general agreement in many most essential particulars with *Cycadaceæ* and *Coniferæ*," especially, and beyond what has already been adverted to, in "the free embryo-sac being filled with endosperm-cells previous to fertilization, the numerous secondary embryo-sacs, the position of the germinal vesicle at the base of these sacs, and in the high development of the long tortuous suspensor." There is an agreement with Angiosperms, however, in several particulars, especially in that of "the germinal vesicle giving rise to one embryo only." And it is concluded that, in special reference to *Santalum* and *Loranthus*, "*Welwitschia* presents an embryogenic process intermediate between that of Gymnosperms and Angiosperms."

And here we should not omit to mention that its wood differs from that of all known Gymnosperms in wanting the disc-bearing wood-cells!

It will be conceded that *Welwitschia* is "the most wonderful discovery, in a botanical point of view, that has been brought to light during the present century." Also, that Dr. Hooker has enjoyed (and improved) an opportunity unequalled by any botanist since that which placed the *Rafflesia* in Mr. Brown's hands. A. G.

4. *American Tea-Plant.*—A newspaper announcement states that the Tea Plant has been discovered by a Chinaman (or as some say by an Englishman formerly engaged in the tea-culture in Assam), in the United States, "covering a large area of land in the central counties of Pennsylvania," and that tea of excellent quality and various sorts, green and black, has been made for the market by a company organized for the purpose. We are told that the agent of the company exhibits in this connexion, a drawing which is recognized as representing a genuine Tea-plant.

A specimen of the prepared tea has been shown to us; by which we recognize that this American Tea-plant is the well-known *Ceanothus Americanus*, the *New Jersey Tea*, the leaves of which were used for this purpose at the beginning of the American revolution. Some one has remarked that the substituted beverage must have tried the patriotism of our great-grandmothers; but others report more favorably of its qualities. A. G.

5. *The Compass Plant.*—Riding near Chicago, August 8, 1863, I saw, for the first time, *Silphium laciniatum* growing wild. The field had once been ploughed and sown with timothy, and there was a grove a few rods to the east. Notwithstanding these unfavorable circumstances, I took a rough measurement of thirty plants, without selection, as follows: Holding a card over each plant with its edge parallel to the central line of my own shadow, I marked upon the card a short line parallel to each leaf of the plant. Measuring afterward the angle which each mark made with the edge of the card, and subtracting from each angle the azimuth of the sun for the estimated central time of observation, I obtained the following results. Only one plant, bearing four old leaves, gave an average

angle with the meridian of more than 34°. Their mean was 18° west. The remaining 29 bore 91 leaves; which made with the meridian the following angles, viz: Seven made angles greater than 35°; fifteen, angles between 35° and 20°; sixteen, angles between 20° and 8°; twenty-eight, angles between 8° and 1°; and twenty-five, angles less than 1°.

Of the 69 angles less than 20° the mean is N. 33′ E., i. e. about half a degree east of the meridian. The error of azimuth, from my want of means to determine the time accurately, may have been as much as three times this quantity. One half the leaves bear within about half a point of north, two-thirds within a point.

The magnetic declination was about 6° east. The observations were made when the sun was about on the magnetic meridian. T. Hill.

Cambridge, Mass., Sept. 6, 1863.

Zoology.—

6. *Classification of animals based on the principle of Cephalization;* by J. D. Dana.—A modification of the brief section on the elliptic method of decephalization, at page 327, is suggested in a note on page 352. The subject admits of much fuller elucidation, and the following is here presented as a substitute for the section referred to.

9. *Elliptic.*—Exhibited in the defectiveness or absence of segments or members normally pertaining to the type of the order or class containing the species, and arising from *abnormal weakness* in the general system, or in an organ. It is exhibited especially in the degradational or inferior types. The cases are—

Incomplete or deficient (1) segments, or (2) members, in either (*a*) the *anterior*, or (*b*) the *posterior* portion of the body; as in the absence of some, or all, of the teeth in Edentates; of the posterior limbs in Whales; of the abnormal appendages and posterior thoracic segments in some Schizopods or degradational Macrurans; of the antennæ, either one or both pairs, in many inferior Entomostracans; of wings in the Flea, etc.

This method of decephalization differs from the defunctionative in implying a deficiency not only of function but also of organ or member.

The incompleteness or deficiency of normal parts referred to above will be better appreciated if contrasted with deficiencies from other causes. The principal other causes are the following:

(1.) A *high degree* of cephalization or cephalic concentration in the system.—Thus in the Crab, the highest of Crustaceans, the abdomen is very small, and *elliptic* both in segments and members, because of the *high degree* of cephalic concentration; while in the Schizopods referred to above, and in the Limulus and many other inferior Crustaceans, the same deficiency comes from *weakness* of life-system or decephalization.

(2.) High development of one part of an organ, at the expense of other adjoining parts.—This principle may be said to include the preceding, since, in that, there is a high development of the anterior or cephalic portion of the structure at the expense of the posterior or circumferential. But here, there is reference to special organs rather than to the structure as a whole. Thus, in the foot of a Horse, there is an enlargement of one toe, normally the third, at the expense of the others, and this enlarged toe has the full normal strength that belongs to the foot under the Herbivore-type.

