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ERRATA.

Page 135, line 6, for *southwest* read *southeast.*
" 322, " 1, " *sleat* " *slate.*

THE

AMERICAN

JOURNAL OF SCIENCE, &c.

Art. I.—*Notice of some Works, recently published, on the Nomenclature of Zoology;* by Augustus A. Gould, M. D.

Report of a Committee (of the British Association) appointed "to consider the rules by which the Nomenclature of Zoology may be established on a uniform and permanent basis." pp. 17, 8vo., Lond. 1842.

Nomenclator Zoologicus, continens Nomina Systematica Generum Animalium tam viventium quam fossilium, etc.; auctore L. Agassiz. 4to. Soloduri, 1842.

The British Association for the Advancement of Science has undertaken one task, for which it will receive the hearty thanks of zoologists. It has undertaken to interpose the weight of its authority in arrest of the growing abuses in nomenclature, and of the injustice which some zoologists have allowed themselves to practice towards their predecessors. None need this legislation, and none will have more cause to be grateful for it, than American zoologists. We have now a host of naturalists rising up, none of them having made any great advances in science, conscious that in this new country they must be surrounded with undescribed objects, and each eager to attach his name to as many species as he may, as though this would indicate the measure of his attainments. From an impatience at investigation, and from having no large libraries or standard collections of objects, by reference to which doubts might be readily solved, there has already arisen among us such a burden of synonyms as to be quite perplexing; and it is even now difficult to settle the claims of priority among the various names so recently imposed.

Our best zoologists have been awake to the necessity of having among themselves some laws, either statute or upon honor, which shall ensure respect and justice to each other's labors, and render the nomenclature of our zoology such as shall bear the test of enlightened criticism. Not a little correspondence has been carried on, among the leading scientific men in the different cities, as to how this desirable object might be best secured. As one effective means, it has been conceded to those who have been engaged upon monographs, that they should settle the synonymy of the objects coming within their province, and that others should abide by their conclusions, unless they were *manifestly* wrong, the writer being allowed the benefit of all *suspicions.* It was deemed to be no difficult thing to establish a code of honor among ourselves; but then the thought arose, of what avail will all this be to us, if the great masters in zoology across the water, shall choose to trample upon us as they have hitherto done? It is indeed discouraging, after having, by a tedious and thorough investigation, determined the novelty of a species, to find, in some subsequent transatlantic journal, the same thing described under a different name, and with such authority as to give the prior describer a very slight chance of regaining his own prior name. With this exercise of the right of the strongest, Americans are familiar. It must be acknowledged that, in very many instances, there is an adequate excuse for this, from the very obscure channels through which the original descriptions have been conveyed to the public, and the very limited circulation such works have had. But this is not always the case; and now, there are scientific publications among us which no naturalist, whether native or foreign, should neglect to consult. Indeed, such works as the Journal of the Academy of Natural Sciences at Philadelphia, the Boston Journal of Natural History, not omitting this Journal, have become as indispensable to the zoological writer who would keep up with the level of science, as the Annals and Magazine of Natural History, or the Annales d'Histoire Naturelle, or Revue Zoologique.

It is but too apparent, however, that courtesy and justice are not meted out where the plea above allowed cannot be made. If we look at the French, for instance, we find by their writings that most of them are, or affect to be, as ignorant of what their neighbors the English are doing in science, as though such a people did not exist. Very seldom do we find a French writer referring to English books, splendid and accessible as they are, though honorable exceptions might be named. We have been sometimes disposed to think that the distinguished professors in Paris, being placed at the head of the magnificent collections and institutions of the French capital, had come to think that nothing in science could be known, which they did not know; that it would therefore be superfluous for them to spend time in culling over the works of others; and finally, that whatever presented itself as new to them, must be actually new, of course.

It was suggested a year or two since, that a paper should be drawn up and signed by the principal zoologists in America, requesting the British Association to take the subject of the laws of nomenclature into consideration, and propose a series of rules for general use. It was thought that rules emanating from such a source, "would be invested with an authority which no individual zoologist, however eminent, could confer on them," sufficient indeed to ensure their observance. The Association has anticipated our wishes, and has proposed a series of rules covering the whole ground of difficulty, just and honorable in their character, and with which no original naturalist author will find much reason to complain. They are drawn up in so concise a style, that we believe a republication of them, in this connection, will be judicious. They ought to be at once disseminated throughout the scientific public, to be duly reflected upon, and modifications suggested, before the code is finally enacted; for the committee, in the caption of their report, invoke us

—"si quid novisti rectius istis,
Candidus imperti; si non, his utere mecum."

The subject is divided into two parts; the first, of *rules for the rectification of the present nomenclature*, and the second for *its improvement in future.* The law of priority is laid down as the only effectual and just one, as a basis of procedure, and gives rise to the first and fundamental maxim:

1. The name given by the founder of a group, or the describer of a species, should be permanently retained, to the exclusion of all subsequent synonyms, (with the exceptions about to be noticed.)

2. The binomial nomenclature having originated with Linnæus, the law of priority, in respect of that nomenclature, is not to extend to the writings of antecedent authors.

3. A generic name, when once established, should never be cancelled in any subsequent subdivision of the group, but retained in a restricted sense, for one of the constituent portions.

4. The generic name should always be retained for that portion of the original genus which was considered typical by the author.

5. When the evidence as to the original type of a genus is not perfectly clear and indisputable, then the person who first subdivides the genus, may affix the original name to any portion of it, at his discretion, and no later author has a right to transfer that name to any other part of the original genus.

6. When two authors define and name the same genus, *both making it of exactly the same extent*, the latter name should be cancelled *in toto*, and not retained in a modified sense.

7. Provided, however, that if these authors select their respective types from different sections of the genus, and these sections be afterwards raised into genera, then both these names may be retained in a restricted sense, for the new genera respectively.

8. If a later name be so defined as to be equal in extent to two or more previously published genera, it must be cancelled *in toto*.

9. In compounding a genus out of several smaller ones, the earliest of them, if otherwise unobjectionable, should be selected, and its former generic name be extended over the new genus so compounded.

10. A name should be changed, which has before been proposed for some other genus in zoology or botany, or for some other species in the same genus, when still retained for such genus or species.

11. A name may be changed, when it implies a false proposition, which is likely to propagate important errors, e. g. *Picus cafer*, for a Mexican bird.

12. A name which has never been clearly defined in some published work, should be changed for the earliest name by which the object shall have been so defined.

13. A new specific name must be given to a species, when its old name has been adopted for a genus which includes that species.

14. In writing zoological names, the rules of Latin orthography must be adhered to.

On the first nine propositions we have no comments to make. Section 10, seems to us somewhat questionable. The different branches of natural history, botany and zoology, and indeed the different departments of zoology, have now become so extensive, and are pursued by students so much to the exclusion of each other, that if a genus in one department bears a name in common with a genus in some other department, the student is very unlikely to meet with it; or if he does, he meets it under such circumstances as to cause him no embarrassment. While, therefore, it is manifestly improper that widely different objects should bear the same name, we are rather disposed to regard this rule as of prospective, rather than of retrospective, application.

Section 11, seems to us liable to still stronger objections. Sad indeed would be the havoc in the nomenclature of American objects, if all the species named *Virginicus* and *Canadensis*, were to receive new names because they are likely to propagate important errors, as they frequently would at the present day. And would it not be as well to allow names of this character, already imposed, to remain, until they become so current that they may be retained with as much propriety as *Caprimulgus*, *Monoculus*, &c., which it is conceded may remain unchanged. It is but proper to say, however, that the committee propose that this rule should be "applied only to extreme cases, and with great caution."

The twelfth proposition seems to us the most important of all, after the fundamental one. It is indeed the very gist of the matter; the point which, if properly maintained, will command all the others. Until lately, the right of priority has been claimed where a man could but show, that, at some anterior period, he had given a name to a specimen in his cabinet, or had read a paper upon the object, and perhaps circulated specimens among his friends. The consequence has been, a superficial acquaintance with the works of naturalists, and an indifference to publication. It was much easier for a man to sit down and attach a ticket to every object in his cabinet which his ignorance suggested *might* be new, and await his chance of claiming his names for such of them as some patient and thorough student should prove to be actually new, than to undertake the task of conning all the published works in which it were likely to find such objects noticed. In this country, we have indeed, from the destitution of books on natural history, been compelled, per force, to risk something, or do nothing. But this should have rendered us doubly cautious in imposing names, and ever ready to retract them when they are proved to be but synonyms, rather than be tempted by the idea, that no one is so likely to be acquainted with the objects at our door as we ourselves, especially when we happen to live in a district which has sustained no naturalist before us. The actual fact is, that as a general thing, the natural objects peculiar to this country have been better known and better described abroad than at home. Certainly we may say this, if we except the last twenty years. Nor need we think that our territory has never been explored. There are collectors constantly employed in this country by foreign naturalists, who, in a quiet way, send across the water immense stores of all kinds of natural objects; and one is surprised when he sees the flood of such objects, collected at our doors and without our knowledge, in the public and private collections abroad.

But things are now come to a different pass. A stop will now be put to all baseless aspirations for notoriety by attaching *nobis* to the names of species, even when new, though not adequately substantiated; and more especially, by appending it to species created by others, in consequence of removing them to other genera.

Two things are now insisted upon in order to give authenticity to a genus or species, viz. *perspicuous definition* and *publication.* For want of the first requisite, some of our most accomplished naturalists have forfeited their claims to the adoption of names given by them. I need only mention the name of the eccentric but learned Rafinesque, to convey an idea of what I mean, to American zoologists. Some of the earlier descriptions of the lamented Say, too, are so brief and indiscriminating, that it has been impossible, without figures or authentic specimens, to identify the objects intended. A rule which we have somewhere seen, that a writer should always describe an object just as if he expected something almost exactly like it would be found next day, would be all that is necessary to ensure a satisfactory compliance with the first requisition. Nor would we be understood to say that all the works of such men are to be forfeited, because, in some instances, they have failed to give diagnostic characters sufficiently perspicuous in view of subsequent discoveries. In regard to the species instituted by Mr. Say, such has been the almost uniform respect of American naturalists for him, that they have striven to perpetuate all his specific names; and where the object intended could not be indisputably determined, they have conventionally fixed upon some species which should bear the name proposed by him. Neither would we justify the wholesale rejection of M. Rafinesque's names, which some have advocated; because it is certain, that many of his species may be satisfactorily made out, and these, beyond all question, should be adopted.

The other requisite is *publication.* And now the question arises, what shall be considered publication. In the words of the committee, "to constitute *publication*, nothing short of the insertion of the above particulars (the essential characters) *in a printed book*, can be held sufficient." The French Academy of Sciences has also decided that nothing can constitute *publication*, but the *rendering one's labors public, through the press.** These two authorities are the highest to which we could possibly have recourse, and their dicta ought to be conclusive on this point.

The definition of the Academy, it will be perceived, is broader than that of the British Association, inasmuch as the former merely requires that a definition should be given to the public *in print*, while the latter requires that it should be given in a *printed book*. We are ready to adopt the most rigid of these requisitions. When descriptions are published, as many of Mr. Say's were, in such a paper as the New Harmony Disseminator, it could not be expected that another naturalist, who might publish descriptions of the same objects, in some widely current scientific work, justifiably ignorant of his predecessor's labors, should forfeit his claim to the names imposed by him. It certainly cannot be expected, that every fugitive newspaper or ephemeral literary periodical, is to be ransacked, before a man may be permitted to name an object. In the case of Mr. Say, however, thanks to the assiduity of his friends, his fugitive publications have been collected, embodied, and given to the public, in books which cannot be set aside. At the present day, every facility which can be asked, is given to authors, for bringing their discoveries before the public as soon as they please, in such a manner as to secure all their rights. It is the custom to print, at short intervals, works in which the essential characters of objects may be given, in anticipation of figures and more ex-

* "Ainsi il est bien établi par l'Académie des Sciences, que les communications faites par MM. Le Guillou, &c., ne sauraient constituer une *publication*, et que leurs travaux resteront inédits, à l'état de manuscrit, jusqu'à ce qu'ils *aient été rendus publics, par la voie de l'impression.*"—Revue Zoologique, 1841, p. 331.

tended descriptions; thus giving date and publicity to a discovery, and allowing ample time for a more satisfactory development of it. Such are the "Annals and Magazine of Natural History," and the "Zoological Proceedings," in London; the "Revue Zoologique," in Paris; and the "Proceedings" of the American Philosophical Society, of the Academy of Natural Sciences at Philadelphia; and of the Boston Society of Natural History, in this country.

It will be perceived that this rule disallows any authority to manuscript names, whether merely attached to specimens in a museum, or even when descriptions are accurately written out in full. In the words of the report, "many birds in the Paris and other continental museums, shells in the British Museum, and fossils in the Scarborough and other public collections, have received MS. names, which will be of no authority until they are published. Nor can any unpublished descriptions, however exact, claim any right of priority till published, and then only from the date of their publication." One who is publishing may, from courtesy, adopt names which he knows have been applied by some other person; but in that case he must append his own cognomen to it, and not that of his friend, for he alone will be responsible to the scientific world for it, and his publication alone can be referred to as authority. If another has given a name and written a description, which the publisher chooses to adopt *in toto*, stating the fact, there can then be no objection that such name should stand, with its author's cognomen appended. Many works of recent date exemplify the force of the objections above made, and have justly incurred unqualified reprobation. Perhaps no case is more glaring than that of M. Kiener, in his beautiful work on Shells, where he has been in the habit of adopting the names imposed by M. Valenciennes, in the museum of the Garden of Plants, and appending M. V.'s name as authority, while Kiener alone describes the shells, and his work is the only one that can be referred to as authority. Now, as many errors are found to exist in the work, a writer very pertinently inquires, who is to be responsible, he who names without describing, or he who describes without naming?

But we proceed to the second part of the Report, in which are offered recommendations for *improving the nomenclature* in future. They are briefly comprised under the seven following rules.

A. "The *best* zoological names are those which are derived from the Latin and Greek, and express some distinguishing characteristic of the object to which they are applied."

B. "It is recommended that the assemblages of genera termed *families*, should be uniformly named by adding the termination *idæ*, to the name of the earliest known, or most typically characterized genus in them; and that their subdivisions, termed *sub-families*, should be similarly constructed, with the termination *inæ*."

C. "Specific names should *always* be written with a small initial letter, even when derived from persons and places, and generic names should be always written with a capital."

D. "It is recommended that the authority for a specific name, when not applying to the generic name also, should be followed by the distinctive expression, (*sp.*)"

E. "It is recommended that new genera or species be *amply* defined, and *extensively* circulated, in the first instance."

F. "It is recommended that in subdividing an old genus, in future, the names given to the subdivisions should agree in gender with that of the original group."

G. "It is recommended that in defining new genera, the etymology of the name should be always stated, and that one species should be invariably selected, as a type or standard of reference."

Under rule A, certain classes of words are specified as objectionable: as, *a. Geographical names;* because, though a name may indicate that an object may be found in such a country, it may also be found equally common in other countries, and therefore the name does not tell the whole truth. *b. Barbarous names;* by giving Latin terminations to local, native names. *c. Technical names;* names expressive of trades or professions, unless carefully chosen. *d. Mythological names. e. Comparative names;* as maximus, minimus, &c. *f. Generic names, compounded from other genera;* this supposes such an alliance between two genera that no other can intervene. *g. Specific names derived from persons. h. Generic names derived from persons. i. Names of harsh and inelegant pronunciation. k. Ancient names of animals applied in a wrong sense.* When the original animal to which they were applied can be ascertained, such names are most desirable. *l. Adjective generic names. m. Hybrid names;* i. e. words compounded of two languages. *n. Names closely resembling other names already used. o. Corrupted words;* those which are ungrammatically compounded. *p. Nonsense names;* words without any derivation or meaning whatever. *q. Names previously cancelled by the operation of rule* 6. *r. Specific names raised to generic.*

Some of these might be regarded as undesirable, rather than as objectionable; such as those under classes *b*, *c*, *d*, *h*. To class *d*, we can see very little objection. To class *g*, we feel very strong objections; not simply because complimentary designations, unless "restricted to persons of high eminence as scientific geologists," are in very bad taste, but because of the awkwardness there is in attempting to pronounce names belonging to a nation whose language we are unacquainted with. What person acquainted with the English language only, or we may add with Latin and Greek also, would venture upon such words as *Michaudi*, *Dupetit Thouarsii*, *Le Guillouii*, *Entrecastauxii*, *Gheisbreghtii*, *Eschscholtzii*. This difficulty is not all on the part of the Englishman; on the contrary, the names of Englishmen and Americans are more formidable and forbidding to all the nations of southern Europe, than their names are to us. Few of us would be likely to recognize our own names when articulated in French or Italian.

With rules A and B, and with the first more especially, would we fully concur; but from the next we should decidedly dissent. The reason given for beginning a specific name, when derived from a proper name, with a small initial, "that when used alone, it is liable to be occasionally mistaken for the title of a genus," seems to us to be too trivial. Persons who are so little experienced as to be misled thus, would be misled by almost any thing. Besides, that the contrary custom is an ancient and almost universal one, we think that few persons would covet the compliment of seeing their cognomen degraded from a *proper* to a *common* name. Perhaps, however, this rule is intended to bear more especially upon the practice sometimes pursued, of commencing common nouns used in the genitive form as specific names, with a capital. If so, we would adopt it thus far. We believe that the following rule would be both more proper and more acceptable. All specific names, except such as are derived from persons or places, should begin with a small initial; and generic names should always begin with a capital.

The method recommended in rule D, is worthy of strict attention. Another mode which we like still better is, to append the name of the author of both the genus and the species, where they are different, thus, *Cyprina Islandica*, Lin., Lam., the name of the person who instituted the species being always kept in close proximity to the specific name, as being more important than the generic name, inasmuch as it is to be unchanged. The only difficulty in the way is, that the means for ascertaining the author of a generic term are not always at hand, and it could hardly be expected that memory would serve for both genus and species. Whoever cannot adopt the latter method, should not fail to apply the former, as recommended.

Any one who has observed the indiscriminate coupling of generic and specific names of incongruous genders, induced by the removal of species from one genus to another, will see the propriety of the precaution proposed in rule F.

These remarks are all that need be offered at this time. The Report itself is fully illustrated throughout, and cannot but do good. We cannot but hope that some method will be taken to give it to the American scientific public in an entire form.

Nothing could have been more timely, in furtherance of the movement so simultaneously made at all the principal foci of science, than the *Nomenclator Zoologicus* of Agassiz. Nor is it probable that any man living is better qualified than he, to undertake a work of the kind.