It is apparent from the facts in paragraphs (1) and (2), that there may be an *elliptic* method of *cephalization* as well as of *decephalization.* The Crab-type is a striking example of the former. The foot of the Horse, considering separately the *Horse-type*, is a case under the former rather than the latter; for, in any related species, a lessening of the disparity of the toes would be evidence of weakness and inferiority *under that type.* Yet, as compared with the higher Carnivore-type, in which the life-system has the strength to develop all the toes in their completeness and fulness of vigor, with great strength of foot, the foot of the horse is *elliptic*, and a mark of inferior cephalization. In the typical Ruminants, the complete series of teeth is indicated in an embryonic state before birth; but part of them fail of development, while the others—those specially characteristic of the type—go forward to great size and perfection. As in the foot of the Horse, there is here an enlargement of one portion at the expense of the others. And this, under the Ruminant-type, is progress toward the highest condition of the type, or *cephalization* by an elliptic method. A Ruminant in which the teeth should be all equally developed would be one of too great feebleness of system to carry the structure to its typical perfection; and such is the Eocene Anoplothere.[1] If, however, the Ruminants were referred to the Megasthene-type as represented in the Carnivores, the *deficiency* of teeth would be an example of *decephalization* by the elliptic method; for such a deficiency under the higher type of the Carnivores would be evidence of abnormal weakness.

The same principle is exemplified in Carnivores; for the size and number of the molar teeth are less the larger the canines. The Machærodus with its huge tusks and but *three* molars to either side of a jaw is a remarkable example. Again, in the Elephant, two incisors are developed into the great tusks of the upper jaw at the expense of the other incisors and canines; and jaws that look as if bearing profoundly the mark of degradation or decephalization, are hence compatible with high *cephalization* under the Herbivore-type.

It is not to be inferred that the enlargement of one part of an organ at the expense of others, is *necessarily* an indication of *general* elevation of grade. Even in the case of the foot of the Horse, the elevation implied is elevation only under the Horse-type or among Solidungulates, and not elevation above all other Herbivores.

These examples are sufficient to illustrate the contrast between the elliptic method of cephalization and of decephalization; and also the fact, that a case of the former in one relation may be one of the latter in a higher, that is, if referred to a higher group as the standard type. The cases that would come under the elliptic method of *cephalization* (as that of the Crab) have been already referred by the writer to the *concentrative*, they being a result of concentration in the life-system.

[1] "Amongst the varied forms of existing Herbivora we find certain teeth disproportionately developed, sometimes to a monstrous size; whilst other teeth are reduced to rudimental minuteness, or are wanting altogether: but the number of teeth never exceeds, in any hoofed quadruped, that displayed in the dental formula of the Anoplotherium. It is likewise most interesting to find that those species with a comparatively defective dentition, as the horned Ruminants for example, manifest transitorily, in the embryo-state, the germs of upper incisors and canines, which disappear before birth, but which were retained and functionally developed in the cloven-footed Anoplothere."—*Goodsir, British Assoc. Rep.*, 1838. *Owen's Brit. Mamm.*, 1846, 433.

(3.) That simplicity of structure which is opposed to the specialized or differentiated condition of superiority of type.—It is evident that the examples of elliptic decephalization, taking this term in its most comprehensive sense, may include the various simplifications which mark unspecialized structures of inferior types. Yet we propose to restrict the term to those examples of deficiencies which are obviously connected with degradational or hypotypic conditions under any type.

7. *On the Embryology of Astercanthion berylinus* Ag., *and a species allied to A. rubens M. T. Asteracanthion pallidus* Ag.; by A. Agassiz. 8 pp. 8vo., from the Proceedings of the American Academy of Arts and Sciences, Ap. 14, 1863.—This paper is a brief description of the embryology of the Echinoderm, above mentioned, illustrated by two excellent plates. The results, it states, are to appear in full in the fifth volume of Prof. Agassiz's Contributions to the Natural History of the United States.

8. *List of the Echinoderms sent to different Institutions in exchange for other specimens with Annotations;* by A. Agassiz. pp. 17–28, 8vo.—The annotations in this printed Catalogue are observations on characteristics of the species, or localities, references to authorities, &c.

9. *On synthetic types in Insects;* by A. S. Packard, Jr. (from the Jour. Bost. Soc. Nat. Hist., June, 1863, pp. 590–603).—Mr. Packard has here brought together much information and some new suggestions with regard to the analogies between the different tribes of Insects. We shall take occasion to quote from his memoir in another number of this Journal.

10. *Beiträge zur Kenntniss der fosillen Pferde und zu einer vergleichenden Odontographie der Hufthiere im Allgemeinen;* von Prof. L. Rütimeyer. 144 pp. 8vo, with 4 plates. Basel, 1863. (From the Verhandlungen der naturf. Ges. in Basel, vol. iii, Heft 4, 1863).—Prof. Rütimeyer, of Basel, the author of a valuable work on the ancient *Lake habitations* of Switzerland,[1] takes up in this new memoir, the subject of fossil horses and related Ungulates. His able memoir is illustrated by numerous figures, representing fossil and recent teeth of different genera and species of Horse, and also of other hoofed quadrupeds.