His plan is as follows. He first gives an alphabetical list of every genus which has been instituted, whether adopted or not, in each of the classes of zoology; he gives its author, the work in which it originally appeared, and the date of publication of that work; then the derivation of the name; and finally, the family to which the genus belongs. Then there is to be a general register combining all the classes, thus bringing side by side, the names which have a double use, and showing where priority belongs. The different items are so printed, in various type, upon the same line, as to be easily distinguished. We sincerely wish he had added one other item, which would have tended greatly to banish from among scientific men, sounds repulsive to classic ears—we mean the accentuation.

So far as genera are concerned then, no zoologist will hereafter be excusable for using, for any new genus, a word already in use; nor for being ignorant of all the genera which have been instituted in any particular family; nor for employing any but the anterior name, when two or more names have been imposed upon the same genus of animals.

This may be thought, at first glance, but a small work; and yet the author has already catalogued, upon the above plan, upwards of seventeen thousand names. Doubtless, many more will be hereafter found, in works to which he has not yet had access. No unpublished names have been introduced.

M. Agassiz has expressed his views, in his introduction, with regard to some of the laws of nomenclature, which, when compared with those laid down in the Report just noticed, accord in the main with our own. For instance, he says he does not think it judicious to discard *barbarous* names, now in use; as in that case nearly a thousand would come to be rejected, and as many others substituted. Nor would he reject all words doubly employed. To show the absurdity of too great strictness in the laws of nomenclature he states, that he knows of more than a thousand names, common to genera in botany and zoology; and says he prefers that some one more solicitous for such eclat than himself, should undertake to substitute other names and affix their *superbus* MIHI.

It is the first grand step towards extrication from future confusion in nomenclature, that we have the generic appellations thus before us under one glance; the next will be to construct tables of specific names upon the same plan, and this we hope will ere long be attempted, in some of the classes at least. We agree with the publishers of the "Nomenclator Zoologicus," that this is a work indispensable to all zoologists and paleontologists.

ART. II.—*A Statement of Elevations in Ohio, with reference to the Geological Formations, and also the Heights of various points in this State and elsewhere;* by CHARLES WHITTLESEY, Esq. of Cleveland, Ohio.

IN giving the levels for Ohio, it should be understood that they have been taken with reference to Lake Erie, as a zero. The surface of Lake Erie has generally been considered as five hundred and sixty four feet above tide-water at Albany; see the Report for Michigan, 1839–40. The topographer of that State, S. W.

Higgins, Esq., puts it at 565.333 feet. If this last number represents the levelage of the Erie Canal, it is probably good for the surface of the Lake, as it was when the surveys were made for that work, twenty five years since. The surface, however, fluctuates in the extreme about six feet, thus rendering all measurements based upon the Lake as a starting point, liable to an error of that amount. The Ohio Canal was explored in 1824–5, and of course its elevations are noted with regard to the stage of water at that time. The difference between 1816 and 1824 in the surface of the Lake, will render all our levels along the Ohio Canal, when referred to the ocean, inaccurate by that amount. In the latter part of the year 1815, and all of the year 1816, the Lake was *high*, about four feet above the point of greatest known depression. From 1819 to 1822 it was *low*, and in 1825 was still but about two feet *above* the lowest known point. The error in adopting the Lake surface, in 1824, as a starting point, may therefore be two feet, making its general surface above the tide-water at Albany, in 1824–5, 563 feet, and at the time of the great rise in June, 1838, 567 feet. In estimating the heights given below, I have used the commonly received number of 564, to express the surface of the Lake. Both upon the Erie and the Ohio canals and other works, the slopes sometimes given to the bottom are rejected, because unknown: where there is more than one summit they counteract each other in some degree. Where there are fractional feet they are rejected. In some cases there are short intervals not measured, or the minutes of a portion of the heights are wanting, or the authorities are contradictory; these are designated by an interrogation, and will go for what they are worth. Information derived from so many sources, and transcribed many times from one note-book to another by different persons, must of course be subject to errors. But it has been drawn from the best authorities, viz. the profiles and reports of the engineers in the public employ.

For location of points I have adopted Columbus, the capital of Ohio, in latitude 39° 57′ north, longitude 83° 3′ west, as the centre of reference. The general course and distance from Columbus being given, the courses and distances of the different places among themselves may easily be found.

The order of stratification in Ohio is as follows, beginning at the lowest of our explored rocks, the limestone.

1. Limestone; thickness unknown, not exceeding 1000 feet; subdivided as follows by Dr. Locke: (1.) Blue limestone and blue limestone marls, over 500 feet in thickness; (2.) Marl, 25 feet; (3.) Flinty limestone, 52 feet; (4.) Marl, 106 feet; (5.) Cliff limestone, 89 feet. This limestone is the surface rock over about two fifths of the western part of Ohio, and extending into Indiana.
2. Bituminous slate, or black shale, 250 to 350 feet.
3. Fine-grained or Waverley sandstone, 25 to 350 feet.
4. Conglomerate or pebbled sand-rock, 100 to 600 feet.
5. Coal measures, say 2000 feet.

FORMATION No. I.—*Elevation of some points at the surface of the limestone formation and at the bottom of the slate.*

Place.	Course and distance from Columbus.	Height above the ocean.	Local dip per mile.
Columbus,		761 feet	S. 81° 52′ E., 22¾ feet.
Bloomingville, Erie Co., deep cut of rail-road,	N. 8¼° E., 100 miles.	724 ? "	
Dublin, Franklin Co.,	N. 36° W. 11 "	831 "	
Bainbridge, Ross Co.,	S. 12½° W. 52 "	744 "	
West Union, Adams Co.,	S. 15¼° W. 80 "	934 "	S. 80½° E., 37.4 feet.
Three miles S. E. of Dayton, bottom of cliff limestone,	S. 76¾° W. 62 "	868 "	N. 14° E., 6 feet.

With the exception of Dayton, these locations are at or near the outcrop of the overlapping slate, and consequently in or near the line of bearing.

No. II.—*Points on the surface of the black shale and under face of the fine-grained sandstone.*

Place.	Course and distance.	Height.	Dip.
Newburg village, Cuyahoga County,	N. 41° E., 154 miles.	764 feet.	S. S. E., very slight.
Sandusky township, Crawford Co., (east line,)	N. 7½° E. 62 "	948 ? "	S. E. and slight.
Big Walnut Creek, National Road,	N. 84° E. 8 "	804 "	nearly E.,—about 30 feet per mile.
Head of Paint Creek Canal, Ross Co.,	South, 43 "	814 "	S. 83° E. 31.99 feet.
Morse mill, on canal, near Portsmouth,	S. 3¼° E. 83 "	518 "	

The last station is about fifteen miles east of the outcrop, which accounts for its being lower in natural level than the others. This formation occupies a narrow belt of about twenty miles in width along the Scioto valley, widening as it extends northward to the Lake. It is here about sixty miles in breadth, east and west, and extends eastward in form of a narrow strip along the southern shore, to and beyond the State line.

No. III.—*Points on the surface of the fine-grained sandstone, corresponding with the inferior face of the conglomerate.*

Place.	Course and distance.	Elevation.
Old Forge, Portage township, Summit County,	N. 45¾° E., 110 miles.	908 feet.
Near Chagrin Falls, Cuyahoga County,	N. 39¾° E. 153 "	894 "
Newton Falls, Trumbull County,	N. 51¾° E. 140 "	898 "
Narrows of Licking, Ohio Canal,	N. 77° E. 44 "	764 "
Jackson township, Jackson Co., near J. Stinson's,	S. 16½° E. 55 "	785 "

The fine-grained sandstone region immediately succeeds the slate, and occupies a tract similar in form, though not quite as extensive. Next to it, on the east, the conglomerate is the surface rock, and from a narrow strip at the south, enlarges, after passing the line of the Reserve,* to a width of fifty miles, spreading over the northeastern counties.

No. V.—*Some points in the lowest bed of coal.*

Place.	Bearing from Columbus	Elevation.	Dip.
Brookfield, Trumbull County, near State line,	N. 55¾° E., 164 miles	990 feet.	Nearly S.,—about 20 feet.
Tallmadge, Summit County,	N. 45¾° E. 112 "	1069 "	S. 33¾° E., 18½ feet.
National Road, between Jacktown and Gratiot,	N. 85° E. 38½ "	1014 "	S. 87° E.—about 35 feet per mile.
Lick township, Jackson Co.	S. 22¼° E. 59 "	760? "	E.'ly and variable.

A line drawn from the Portage summit northeasterly, and parallel to the Lake shore, will be a general boundary of the coal region on the north in Ohio; and continued from this summit to the Licking summit, and thence south to the River, it will form the western limit of this great field, extending to the Alleghanies.

Elevation of places in Ohio.

Place.	Elevation—feet.	Surface rock.
Little Mountain, Lake Co.,	1164	Conglomerate.
Mantua, Portage Co., summit of Chagrin and Cuyahoga Rivers,	1140	"
Mahoning summit, Champion, Trumbull County,	908	Fine-grained sandstone.
Brookfield, Trumbull Co.,	1154	Coal measures.
Portage Summit Lake,	958	Top of conglomerate.
High land adjacent,	1150?	
Ravenna summit, Portage and Ohio Canal,	1068	Coal measures.
Hanover, Columbiana Co., Sandy and Brown Canal summit,	1123	"

* The Reserve is the eastern part of Ohio.