11. *Methods of Study in Natural History;* by L. Agassiz. 320 pp. 12mo. Boston, 1863: Ticknor & Fields.—The chapters of this interesting volume were originally delivered as oral lectures by Prof. Agassiz, and subsequently, with the exception of the last, written out in a popular form for the Atlantic Monthly. The author here presents his views on nomenclature and classification, on the nature and constancy of species as opposed to the transmutation theory, on the formation of coral reefs and the length of geological time as determined by their rate of progress, on alternate generation, and on the ovarian egg and the relations of embryology to classification.

12. *On the nomenclature of the Foraminifera;* by W. K. Parker and T. R. Jones. Part VIII, *Textularia.* 8 pp. 8vo. (Ann. and Mag. Nat. Hist. for Feb. 1863).—The characteristics, varieties of form, geographical distribution and generic synonymy of the Textularia group are the subjects of this paper.

[1] Die Fauna der Pfahlbauten in der Schweiz. Untersuchungen über die Geschichte der wilden und der Haus-Säugethiere von Mittel-Europa. 248 pp. 4to, with woodcuts and 6 plates. Basel, 1861.

VI. ASTRONOMY AND METEOROLOGY.

1. *Discovery of a new Planet, Asteroid* (79), (in a letter to the Editors from Prof. JAMES C. WATSON, Director of the Observatory, dated Ann Arbor, Oct. 9, 1863).—I have the pleasure to inform you that I discovered a new planet on the night of the 14th of September, which I have observed as follows:

Ann Arbor M. T.						R. A.			Dec.			
1863 Sept.	14d	15h	22m	56s		1h	0m	45·54	+9°	53′	36″	·2
"	14	16	24	21		1	0	44·52	9	53	24	·1
"	15	10	0	49		1	0	23·99	9	49	16	·8
"	16	9	42	43		0	59	54·76	9	44	0	·0
"	16	9	48	28		0	59	54·33	9	43	54	·5
"	19	9	8	11		0	58	16·68	9	26	13	·4
"	19	9	45	37		0	58	15·56	9	26	4	·0
"	20	14	45	1		0	57	32·34	9	18	21	·0
"	21	9	46	18		0	57	3·99	9	13	7	·6
"	22	9	48	36		0	56	25·42	9	6	14	·1
"	23	10	59	50		0	55	44·27	+8	58	54	·5

I have also observed the planet on Sept. 25, 26, 28 and 29, and on Oct. 6, but the observations are not yet reduced.

From the observations on Sept. 14, 19 and 23, I have computed the following elements, which up to the present time represent the observed places very satisfactorily:

Elements of Asteroid (79).

Epoch, 1863, Sept. 19·0, M. T. Greenwich.

$$M = 333^\circ\ 50'\ 17''\cdot8$$

$$\left.\begin{array}{l}\pi = 45\ \ 35\ \ 56\ \cdot0 \\ \Omega = 206\ \ 37\ \ 17\ \cdot4\end{array}\right\}\text{Mean Equinox 1863·0.}$$

$$i = 4\ \ 38\ \ 27\ \cdot1$$

$$\varphi = 11\ \ 19\ \ 25\ \cdot1$$

$$\log.\ a = 0\cdot388320$$

$$\log.\ \mu = 2\cdot967527$$

$$\mu = 927''\cdot955$$

These elements give the following ephemeris for Greenwich mean noon:

		R. A.			Dec.		log. Δ.
Oct.	25	0h	33m	15s	+4°	39′·9	0·01742
	26	0	32	43	4	32 ·4	0·01889
	27	0	32	13	4	25 ·1	0·02044
	28	0	31	44	4	18 ·0	0·02206
	29	0	31	17	4	11 ·1	0·02371
	30	0	30	51	4	4 ·3	0·02539
	31	0	30	27	3	57 ·8	0·02716
Nov.	1	0	30	5	3	51 ·6	0·02898
	2	0	29	45	3	45 ·6	0·03085
	3	0	29	27	3	39 ·7	0·03276
	4	0	29	11	3	34 ·2	0·03471
	5	0	28	56	3	28 ·8	0·03668
	6	0	28	44	3	23 ·7	0·03870
	7	0	28	34	3	18 ·9	0·04073
	8	0	28	27	3	14 ·3	0·04277
	9	0	28	22	3	10 ·0	0·04481
	10	0	28	20	+3	5 ·9	0·04686

The expressions for the coördinates referred to the equator are the following:

$$x = r(9{\cdot}999712)\sin(135°\ 31'\ 25''{\cdot}0 + v)$$
$$y = r(9{\cdot}974897)\sin(44\ \ 47\ \ 45\ {\cdot}5 + v)$$
$$z = r(9{\cdot}521645)\sin(51\ \ 25\ \ 55\ {\cdot}8 + v)$$

The planet shines like a star of the 10th magnitude.

2. *Observations in Brussels of the Meteors of August,* 1863; by Mr. Quetelet.—Mr. Quetelet, on the evening of Aug. 7th, saw nine shooting stars between $10^h\ 10^m$ and $11^h\ 10^m$. On the 8th in about an hour from $10^h\ 6^m$ he saw 17. On the 10th, observing alone as before, from 9^h to 10^h he saw 23, and from 10^h to 11^h, 33 meteors. He registered upon a chart the paths, and could not command more than one-third of the sky. He estimated the numbers visible for the two hours at 70 and 100. From 11^h to 12^h his son and Mr. Hooreman saw 112 and estimated the number visible at 180. On the 11th Mr. Quetelet saw 11 between $10\frac{1}{2}^h$ and 11^h, and his son and Mr. Hooreman saw 40 during the next hour.