Place.	Elevation—feet.	Surface rock.
Huron summit, swamp, S. E. corner of county,	978	Near junction of slate and limestone.
Harrisville, Medina County, Kilbuck summit,	901	Conglomerate.
Tyamochtee summit, T. 5, S., R. 15, E., Marion Co.,	898	Limestone.
Blanchard's fork of Anglaise, 2¾ miles E. of Fort Findlay, Hancock Co.,	1052	"
Loraine's summit, Miami extension canal,	942	"
Somerset, Perry Co.,	1159	Border of coal measures.
Zanesville—river, at bridge,	679	
" hill, E. of town,	801	Coal measures.
Hillsborough, Highland Co.,	1124	Limestone.
Greenville, Darke Co.,	1044	"
Summit between Scioto and Mad rivers, near Mechanicsburg, Champagne Co.,	1007	"
Summit of Great Miami and Scioto, Logan Co.,	1350?	"

Height of places in Michigan, above the ocean.

Head waters of Belle River, Lapeer County,	992	feet.
Summit between waters of Saginaw Bay and Lake Mich.	673	"
Pontiac summit, Clinton and Kalamazoo Canal,	914	"
Hillsdale County, seven miles east of Jonesville,	1211	"
Summit of Central Railroad, on the line between Jackson and Washtenaw Counties,	1015	"
Fort Holmes, Mackinaw,	797	"

Height of Lakes.

Ontario,	232 feet.
Erie,	565.333
St. Clair,	570.005
Huron and Michigan,	578.008
Superior,	596.180

Height of points on the Ohio.

Pittsburg,	705	feet.
Marietta,	567	"
Portsmouth,	479	"
Cincinnati,	432	"

Summit of Wabash and Erie Canal, near Fort Wayne, Indiana, - - - - - - - -	810 feet.
Summit of Chicago Creek and Illinois River, -	595 "
Portage, Fox and Wiskonsan Rivers, Fort Winnebago,	699 "

Elevations in Pennsylvania.

Conneaut Lake, - - - - - -	1074 feet.
Alleghany summit, northern route of Schlatter's surveys, - - - - - - -	2002 "
Sugar Run summit, two miles north of Portage Railroad summit, - - - - - - -	2183 "
Chestnut Ridge, National Road, - - -	3612 "
Keyser's " " " - - - -	2843 "
West Alexandria, - - - - - - -	1797 "
Washington, - - - - - - -	1400 "

Elevations in New York.

Chatauque Lake, - - - - - -	1291 feet.
Franklinville, Chatauque County, (in a valley,)	1588 "
Summit between Elm Creek and Little Valley Creek, Cattaraugus County, - - - - - -	1725 "
Summit between Big and Little Valley Creeks, -	2180 "
" " Cayuga Lake and Susquehanna River,	981 "
" " Seneca Lake and Chemung River,	890 "
" " the sources of the Alleghany and the waters of Lake Erie, at the lowest pass, - -	1200 "
The ledge of *Niagara limestone* causing the cataracts of Niagara, Genesee, Oswego and Black Rivers,	551 "
Height of land between Buffalo and Lewiston,	640 "
" " between Buffalo and Lockport, -	590 "
Mohawk, at Little Falls, - - - - -	385 "
Hills adjacent, - - - - - -	1097 "
Round Top, Catskill Mountains, - - -	3804 "
Summit on Welland Canal, - - - -	624? "

This collection of altitudes is now published with a desire to bring out similar statements from other quarters. Topographical geology is of the highest value in reducing the science from a state of general calculation to the exactness of mathematical rules. But such results require great labor, and make little show on paper. If the engineers and geologists of the United States would combine and assist each other by publications similar to the above, (excepting its errors,) the difficulty of collecting such facts would be done away with at once. This Journal appears to be the most convenient organ for such publications. When tables can be formed showing the elevation, extent, thickness and dip of the great formations in the individual States, topographical and geological models may be constructed, which shall be miniature copies of each.

Art. III.—*Tides in the North American Lakes;* by D. Ruggles, 1st Lieut. 5th Regt. U. S. Infantry.

Father Louis Hennepin, during his voyage of discovery in 1679, observed singular currents and fluctuations in the Straits of Michillimackinac, for which he was unable to give satisfactory explanations. More recently the subject has attracted the attention of scientific observers, without producing satisfactory results.

The Hon. Lewis Cass, while governor of Michigan Territory, caused observations to be made during the months of July and August, 1828, at Green Bay, then within the jurisdiction of Michigan, which have been generally received as conclusive evidence of the non-existence of tides in the great North American lakes.

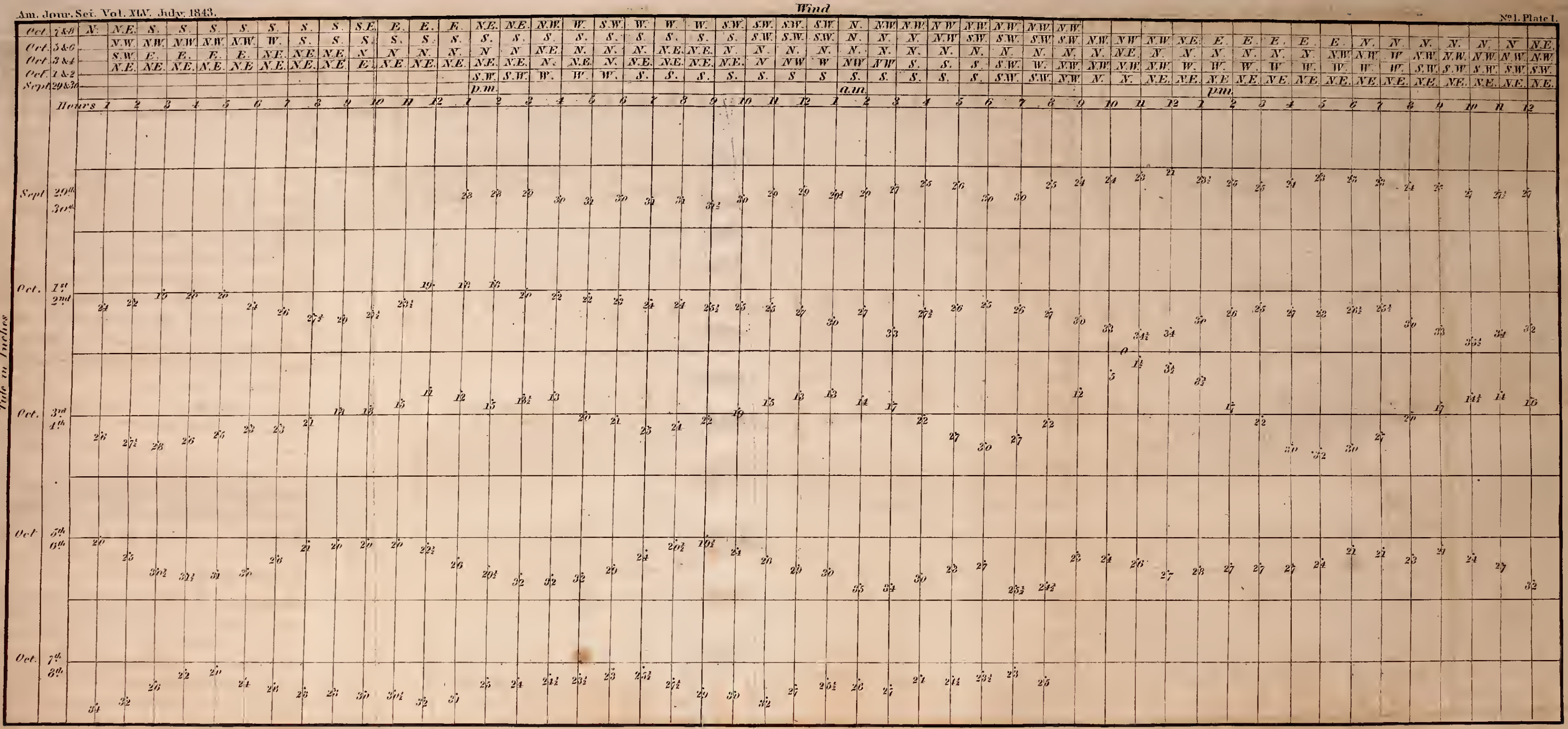
On examination of the table of the Governor's observations, in 1836, I was led to believe that an erroneous view of the subject had been entertained throughout the investigation.

I find the Governor's tabular statement, published in a small volume entitled "Historical and Scientific Sketches of Michigan," embodied in an able article from the pen of Colonel Henry Whiting of the Army, and as I am unable to satisfy myself whether the tabular statement has ever appeared in the Journal of Science, I have considered it necessary to transcribe it, (see table A,) reserving also the liberty of occasional reference to Col. Whiting's communication.

I now propose to make a preliminary examination of this table, with the view of ascertaining the results, premising that satisfactory precautions were apparently taken to guard against local inequalities and fluctuations.

Taking as an example, the 16th of July, we find that observations were made at 6½ and 8 o'clock, A. M., and at 1, 4, and 7½

Plate I.



Tabular Diagram (C.)

P. M. The question at once arises, as to the time of high water. Was it high water between 8 and 1 o'clock? Was it low water at 1 or 4 o'clock? We observe that embarrassment meets us at the very threshold, and it is therefore unnecessary to quote largely from this table, as observations were, in no instance, taken more than six times during twenty four hours. Bearing this fact in mind, a bare inspection of the table unfolds its true characteristics; and were I to adopt it as conclusive evidence, I should be led to concur in the opinion of its distinguished author, "that the changes in the elevation of the water are entirely too variable to be traced to any regular permanent cause."