3. *Observations by* Mr. Alex. S. Herschel *of the August Meteors, in England.*—Mr. Alex. S. Herschel, son of Sir John Herschel, in a letter to Mr. Quetelet, published in the *Bulletin* of the Royal Belgian Academy, gives the following altitudes and velocities of five meteors of the August stream observed on the evening of Aug. 10th, 1863. The second column of the table below gives the hour of observation, the third the duration of flight in seconds, the fourth the altitude at first appearance, the fifth the altitude at disappearance, and the sixth the velocity. The unit of altitude and velocity is the English mile.

Altitudes and velocities of shooting stars observed Aug. 10th, 1863, *in England.*

No.	Time.	Duration.	1st Alt.	2d Alt.	Velocity.
1.	$9^h\ 23^m$	3^s	70	58	35
2.	$36\frac{1}{2}^m$	$1^s{\cdot}7$	114	73	36
3.	$10^h\ 7\frac{1}{2}^m$	$1^s{\cdot}2$	131	66	75
4.	11^m	$1^s{\cdot}4$	105	52	41
5.	19^m		79	58	38

The altitude and velocity of No. 3 Mr. Herschel considers as too great. The path and duration of No. 5 were very carefully noted, and he thinks the velocity of 38 miles is very near the truth. Two flights, No. 1, and No. 4, were observed at three places.

These velocities imply a less inclination of the August ring (motion retrograde) than that given in this Journal, 2d ser., vol. xxxii, p. 448.

Mr. Herschel gives also the altitudes of four other meteors as follows:

Aug. 8th,	$10^h\ 58^m$	1st alt. 153 miles,	2d alt. 61 miles.
" 9th,	$12^h\ 5^m$	" "	" 45 "
" 10th,	$10^h\ 1^m$	" 52 "	" 25 "
" 11th,	$12^h\ 10\frac{1}{4}^m$	" 122 "	" 86 "

The second and fourth were brilliant fireballs from which explosions were heard.

4. *Observations on the August Meteors, by* Dr. E. Heis, *at Münster.*—At Münster this year, the following numbers were seen, from Aug. 8th to Aug. 14th, by Prof. E. Heis and eighteen assistants. Most of the paths were drawn on charts.

Number of shooting stars seen at Münster from Aug. 8th to Aug. 14th, 1863.

Hour.	8th.	9th.	10th.	11th.	12th.	13th.	14th.	Total.
From 9^h to 10^h,	26	41	93	24	45	33	18	280
10^h to 11^h,	67	57	144	90	54	44	29	485
11^h to 12^h,	58	61	166	98	69	44		496
12^h to 13^h,			158					158
13^h to $13\frac{1}{4}^h$,			39					39
Total,	151	159	600	212	168	121	47	1458
Per hour from 9^h to 12^h,	50	53	134	71	56	40		

From 9^h to 10^h on the 11th the sky was mostly covered by clouds. The observations were made entirely in the open air. The heavens were divided among the several observers. Each observer drew on the chart the path, and a secretary noted the time (to the second), the magnitude, and other circumstances. On the 10th the stars followed each other so fast that the smaller ones, of the third and fourth magnitudes, could not be recorded. No star was counted twice.

The brightness and duration of the trains of the meteors on the 10th were remarkable. With the naked eye trains were noticed which lasted 7, 10, 14 and even 43 seconds. With the comet seeker one train was seen 55, another 60, and a third 168 seconds.

Corresponding observations were made at places near Münster, which will furnish the data for computing altitudes.—Condensed from *Heis' Wochenschrift of Aug.* 19*th and Aug.* 26*th.*

5. *Die grosse Feuerkugel, welche am Abende des* 4. *März*, 1863, *in Holland, Deutschland, Belgien und England geschen worden ist*, von Dr. Eduard Heis. Nebst einer Karte. 8°, pp. 56. Halle, 1863. Abdnuck aus der Wochenschrift, &c.—This is a very full account of a detonating meteoric fireball which was visible throughout a region 500 miles in diameter. Dr. Heis obtained reports from over 100 different places, and his summary of the various accounts is well arranged. The meteor was first seen at an altitude of 88 English miles over the North sea, and it disappeared with a loud explosion over the south part of Holland at an altitude of 17 miles. The course was nearly south, was inclined 22° to the horizon, and was 187 miles long. The duration of flight was estimated at 4 or 5 seconds. The velocity was considered therefore as 41½ miles, which gives a hyperbolic orbit around the sun for its former path.