Reflection satisfied me, however, that an error in this tabular statement may have arisen from the absence of rapid consecutive observations, which alone enable us to grasp a principle clothed in subtle and fluctuating indications.

The accompanying table B, is the result of observations made under my superintendence, during the months of September and October, 1836, at Green Bay, Wisconsin Territory, with the view of elucidating the subject, and ascertaining, as far as practicable, the nature of these fluctuations.

They were made at Fort Howard, on the left bank of the Fox River, and about one mile above the expansion of the river into Green Bay. It is to be observed, however, that the river still winds some four or five miles through an extensive alluvial deposit, before it is lost in deep water. The river is about half a mile wide, and is between fifteen and forty feet in depth.

The station taken for Gov. Cass' observations is two miles above Fort Howard, on the right bank of the river, and about two miles below where the current ceases to be perceptible.

My observations were made by a vertical rod, protected from local inequalities by two perforated concentric enclosures, and graduated in inches referring to a zero plain above high-water mark. The station was near a sentinel's post, and under the immediate and constant supervision of the sergeant of the guard, and may therefore, I think, be considered as entirely free from any accidental inequalities or irregularities of importance.

It will be observed, that the general direction of the winds was also noted; but as I regard them only as a modifying cause, increasing or diminishing results according to their direction, I shall not comprehend their influence in this discussion, and especially as they constitute an element susceptible of future examination.

TABLE A.

Table of observations on the rise and fall of the Lake at Green Bay, made by Gov. Cass in 1828.

Day of the month.		Time of the day.	Course of the wind.	Strength of the wind.	Height of water.
July	15.	9	N.	Moderate.	9
"	"	Noon.	"	"	8
"	"	4	"	"	5½
"	"	7½	"	"	11
"	16.	6½	W.	Light.	10
"	"	8	"	"	10½
"	"	1	"	"	6
"	"	4	"	"	6
"	"	7½	"	"	6½
"	17.	6	S. W.	"	6
"	"	8	"	"	8¼
"	"	Noon.	"	"	6
"	"	4	"	"	5½
"	"	7½	"	"	8
"	18.	6	"	"	1
"	"	8	"	"	4
"	"	Noon.	"	Strong.	7
"	"	4	"	"	4
"	"	7½	"	"	7
"	19.	6	W. of S. W.	Light.	7
"	"	8	"	"	5
"	"	9	"	"	11
"	"	Noon.	"	"	5½
"	"	4	"	"	7
"	"	7½	"	"	6½
"	20.	8	No wind.	None.	6
"	"	Noon.	N. W.	Light.	8
"	"	4	"	"	10
"	"	7½	"	"	5½
"	21.	8	S. W.	"	9½
"	"	2	"	"	10
"	"	4	"	"	
"	"	7½	N.	Violent.	18
"	22.	7	S. W.	Light.	10
"	"	Noon.	"	"	0
"	"	4	"	"	14
"	"	7½	"	"	11
"	23.	8	"	Moderate.	3½
"	"	Noon.	"	"	1½
"	"	4	"	"	11½
"	"	7½	"	"	11
"	24.	8	N. E.	Light.	9
"	"	Noon.	"	"	8

TABLE A.—CONTINUED.

Day of the month.	Time of the day.	Course of the wind.	Strength of the wind.	Height of water.
July 24.	4	N. E.	Light.	14
" "	7½	"	"	10
" 25.	8	S. W.	Moderate.	5½
" "	Noon.	"	"	5½
" "	4	"	"	9½
" "	7½	"	"	12½
" 26.	8	"	Light.	11
" "	Noon.	"	"	10
" "	4	"	"	8½
" "	7½	"	"	7
" 27.	8	W.	"	10½
" "	Noon.	"	"	6
" "	4	"	"	2
" "	7½	"	"	12
" 28.	8	N.	Fresh.	4
" "	Noon.	"	"	11
" "	4	"	"	2
" "	7½	"	"	8½
" 29.	8	S. W.	Light.	11
" "	Noon.	"	"	6½
" "	4	"	"	4
" "	7½	"	"	8
" 30.	8	N. W.	"	9
" "	Noon.	"	"	5
" "	4	"	"	9
" 31.	8	S. W.	"	7
" "	Noon.	"	"	7
" "	4	"	"	8
" "	7½	"	"	7½
Aug. 1.	8	N.	"	13
" "	Noon.	"	"	9
" "	4	"	"	7
" "	7½	"	"	8
" 2.	8	N. E.	"	7
" "	Noon.	"	"	11
" "	4	"	"	1
" "	7½	"	"	11
" 3.	8	S. W.	"	4
" "	Noon.	"	"	10
" "	4	"	"	7
" "	7½	"	"	9
" "	9	"	"	7
" 4.	8	N. W.	"	7
" "	Noon.	"	"	8
" "	4	"	"	12
" "	7½	"	"	5

TABLE A.—CONTINUED.

Day of the month.	Time of the day.	Course of the wind.	Strength of the wind.	Height of water.
Aug. 5.	8	S. W.	Light.	6
" "	Noon.	"	"	6½
" "	4	"	"	12
" "	7½	"	"	7
" 6.	8	"	"	6
" "	Noon.	"	"	9
" "	4	"	"	8
" "	7½	"	"	10
" 7.	8	"	"	8
" "	Noon.	"	"	6
" "	7½	"	"	9
" 8.	Noon.	N.	"	6
" "	4	"	"	7
" "	7½	"	"	7
" 9.	8	S. W.	Strong.	2
" "	Noon.	"	"	0
" "	4	"	"	13
" "	7½	"	"	6
" 10.	8	N. E.	Pretty fresh.	13
" "	Noon.	"	"	9
" "	4	"	"	10
" "	7½	"	"	16
" 11.	8	"	Light.	10
" "	Noon.	"	"	8
" "	4	"	"	6
" "	7½	"	"	7
" 12.	8	S. W.	"	8
" "	Noon.	"	"	2
" "	4	"	"	5
" "	7½	"	"	9
" 13.	8	"	"	0
" "	Noon.	"	"	5
" "	4	"	"	4½
" "	7½	"	"	9
" 14.	8	"	Moderate.	4
" "	Noon.	"	"	5
" "	4	"	"	6
" "	7½	"	"	5
" 15.	8	N.	Fresh.	10
" "	Noon.	"	"	6
" "	4	"	"	3
" "	7½	"	"	4
" 16.	8	S. W.	Light.	6
" "	Noon.	"	"	6
" "	4	"	"	5
" "	7½	"	"	7

TABLE A.—CONTINUED.

Day of the month.	Time of the day.	Course of the wind.	Strength of the wind.	Height of water.
Aug. 17.	8	N.	Light.	7
" "	Noon.	"	"	3
" "	4	"	"	11
" "	7½	"	"	7
" 18.	8	N. W.	"	4
" "	Noon.	"	"	7
" "	4	"	"	10
" "	7½	"	"	5
" 19.	8	S.	Fresh.	4
" "	Noon.	"	"	8
" "	4	"	"	8
" "	7½	"	"	5
" 20.	8	S. W.	Light.	5
" "	Noon.	"	"	7
" "	4	"	"	11
" "	7½	"	"	8
" 21.	8	N.	"	6
" "	Noon.	"	"	8
" "	4	"	"	10
" "	7½	"	"	9
" 22.	8	None.	"	10
" "	Noon.	"	"	7
" "	4	"	"	11
" "	7½	"	"	14
" 23.	8	S. W.	"	8
" "	Noon.	"	"	7
" "	4	"	"	11
" "	7½	"	"	7
" 24.	8	"	Moderate.	8
" "	Noon.	"	"	9
" "	4	"	"	7
" "	7½	"	"	8
" 25.	8	"	Light.	10
" "	Noon.	"	"	4
" "	4	"	"	11
" "	7½	"	"	13
" 26.	8	Northerly.	"	12
" "	Noon.	"	"	8
" "	4	"	"	10
" "	7½	"	"	7
" 27.	8	"	"	12
" "	Noon.	"	"	8
" "	4	"	"	9
" "	7½	"	"	14
" 28.	8	"	"	12
" 29.	Noon.	"	"	13

Table B.

Tide and Wind table at Green Bay, Wisconsin Territory, for seven days, commencing the 29th of September, 1836.

Tide—(in inches.)

1836. Hour,	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	High water.	Low water.
Sept. 29, P. M.	28	28	29	30	31	30	31	31	31½	30	29	29	1 & 12 P. M.	9 P. M.
" 30, A. M.	29½	29	27	25	26	30	30	25	24	24	23	21	4 & 12 A. M.	1 A. M.
" " P. M.	23½	25	25	24	23	23	23	24	25	27	27½	27	1 & 6 P. M.	3 & 11 P. M.
Oct. 1, A. M.	24	22	19	20	20	24	26	27½	29	27½	23½	19	3 & 12 A. M.	9 A. M.
" " P. M.	18	18	20	22	22	23	24	24	25½	25	25	27	1 P. M.	12 P. M.
" 2, A. M.	30	27	33	27½	26	25	26	27	30	33	34½	34	6 A. M.	3 & 11 A. M.
" " P. M.	30	26	25	27	28	26½	25½	30	33	35½	34	32	3 P. M.	10 P. M.
" 3, A. M.	26	27½	28	26	25	23	23	21	18	18	15	11	12 A. M.	3 A. M.
" " P. M.	12	15	13½	13	20	21	25	24	22	19	15	13	1 & 12 P. M.	7 P. M.
" 4, A. M.	13	14	17	22	27	30	27	22	12	5	1½	3½	1 & 11 A. M.	6 A. M.
" " P. M.	8½	17	22	30	32	30	27	20	17	14½	14	16	1 & 11 P. M.	5 P. M.
" 5, A. M.	20	25	30½	31½	31	30	26	21	20	20	20	22½	1 & 10 A. M.	4 A. M.
" " P. M.	26	29½	32	32	32	29	24	20½	19½	24	26	29	9 P. M.	4 P. M.
" 6, A. M.	30	35	34	30	28	27	25½	24	23	24	26	29	9 A. M.	2 A. M.
" " P. M.	28	27	27	27	24	21	21	23	21	24	27	32	7 P. M.	1 & 12 P. M.
" 7, A. M.	34	32	26	22	20	24	26	28	28	30	30½	32	5 A. M.	1 & 12 A. M.
" " P. M.	31	25	24	24½	23½	23	25½	27½	29	30	32	27	6 P. M.	1 & 11 P. M.
" 8, A. M.	25½	26	27	24	24½	23½	23	25					7 A. M.	

Wind—(direction and strength.)

1836. Hour,	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
Sept. 29, P. M.	S. W.	S. W.	W.	W.	W.	S.	S.	S.	S.	S.	S.	S.
" 30, A. M.	S.	S.	S.	S.	S.	S. W.	S. W.	N. W.	N.	N.	N. E.	N. E.
" " P. M.	N. E.	N. E.	N. E.	N. E.	N. E.	N. E.	N. E.	N. E.	N. E.	N. E.	N. E.	N. E.
Oct. 1, A. M.	N. E.	N. E.	N. E.	N. E.	N. E.	N. E.	N. E.	N. E.	E.	N. E.	N. E.	N. E.
" " P. M.	N. E.	N. E.	N.	N. E.	N.	N. E.	N. E.	N. E.	N. E.	N.	N. W.	W.
" 2, A. M.	N. W.	N. W.	S.	S.	S.	S. W.	W.	N. W.	N. W.	N. W.	N. W.	W.
" " P. M.	W.	W.	W.	W.	W.	W.	W.	S. W.	S. W.	S. W.	S. W.	S. W.
" 3, A. M.	S. W.	E.	E.	E.	E.	N. E.	N. E.	N. E.	N.	N.	N.	N.
" " P. M.	N.	N.	N. E.	N.	N.	N.	N. E.	N. E.	N.	N.	N.	N.
" 4, A. M.	N.	N.	N.	N.	N.	N.	N.	N.	N.	N. E.	N.	N.
" " P. M.	N.	N.	N.	N.	N. W.	N. W.	W.	N. W.	N. W.	N. W.	N. W.	N. W.
" 5, A. M.	N. W.	N. W.	N. W.	N. W.	N. W.	N. W.	W.	S.	S.	S.	S.	S.
" " P. M.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S. W.	S. W.	S. W.
" 6, A. M.	N.	N.	N.	N. W.	S. W.	S. W.	S. W.	S. W.	N. W.	N. W.	N. W.	N. E.
" " P. M.	E.	E.	E.	E.	E.	E.	N.	N.	N.	N.	N.	N. E.
" 7, A. M.	N. E.	S.	S.	S.	S.	S.	S.	S.	S. E.	E.	E.	E.
" " P. M.	N. E.	N. E.	N. W.	W.	S. W.	W.	W.	W.	S. W.	S. W.	S. W.	S. W.
" 8, A. M.	N.	N. W.	N. W.	N. W.	N. W.	N. W.	N. W.	N. W.				

Remarks on wind and weather.—Sept. 29th, P. M., high and middling clear.—30th, A. M., high, north, clear; P. M., middling clear.—Oct. 1st, A. M., middling; P. M., high.—2d, A. M., high; P. M., middling.—3d, A. M., middling high, some rain; P. M., high wind and rain.—4th, A. M., high, strong wind, N. and N. E., at half past 11 o'clock water standing at zero; P. M., high.—5th, A. M., high; P. M., middling high.—6th, A. M., high; P. M., middling high.—7th, A. M., middling high; P. M., middling high.—8th, A. M., light.

The tabular diagram C, is intended to illustrate the observations embodied in table B, referred to a system of rectangular ordinates.

The scale comprehends 40 parts in altitude to 1 inch, or 1 part $=\frac{1}{40}$ inch; and 4 parts or 4 hours in longitude to 1 inch, or 1 hour$=\frac{1}{4}$ inch. As the observations are referred to a superior plane, as zero, the proper algebraical sign would be *minus;* but I have used the sign *minus* to represent depression, and *plus* elevation, indicating the relation in which they succeed each other.

Example I.—Sept. 29th, at 2 o'clock, P. M. 28 inches, wind S. W. high, and at 9 o'clock, P. M. 31½ inches, wind S. middling, $=-3\frac{1}{2}$ inches, time 7 hours.

Sept. 30th, at 4 o'clock, A. M. 25 inches, wind S. high, $=+6\frac{1}{2}$ inches, time 7 hours; at 6½ o'clock, A. M. 30 inches, wind S. W. high, $=-5$ inches, time 2½ hours; at 12 M. 21 inches, wind N. E. high, $=+9$ inches, time 5½ hours.

Result, 2 elevations, 15½ inches, and 2 depressions of 8½ inches, in 22 hours.

Ex. II.—Oct. 2nd, 3 A. M. 33 inches, wind S. high; 6 A. M. 25 inches, wind S. W. high, $=+8$ inches, time 3 hours.

At 11 A. M. 34½ inches, wind N. W. high, $=-9\frac{1}{2}$ inches, time 5 hours; at 7 P. M. 25 inches, wind W. middling, $=+9$ inches, time 8 hours; at 10 P. M. 35½ inches, wind S. W. middling, $=-10$ inches, time 3 hours.

Result, 2 elevations $+8+9=17$ inches, and 2 depressions $-9\frac{1}{2}-10=-19\frac{1}{2}$ inches, in 19 hours.

Ex. III.—Oct. 4th, 1 A. M. 13 inches, wind N. high, and at 6 A. M. 30 inches, wind N. high, $=-17$ inches, time 5 hours.

At 11½ A. M. 0 in., wind N. strong, $=+30$ inches, time 5½ hours; at 5 P. M. 32 inches, $=-32$ inches, wind N. W. high, time 5½ hours; and at 11 P. M. 14 inches, wind N. W. high, $=+14$ inches, time 6 hours.

Result, 2 elevations $+30+14=44$ inches, and 2 depressions $-17-32=-49$ inches, time 22 hours.

It is believed that these examples illustrate the principles involved, in such a manner as to induce continued and minute investigation, and the employment of instruments to comprehend all of the elements entering into the calculation.

Colonel Whiting remarks, that "in speculating on the supposed tides of the North American lakes, it has been natural to regard the head of Green Bay as the point where they would show themselves in the greatest fullness. The course of planetary attraction, operating on a line from east to west, would traverse a space of from four hundred and fifty to five hundred miles." And further, he remarks, that "the configuration of the coasts too, through which the line passes, would appear to lend much extraneous aid, to give whatever wave might be formed an undue elevation."

I fully concur in the opinion advanced respecting the position at Green Bay, as one favorable for observations, but an inspection of the outline of the lakes and straits operated on, convinces me that the phenomenon would be retarded and diminished by the irregularities and sharp projections, instead of being increased, as inferred by the gentleman quoted.

It has been, I believe, satisfactorily demonstrated, that certain periodical *undulations*, induced by planetary influence, constitute tides, attended, of course, with local superficial translation. It is to be observed, that when the sun and moon act in conjunction, or opposition, the attractive force, being then most powerful, the result must be the most evident; and when the moon is in quadrature, that attractive force becomes the least possible, yet never so slight as to induce the belief, that the phenomenon has ceased; still possessing, therefore, the characteristics of a tide. Now, in continuation of this view, we observe that the variable and often cramped configuration of the coast, united with the alternate deep and shallow water, and also the islands which so strongly guard the entrance to Green Bay, tend manifestly to impede undulation, as well as to limit the influence of local translation; therefore these fixed and permanent modifying causes, being a constant quantity, the variable influence of planetary attraction, must, when *slight*, be almost neutralized, with great irregularities attending the results; and only when *great* present the most satisfactory evidence of cause and effect.

The low specific gravity of lake water, varying very little from unity, renders it less subject to planetary influence than sea water, and more liable to the action of the winds; moreover the majestic current of the whole body of lake water, collected from innumerable rivers, is diametrically opposed to the course of planetary attraction, having a direct tendency to modify results.

I have observed constant elevation and depression of the ice, three or four feet in thickness, during winter, at Green Bay, with crevices uniformly broken, along shore, through which the water overflowed extensive portions of the surface, indicating those undulations observable during every other period of the year.

I have also the concurrent testimony of several gentlemen of intelligence, that undulations are observed, almost uniformly, throughout most of the great northern lakes.

Detroit, Michigan, Jan. 1, 1843.

Art. IV.—*Observations on some interesting Plants of New England;* by Edward Tuckerman, Jun.