6. *Die Meteoriten, ihre Geschichte, mineralogische und chemische Beschaffenheit*, von Dr. Otto Buchner. 8vo. pp. 202. Leipzig, 1863.—This work attempts to give a brief historical sketch of all described meteorites, including what is known regarding their mineralogical and chemical characters, and also indicates in what museums or collections the specimens are preserved. It is divided into four sections. Part I treats of *Stony Meteorites*, whose time of fall is known; these number 153 and are arranged chronologically, commencing with the *Ensissheim* stone, which fell Nov. 16th, 1492, and ending with that of *Meno*, whose fall was observed Oct. 7th, 1862. II. *Stony Meteorites*—time of fall unknown—nine in number, arranged in the order of their discovery. III. Including those intermediate in character between the stony and iron meteorites, or the *stone-and-iron* meteorites. These number 10, and are arranged in the order of their discovery. IV. *Iron Meteorites*, eighty-three in number,

arranged in the order of their discovery. The date of fall of only two of these is with certainty known, viz: *Agram*, May 26th, 1751, and *Braunau*, July 14th, 1847. It may be questioned whether the iron from Charlotte, Dickson Co., Tenn., fell as stated on July 31st (or Aug. 1st), 1835, as the evidence is far from conclusive that the mass of iron found had any connection with the meteoric explosion which took place at that time.

The four classes make in all a sum total of 255 meteorites. Of these we notice over 60 are from the United States, among them are perhaps included a few that when more thoroughly investigated will prove to have no claim to meteoric origin.

The book also contains a catalogue of the various European and American collections of meteorites, and the number of localities represented in each cabinet. Among the public collections containing specimens from more than 50 localities are the museums of Vienna (194), London (British Museum, 190), Göttingen (125), Paris (Garden of Plants, 53). The largest private collections are those of R. P. Greg, Manchester, Eng., numbering 191—Reichenbach, Vienna, 176—C. U. Shepard, Amherst College, 151—Nevill, London, 101—Auerbach, Moscow, 76—Neumann, Prague, 61—J. Lawrence Smith, Louisville, 60.

This work is exceedingly valuable to all who are occupied in the study of meteorites. In most cases it gives accurate references to original authorities, and full citations of the literature of the subject. We have noticed some minor errors in the statements made, but it is almost impossible to bring together such a mass of material without committing an occasional oversight. The author deserves the thanks of all interested in the subject for arranging in such a compact and convenient form the facts which heretofore were only to be found by a laborious search through hundreds of volumes of Journals and Proceedings of learned Societies. Dr. Buchner proposes, as time shall demand, to publish a Supplement to this book, and begs that those interested will communicate to him information regarding private or public meteoric cabinets, and corrections or omissions noticed in the present work. His address is, Dr. Otto Buchner, Professor in the University of Giessen, Darmstadt. G. J. B.

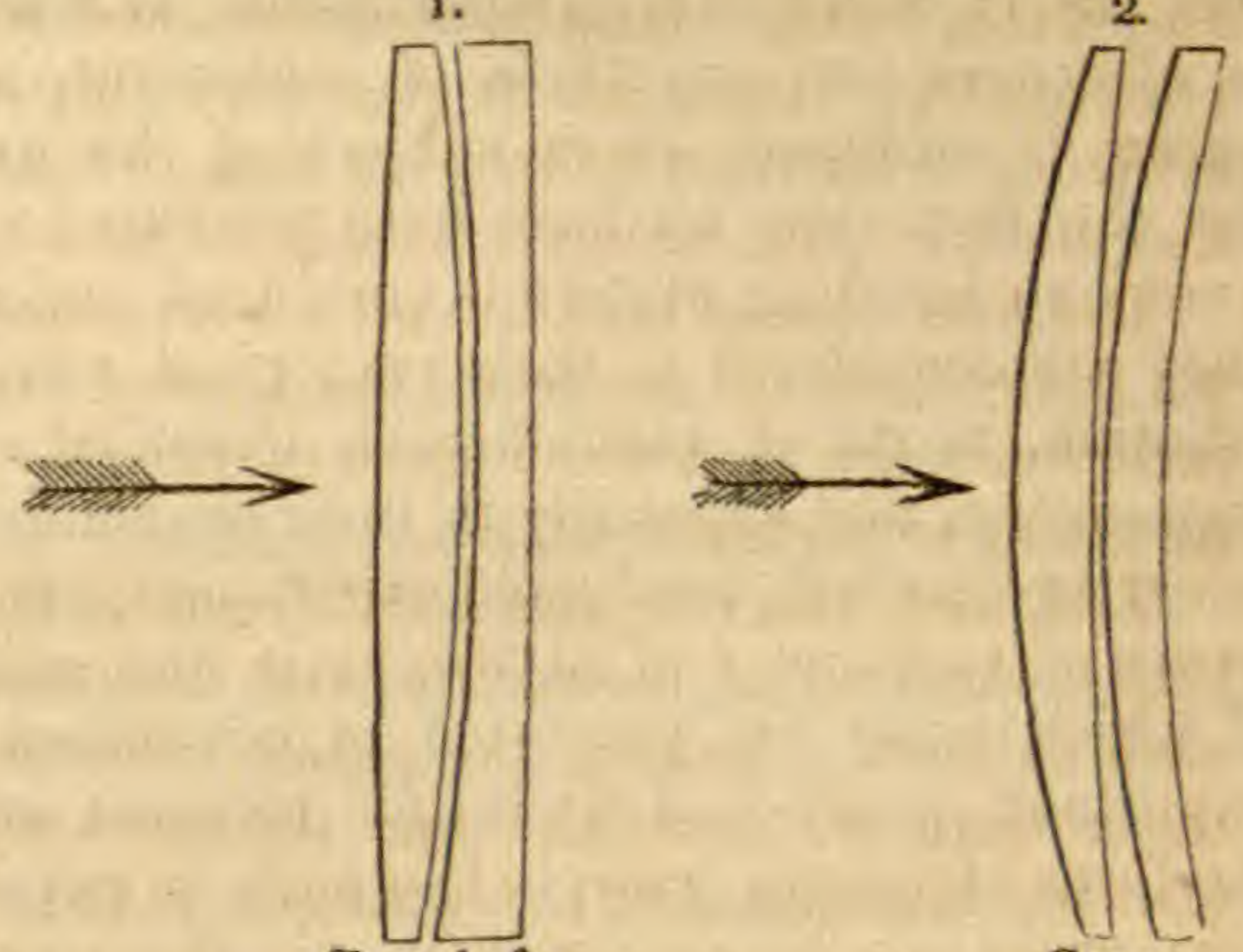
7. *Shooting Stars of November.*—The Committee of the Conn. Acad. of Arts and Sciences on Periodical Meteors have arranged for a watch on the nights and mornings near the 13th of the coming November. A special watch for determining parallax will be kept (weather permitting) on the night of the 13th and morning of the 14th. The Committee, in like manner as heretofore, invite observers in other places to communicate their observations. They may be sent either to Prof. H. A. Newton or to the undersigned. ALEX. C. TWINING, *Ch'n.*