Campanula rotundifolia, (L.)—Hab. (*α.*) Moist rocks, Notch of the White Mountains. (*β.*) Var. *alpina:* caule 3–6-poll. unifloro, foliis caulinis nunc linearibus, radicalibus cordatis ovatisve crenatis s. integris. White Mountains; stony alpine moor on Mount Monroe.—Our plant of the Notch is a little dwarfed, but differs apparently in no other respect from the form of the low country. I have found this very state in wet places in the alpine regions. The plant above noticed as a variety, is very distinct in its ordinary habit, but the examination of more than thirty excellent specimens, has led me to doubt every character by which I had supposed it might be distinguished. The stem-leaves, often regularly linear, vary to lanceolate wherever the plant attains to an inch or two more of height; the radical leaves occur cordate, cordato-ovate, and ovate; crenate, crenulate, and entire. The length of the segments of the calyx does not seem in our plant to afford any character, since in some of my specimens of the alpine variety, this is all but twice as great as in others.—The Campanula linifolia of *DC. Prodr.* 7, 471, *Hook. Bor. Amer.* 2, 27, I have not seen; but if the above observations are, as I believe, correct, its claims to rank as a species, would hardly seem to be greater than those of our variety.

Is not this what Pursh saw in Peck's herbarium, and afterwards described as "Swertia pusilla," from "specimens from Labrador, in the Banksian herbarium," the New England station being no doubt given from memory. I refer only to the White Mountain Swertia, to which our Campanula makes the nearest approach of any thing that has been found. To continue this enquiry for a moment, is there reason to admit that Alchemilla alpina and Sibbaldia procumbens are inhabitants of the New England mountains, as Pursh has said? On the one hand, all our botanists have been unable to find them; while, if memory was the authority, we might conjecture that the Alchemilla was Potentilla tridentata, and the Sibbaldia, P. minima. These are found. On the other hand, the recent discovery of Aspidium aculeatum, after it had been lost almost forty years, in the very mountains where Pursh gathered it,* seems to encourage us to further search for these interesting plants. Dryas integrifolia may also be mentioned, which, though found by Peck, and also seen by Bigelow, *Fl. Bost.*, *edit.* 3, *p.* 219, and by Pursh, has never since occurred to any botanist. So remarkable a plant could hardly, it would seem, be mistaken by any one, much less by the eminent botanists who have given it a station in our Flora. If any where, this may possibly grow on some part of the highest rocky region of Mount Washington, all of which is gone over by the way which Peck and the earlier botanists took, while but a small part, and that the least promising, is traversed by the new path. Some alpine plants are singularly local and rare; as is Arbutus alpina at the White Mountains, found by Dr. Robbins in 1829, but since that time only by Dr. Gray and myself in September last. In 1840, I ascended the great spur of Mount Washington by the old way, with Dryas in mind, but was unsuccessful in finding it. This region is so vast, barren, and difficult of examination, and the plant doubtless so local, that it may be very long before we can pronounce positively whether it is yet an inhabitant of our mountains.

Utricularia intermedia, (Hayne): foliis distichis dichotome multipartitis ambitu reniformibus, laciniis setaceis spinuloso-denticulatis, calcare conico, labio superiore integro palato duplo longiore, pedunculis fructiferis erectis. *Koch, Syn. p.* 579, *Richards. App. p.* 2, *Gray in Ann. Lyc. N. Y.*, *Hook. Bor. Amer.* 2, 118. *U. Millefolium, Nutt. MSS. in herb. Greene.*

Hab. Tewksbury, B. D. Greene, Esq.; Plymouth, in an inundated swamp near the West Pond, with Zizania.—I found only

* The Aspidium was found on the lower part of the Chin of Mansfield. Carex saxatilis grows about three thousand feet higher up. It is improbable that we shall ever come any nearer to the "hemlock woods," where Pursh found the Carex of his Flora.

the leaves, which at once distinguish this from every other species which has yet been found in Massachusetts. Scapes 6–8 inches high, with two or three flowers, which are much smaller than those of U. vulgaris. The leaves somewhat resemble those of yarrow, whence the specific name proposed by Mr. Nuttall for the American plant. They are many-cleft, with the segments linear and spinulose-denticulate. The bladders grow separately from the leaves, on branched stalks. The specimens seem to agree with the European; and the Tewksbury plant is pronounced to be U. intermedia, by Hooker, l. c.

U. STRIATA, (Le Conte): foliis dichotomis capillaceis, calcare breviusculo subconico obtuso, labio superiore rotundato-ovato subemarginato margine undulato, inferiore trilobo margine reflexo, pedunculis erectis 2–6-floris. *Torr. Fl.* 1, 20. (*p. m.*)

Hab. Tewksbury, Mr. Greene; *(v. s. ex herb. Greene sine nom.)* Agrees with the New Jersey plant in every respect, but that in the latter the flowers are somewhat larger. Leaves capillary at the extremities, but apparently analogous with the setaceous true leaves of U. intermedia. Bladders few, among the leaves. Flowers somewhat numerous; in my specimen six. Spur short, obtuse. The Flora of New England is very rich in this curious and elegant genus. With these, eight species are now known to be inhabitants of our waters; while in the recent New York catalogue of Dr. Torrey, only five are mentioned. One or two others will most probably be added to our list; and I am almost certain that I have the true U. minor from Plymouth.

OXYRIA RENIFORMIS, *R. Br., Oakes, Pl. N. Eng. (in Hovey's Mag.) p.* 16.—Hab. White Mountains; moist ravines in the most alpine regions, Pickering and Oakes, 1825; E. T. 1840. I believe this plant has been found by no others, and it is one of the rarer forms of our alpine regions. I found it growing on rocks in a very secluded alpine gully, with Cardamine bellidifolia.

BETULA.—Having been led to examine several small-leafed Birches in my collection, I arrived at some results which seemed worthy of being mentioned, especially as there is some confusion in regard to our species.

B. PUMILA, (L.): humilis, foliis orbiculato-obovatis serratis subtus ramulisque pubescentibus, amentis fœmineis cylindricis. *Willd. Sp.* 4, 467, *L. Mant.* 124, *Kalm, Itin.* 1, 108, *(sub B. nana.)* *B. glandulosa, Sulliv. (ex spec.)*

Hab. "In several low places towards the hills," Pennsylvania, Bartram, (ex Kalm.) High Mountains of New York and Pennsylvania, Pursh. Cedar Swamps, Columbus, Ohio, Sullivant. The plant from the last station, is the only one that I have seen. It seems to be the B. pumila of Willdenow, and is distinguished from B. glandulosa by the entire want of the resinous dots found on that species, as well as by its dense soft pubescence, mostly broad-ovate leaves and larger aments. The present is possibly the more southern, and B. glandulosa the more northern of these allied species. Although it is enumerated in the Flora Boreali-Americana of Sir William Hooker, it would seem that the distinguished author refers his own specimens rather to B. glandulosa. The citation "Canada, (Linn.)" I have not been able to identify; our species being established on Kalm's specimens, in the first Mantissa, where the habitat mentioned is "America Septentrionalis."

B. glandulosa, (Michx.): humilis, ramis glanduloso-punctatis glabris, foliis obovatis basi integerrimis obtuse serratis glabris, amentis fœmineis breviusculis lato-cylindraceis, squamis trifidis lobis oblongo-subovatis subæqualibus. E. T.—*Michx. Fl.* 2, 180, *Pursh, Fl.* 2, 622, *Hook. Bor. Amer.* 2, 156, *and B. pumila, Hook. l. c.*

Hab. "Circa lacus a sinu Hudsonis ad Mistassins," Michaux. "Canada," Masson, in herb. Lambert. (White Mountains?) My specimen is that from the Lambertian herbarium, and a very beautiful one. The species seems to be distinguished by its very glabrous habit, and its leaves (all in my specimen) cuneate and very entire at the base; thus somewhat resembling small leaves of Cratægus parvifolia. By the former character it is separated from B. pumila, and by the latter from the succeeding species. Among my White Mountain specimens are two, that may possibly belong to this species.

B. Littelliana (mihi): humilis glabra, ramis resinoso-punctatis, foliis subrotundis grosse serratis petiolis nunc 4 lin. longis, amentis fœmineis oblongo-cylindraceis, squamis trifidis lobis oblongo-obovatis intermedio longiori.

Hab. White Mountains, in Oakes's Gulf, between Mount Washington and Monroe, and elsewhere in the alpine regions. A somewhat erect shrub, with leaves which are from two to four times as large as those of B. nana. To this last, which occurs on our mountains in a state undistinguishable from the Swedish and Scottish plants, our specimens cannot properly be referred. And from the B. glandulosa of Michx., with whose description my Canada specimen collected by Masson, perfectly agrees, they seem quite distinct. I have, however, seen many more specimens of B. Littelliana than of the former plant, the characters of which may possibly vary.

D. in honorem b. Henrici Little, M. D., Montium Alborum scrutatoris acerrimi.

B. NANA (L.): humillima glaberrima, ramis lævigatis s. resinoso-punctatis, foliis suborbicularibus grosse dentatis, amentis fœmineis brevibus cylindraceis, squamis profunde trifidis laciniis oblongis subæqualibus. *Hook. Bor. Amer.* 2, 156, (*p. m.*) *Michx. Fl.* 2, 180, *Pursh, Fl.* 2, 622, *Bigel. Fl. Bost.* 356.

Hab. White Mountains, alpine regions; Cutler, Peck, Bigelow, Boott, Oakes, etc. A very low, often prostrate shrub, with very small, more or less orbicular leaves, and short cylindrical aments. The leaves are generally about five lines each way in dimension: those of B. Littelliana occur often nine lines in length, by more than an inch in breadth, the petioles being longer in proportion. The aments also in the latter are twice as large as in B. nana.