New Haven, Oct. 27th, 1863.

VII. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *New Achromatic Object-glass.*—A new combination of curves has recently been applied by Steinheil of Munich, in the construction of the achromatic object-glass. It was originally proposed by Gauss above forty years ago, in a communication to the "*Zeitschrift für Astronomie,*" vol. iv, 1817. The curves are characterized by their great depth and abruptness, having

in some cases radii of less than *one-tenth* of the focal length. Each lens is a meniscus, whereas the practice has hitherto been to make both the surfaces of the crown lens convex, with more or less variation in the proportion of curvature, always, however, retaining the same general character of convexity. The lenses of the new system are shown in the following figures in comparison with the Fraunhofer lens.



It is perhaps not unnatural that opticians should have hesitated in giving their confidence to so decided a departure from established usage, especially as there may have been reason for apprehending practical difficulties in working surfaces to correspond with the new curves. Yet these difficulties have certainly now been overcome, at least for lenses of moderate dimensions, and it is understood that the Messrs. Steinheil are prepared to apply the system to object-glasses of the largest size for which suitable material can be procured.

The chief advantage proposed to be gained is the more complete removal of the indistinctness caused by the dispersion of the rays of different colors, without sensibly increasing the influence of spherical aberration even when the ratio of the aperture to the focal length is considerably increased above its usual proportion. In the opinion of competent judges, the trials which have thus far been made of telescopes of the new construction have exhibited a decided superiority as respects clearness of definition with large angles of aperture, thus confirming in the most satisfactory manner the correctness of the principles upon which Gauss has based his theory. Steinheil's notices of the new object glasses are in the *Sitzungsberichte der k. bayer Akad. der Wiss., München*, 1860, Heft. ii, and v.

Book Notices.—

1. *Storer's Dictionary of the Solubilities of Chemical Substances.* Part II, pp. 233–456.—This part brings the subject down to near the close of the oxyds, ending with Protoxyd of tin. The work is done in the same style of conscientious fidelity and with the same evidence of erudition which characterized the first part. Confirming all we had occasion to say on the appearance of Part I ([2], xxxv, 303), we congratulate the author upon the prospect of completing his "First Outlines" with another half year, a work which, however irksome in its performance, is a most substantial contribution to science and an enduring monument of the author's industry and learning.

2. *Practical Treatise on Limes, Hydraulic Cements and Mortars*, containing reports of numerous experiments conducted in New York City, 1858 to 1861; by Quincy A. Gilmore, A.M., Brig. General U. S. Volunteers, and Major U. S. Corps of Engineers. New York: D. Van Nos-

trand, 192 Broadway, 1863. 8vo. pp. 333.—General Gilmore's treatise is number nine of a series of "Papers on Practical Engineering" issued from the U. S. Engineer Department. It is admirable for its lucid method, its clearness and exactness of statement, and the amount of original matter it embraces on a subject of the greatest practical importance. For the first time we have here presented a full account of the various American building materials embraced under the three heads considered. They are considered in their geological relations, their geographical distribution, mode of manufacture, chemical constitution before and after preparation, and especially in their economical value. This last has been tested by carefully conducted experiments upon the tenacity, hardness and power of endurance of atmospheric and subaqueous changes, the results of which trials are presented in condensed tables. Many of the more important qualities of hydraulic cements are presented by diagrams in the form of curves projected in such a manner as to present to the eye the comparison between various samples, more prominently than a written description could do. The volume is prepared in the best mechanical style. The pressing public duties of the distinguished author before Charleston have prevented his careful revision of the sheets, although the typography is remarkably correct. A few slips of this sort will be easily corrected in a second edition.

OBITUARY.

STILLMAN MASTERMAN.—It is with pain that we record the early decease of our contributor Stillman Masterman, of Weld, (Maine,) who died of pulmonary consumption in that town on the 19th July, at the age of 32 years.

From an interesting and touching account of his life written by himself during his last moments, and with a perusal of which we have been favored, we learn the prominent incidents of his life. It has been simply a new version of the old, yet always new, story. It has shown a pure and aspiring spirit, athirst for knowledge, and yearning to take part in the enlargement of its bounds,—a mind formed for thorough investigation, and exalted above all local discouragements and the sneers of ignorance,—an enthusiasm, rising almost to devotion, for Nature in her grandest and most majestic manifestations,—a character full of all delicacy, modesty, and native refinement; but with all these gifts, which, if accompanied by a sturdier physical frame and aided by larger advantages in youth, would have surely led to high eminence in science, restricted by circumstances over which he had no control until it was too late.

Such an earnest, truthful, follower of science, Masterman truly was. None who met him could fail to note the almost feminine delicacy of his moral nature, and the careful observer soon remarked that the reserve of his manner sprang from modesty and not from coldness.

He was born in Weld, a village in Franklin Co., Maine, just at the base of Mt. Blue,—on the 28th of January, 1831. His first attendance at the district school was at a rather later age than usual, and he has vividly described the sensations which were awakened in him, when, in his eleventh year, an elementary geography was first put into his hands, and for the first time he learned the form of the earth and the primary facts of physical geography. From this time on, he seized and drained every old book which he could find containing information on any branch of physical science, and laid aside such sums of money as came into his possession until he had accumulated the means of purchasing new books on these subjects. These he studied with ardor. "Frequently," he writes, "I carried books into the field when I went out to labor, to peruse while resting from toil;—some shady tree serving as my school room, and a few moments of study serving to give me a subject for frequent rehearsals during an hour's toil."

At the age of twenty, he had managed to collect a number of scientific and classical works and had thus become familiar with the elements of physics and chemistry,—making such apparatus as the materials at hand would permit, grinding and polishing small lenses and constructing prisms for experiments on fluid media. He had also taught himself Latin enough to read with some ease, and had become familiar with algebra, geometry and trigonometry. At this period he went to the newly opened territory of Minnesota to seek his fortune in the far West; but after a little more than a year he returned to his native place, with health much impaired by the influence of that climate.

His first publication of which I am aware was a paper entitled, "Observations on Thunder and Lightning" printed in the Appendix to the Tenth (1855) Annual Report of the Regents of the Smithsonian Institution. In this he gave an interesting series of results of observations of sixteen thunder-storms during the years 1850–4 at Weld, and at Stillwater (Minn.) For each of these storms he had noted the character and the intervals of phase for the different thunder-peals,—and to these observations added the record of other curious allied phenomena. The summary shows the average length of the thunder-peal to have been $21^{s}\cdot85$ in the 148 observed instances when preceded by visible lightning, and $26^{s}\cdot60$ in the 156 cases when not so preceded. The minimum interval which he had recorded between the flash and report was 1^{s}; the maximum, 56^{s}. A very full account is given of a severe thunder-storm, 1854, June 15; and an authentic instance is afforded of the occurrence of thunder in a cloudless sky,—which he succeeded in tracing to its origin in a storm below the eastern horizon.

Mr. Masterman subsequently became a frequent contributor to this and other scientific journals. Without instruments, he devoted himself earnestly and faithfully to the observation of such celestial phenomena as could be sufficiently noted by the naked eye, and his observations of maxima and minima of variable stars as well as of the zodiacal light, auroras, meteors, the brilliancy of planets, &c., &c., are surpassed in accuracy by none made under similar circumstances. Among his writings are the following,—the list being, however, probably incomplete.

Smithsonian Reports, 1855, pp. 265–282; 1857, 323–332.

Amer. Jour. of Science, [2], xxx, 155; xxxv, 149, 50; xxxvi 143–5.

Astron. Journal, v, 31, 37, 105, 140, 191; vi, 38, 44, 47, 83, 85, 96, 183, 187.

During the last two years, until his failing health compelled him to desist from all labor, Mr. Masterman was engaged upon the reduction of observations made at the Washington Observatory during the fifteen years previous to the defection of the late Superintendent, and subsequently found unreduced. In this, as in all his other labors, his efforts were marked by ability and success.

He died, as he had lived, a Christian and a philosopher, resigned to the will of God, and in full peace of mind. B. A. G.

MAJOR EDWARD B. HUNT.—The death of Major Hunt of the U. S. Topographical Engineers, by an unhappy accident while in discharge of his duties, has occasioned universal regret and sadness wherever this gifted and excellent officer was known. To science his loss is great. In the very prime of life and power, he had already added many important contributions to our stock of physical knowledge, and at the time of his death had been engaged for many months in elaborating his novel ideas upon the subject of Molecular Physics, some outlines of which he had already presented at one of the Sessions of the Connecticut Academy of Arts and Sciences in the Spring of this year.

Major Hunt was appointed a cadet in the U. S. Military Academy at West Point, from New York, in 1841, and was graduated in July, 1845, as Second Lieutenant of Engineers. He discharged the duties of Assistant Professor of Engineering at the Military Academy from 1846 to 1849. It was during this period that he first became a contributor to the pages of this Journal, his earliest paper "On the dispersion of light," appearing in volume vii, p. 364, and from that time he was a frequent contributor on various physical subjects, his papers being always marked by originality and power of generalization. The volumes of Proceedings of the American Association also contain numerous papers from his pen. One of his latest productions, *Union Foundations*, is an attempt to show the physical reasons for American national unity, a very readable and ingenious paper, the product of a glowing patriotism. We should add to these his proper professional papers on Engineering topics.

For some two years past Major Hunt had been engaged under a special commission in perfecting a submarine battery of his own invention, the details of which are very properly kept secret. He had just completed this apparatus and perfected his system, and on the 1st of October was engaged in making a final experiment on board a vessel some five miles from Brooklyn Navy Yard. The gun (a breech loader) had been fired in the submerged cell, when, from an unwonted escape of smoke in the vessel, Major Hunt who was on deck with his men, was induced to go below to ascertain what the difficulty was, and there was suffocated by the irrespirable gases; he succeeded in partly ascending to the companion way, and extended a hand as a signal for relief. An attendant seized the hand; but at the same instant exhaustion caused him to fall into the hold where he lay with his face in a pool of water. One of the men descended instantly to his relief, but succeeded only in raising his head out of the water, when in turn overcome by the foul gases, he was compelled to seek safety by escape from the apartment. Another man partially succeeded in securing a rope to the fallen officer, but, sad to relate, in the effort to raise him the fastening gave way, and he fell heavily a second time, producing, as is supposed, concussion of the brain. He was not extricated until, by throwing down water, the atmosphere was so far renewed as to render it possible to go into the hold, and it was some hours before surgical aid could be obtained, owing to their distance from shore, when it was alas! too late. He died soon afterwards, and, as a soldier should, in the service of his country, not, indeed, on the battlefield, but with his armor on, and while engaged in perfecting a most important application of scientific principles to defensive war.

His funeral was celebrated with military honors at West Point Military Academy on Monday, October 5th, where he sleeps in the Military Cemetery amid the honored remains of many companions-in-arms, who have fallen on various fields in their country's service, or while engaged in the more peaceful duties of their profession.

Major Hunt was about 40 years of age at the time of his death. He leaves a widow and one son. A fuller notice of his life and labors will appear in a future issue of this Journal. The Connecticut Academy sent Prof. Alex. C. Twining as a delegate to represent them and the men of science in Connecticut at the funeral, and from the report of this delegation, submitted at the last session of the Academy, we have drawn some of the details of Major Hunt's tragical death.

Prof. Eilhard Mitscherlich.—Prof. Mitscherlich has recently died at Berlin at the age of sixty-nine. He had long been known as one of the ablest philosophical chemists of the day, and the estimation in which he was held was exemplified by the numbers who attended his classes in the University of Berlin, and the Friedrich-Wilhelm's-Institut in that city. The mere titles of his writings would occupy nearly two columns of this journal; they embrace a wide range in chemical science, and may be found in the publications of the Academy of Sciences of Berlin, of which he was a member, and in German periodicals. Besides these, he was the author of a 'Lehrbuch der Chemie,' in two volumes, which has passed through two editions, and has been translated into French. Dr. Mitscherlich was elected a Foreign Member of the Royal Society in 1828; and in 1829 one of the Royal Medals was awarded to him for his 'Discoveries relating to the Laws of Crystallization and the Properties of Crystals.' It is, perhaps, by his researches into the phenomena of dimorphism that he will be best remembered.—*Athenæum*, No. 1876, p. 470.

Proc. of the Am. Acad. of Arts and Sciences, (continued from vol. xxxv) Vol. V, from May, 1860 to May, 1862.—314, Notes upon a portion of Dr. Seeman's recent collection of dried Plants gathered in the Feejee Islands; *Asa Gray.*—321, Characters of new or obscure species of Plants of Monopetalous Orders in the Collection of the United States South Pacific Exploring Expedition under Capt. Charles Wilkes, U.S.N., with occasional remarks, &c.; *Asa Gray.*—353, Abstract of a Memoir upon the Attraction of Saturn's Ring; *Prof. Peirce.*—355, On the double salts of cyanid of Mercury; *Wm. P. Dexter.*—359, Remarks upon the recent determinations of the Atomic Weight of Antimony; *W. P. Dexter.*—370, Criticism on a recent work by Capt. Rodman, U.S.A., on improvements in heavy ordnance; *Prof. Treadwell.*—379, Upon the System of Saturn; *Prof. Peirce.*—383, Observations on North American and other Lichens; *Edward Tuckerman, A.M.* (continued from vol. iv, p. 407.)

Proceedings of the Academy of Natural Sciences of Philadelphia, from January to July, 1863.—2, Descriptions of Fossils from the Yellow Sandstone lying beneath the "Burlington Limestone," at Burlington, Iowa; *Alexander Winchell.*—25, Catalogue of the North American Sciænoid Fishes; *Theodore Gill.*—33, Systematic arrangement of the Mollusks of the Family Viviparidæ and others, inhabiting the United States; *Theodore Gill.*—40, Additional Remarks on the North American Ægiothi; *Elliott Coues, A.M., M.D.*—43, On Trachycephalus, Scaphiopus and other American Batrachia; *E. D. Cope.*—55, Enumeration of the Species of

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