B. PAPYRACEA, *β. minor*, (mihi): foliis minoribus ovatis acutiusculis aliquandoque subrotundatis obtusis glaberrimis.

Hab. White Mountains, alpine regions. From a shrub of the size, and much the habit of B. Littelliana, this attains sometimes in sheltered spots to the height of nine feet and over, and a circumference of sixteen inches. These were the dimensions of one measured by me on Mount Pleasant. It is a well-marked form, and in its most alpine and smallest states, may always be recognized by its ovate, more or less acutish leaves. Rounded leaves also frequently occur among the others. It was perhaps the discovery of such leaves upon northern forms of B. alba, which led some botanists to deny *(Fl. Lapp.* 275,) the distinctness of B. nana. If cold has this effect on the leaves of these shrubs, the character loses some of its value, though it is a very striking one. All the four last mentioned Betulæ, (if B. glandulosa really occurs,) approach each other very nearly in their smallest forms, but may with care be distinguished. The smallest shrub, with suborbicular leaves, is B. nana; the large one, with rounded leaves, B. Littelliana; that with ovate acutish leaves, B. papyracea, *β.* minor, and B. glandulosa has rather large cuneate leaves.

Alnus.—Instead of one, there are three very different Alders in New England. The following account of them will, it is hoped, be found accurate.

A. incana, (Willd.): foliis submembranaceis oblongis acutiusculis basi obtusis s. cordatis margine sublobatis argute serratis subtus glaucis pubescentibus venis hirsutis axillis venarum nudis, amentis fœmineis ovalibus, stipulis oblongo-lanceolatis. E. T.—*Betula incana, L. Suppl.* 417. *Alnus incana, Willd. Sp.* 4, 335, *Muhl. Catal. p.* 89, *Hook. Bor. Amer.* 2, 157, (*part.*)—*A. glauca, Michx. f. Sylv.* 1, 379, *Oakes, Catal. Verm. p.* 25. *A. crispa, Pursh, Fl.* 2, 623. *(part., non Michx. nec Gray.)*

Hab. "New Hampshire and Vermont. Unknown in the Southern, and rare in the Middle States," Michaux f. Pokono Mountain, Pennsylvania, Pursh, in herb. Lambert. Pennsylvania, Muhlenberg. Massachusetts, and northern parts of New England, as at Cambridge, Woburn, Framingham, Ipswich; and exceedingly abundant about the White Mountains; Oakes, E. T. The Alnus glauca of Michaux's Sylva, though a very abundant species at the north, seems to have been neglected by botanists, and is hardly to be found mentioned in our manuals. By the leaves it is easily distinguishable from A. serrulata, and is besides commonly taller, so as sometimes to become a small tree. I have a specimen from the Lambertian herbarium, ticketed by Pursh "Alnus crispa, July 25, 1808, Pokono,"* which is perhaps the A. incana of Muhlenberg's catalogue, and is certainly only a stunted form of the present species. The shrub is rare, according to Michaux, in the Middle States, and possibly is there found only on the mountains.

Our species seems too near to A. incana to be kept separate. The leaves agree perfectly well with those of my foreign specimens of the latter, while that has been recognized as American by Muhlenberg and by Hooker.

A. rubra, (Marsh.): foliis subcoriaceis obovatis acutis argute serratis venis axillisque venarum villosis, amentis fœmineis ovato-oblongiusculis, stipulis ovalibus obtusis. E. T.—*Betula Alnus, Clayt. & Gronov. Fl. Virg. edit.* 1, *p.* 115. *B. pedunculis ramosis, &c. Clayt. & Gronov. Virg. edit.* 2, *p.* 146. *Betula-*

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*It appears that "*incana*" had originally been written on this ticket, but it is crossed out, and *crispa* written over. And Michaux, after the description of his Alnus glauca, adds the synonym, "A. incana, Willd."

Alnus rubra, Marsh. Arb. p. 20, *(ex Darlingt., descr. quæ.) Betula serrulata, Ait. Kew. edit.* 1, 3, 338. *Betula-Alnus serrulata, Michx. Fl.* 2, 181. *Alnus serrulata, Willd. Sp.* 4, 336, *aucttque. B. incana, β. Hook. Bor. Amer.* 2, 157.

Hab. Northern, Middle, and Western States, Michx. f. New England to Carolina. A straggling shrub, 6–15 feet high, growing in close thickets. Leaves obovate, acute at base, thick and somewhat coriaceous, and rough-veined beneath. Appears very different from A. incana. Is it not possible that Hooker's arrangement above cited, was founded upon specimens of our A. incana, incorrectly referred to the present species? It is a well known fact that the two have long been confounded in this country. The name of our own botanist should have the priority: his description, though short, notices the most striking features of the species, and cannot be mistaken. The A. rubra of Bongard, is many years later. Add to this, that Marshall's name is far more expressive and apt than that of Aiton.

A. crispa, (Michx.): foliis ovalibus acutis basi obtusiusculis duplicato-serratis, pubescentia molli glutinosa indutis s. glabriusculis venis axillisque villosis, amentis fœmineis longe pedicellatis ovalibus, stipulis late ovatis. E. T.—*Betula crispa, Ait. Kew. edit.* 1, 3, 339, *(ex Gray, N. Carol.* 43.*) Betula-Alnus crispa, Michx. Fl.* 2, 181. *Alnus undulata, Willd. Sp.* 4, 336, *Muhl. Catal.* 89.

Hab. Newfoundland and Hudson's Bay, Aiton; Canada, Michaux; New England, (ex Cutler, forsan,) Muhlenberg. White Mountains, sides of the Notch hills, and on the plain of the Ammonoosuck. Also in the alpine regions, E. T.; high peaks of the Green Mountains, Vermont, Dr. Robbins, *(Oakes, Catal. Verm.* 25;) mountains of Essex, N. Y., Mr. Macrae. Aiton's description, though less perfect than that of Michaux, seems to answer to our plant, and is considered as belonging to it, by Dr. Gray, (l. c.) It is our handsomest species, and remarkable, except in the alpine state, for the soft pubescence of its leaves, which are also, and particularly on the lower surface, besprinkled with glutinous particles. From oval, the characteristical form, the leaves vary, occasionally, to broad ovate and even cordate. The aments are on somewhat long pedicels, and add much to the elegance of the shrub. The alpine state has smaller and more glabrous leaves. To this last, Alnus Mitchelliana, Curt.

MSS., from the mountains of North Carolina, with a specimen of which I have been favored by the author, seems to approach, perhaps too near. The erroneous station given by Pursh for this species, (there is, I believe, no evidence that he was acquainted with the true plant, his own specimen belonging to A. incana,) has perhaps contributed to the uncertainty with which it has been regarded. It was probably known to Cutler, but seems to have escaped our other botanists, until recently.

Salix myrtilloides, (L.): foliis oblongo-ellipticis acutis s. obtusis basi obtusiusculis integerrimis utrinque glaberrimis subtus reticulato-venosis glaucescentibus, amentis pedunculatis capsulis ovato-conicis glabris longe pedicellatis, squamis brevibus obtusis pilosiusculis, stylo perbrevi, stigmatis lobis fissis. E. T.—*Wahlenb. Fl. Lapp. p.* 266, *Fries, Mantiss. p.* 71, *Koch, Comment. in Sal. p.* 52. *S. pedicellaris, Pursh, Fl.* 2, 611, *and Auctt. Amer.*

Hab. Swamps, New England; Ipswich, Oakes; Cambridge, Framingham, &c. E. T. A low shrub, with a somewhat virgate habit, and remarkable for its entire smoothness. The leaves are elliptical, with a base more or less obtuse, the margin reflexed, and the under side commonly glaucescent. The fertile aments are rather loosely flowered, the capsules on long pedicels, the stigma almost sessile. No one can compare Pursh's description of his S. pedicellaris with that given by Wahlenberg of S. myrtilloides, without noticing a remarkable agreement in the principal characters of the species. Mr. Oakes long ago suspected that the plants were the same; and a careful study of our S. pedicellaris as compared with Lapland specimens of S. myrtilloides, received from the illustrious Wahlenberg, have satisfied me of their identity. The Lapland species is less inclined to be glaucous, as Pursh described his specimens; but this is believed to be a variable character in this genus. The foreign plant is better distinguished by the broad, often cordate base of the leaves, a habit which I have never observed in ours. But Koch remarks of the species, (Comm. p. 52,) "foliorum forma valde variabilis, occurrunt scil. subrotundo-ovata, basi subcordata apice obtusissima, ovata, oblonga, acuminata, et lanceolata utrinque acuta." I cannot discern any differences in the inflorescence of the two plants. Wahlenberg remarks that there is hardly any Willow so entirely smooth and so very distinct as this. Fries truly calls it elegant; noticing also, as does Wahlenberg, its resemblance in habit to

Vaccinium uliginosum. It being a very northern and remarkably broad-leafed state of the species, which suggests this comparison, it is not surprising that our much larger and narrower-leafed form should not so well compare with our exclusively alpine and small-leafed form of the Vaccinium. Fries remarks upon S. myrtilloides, that its leaves do not easily blacken in drying: this is also true of our plant, which preserves all its beauty in the herbarium. It should be added, that according to Fries and Koch, this is not the S. myrtilloides of Willdenow, nor of Smith.

S. AMBIGUA, (Ehrh.): amentis sessilibus fructiferis breviter pedunculatis, pedunculo minute foliato, capsulis ex ovata basi lanceolatis tomentosis longe pedicellatis, pedicello nectarium ter quaterve superante, stylo brevi, stigmatibus ovatis emarginatis, foliis ellipticis obovatis lanceolatisve recurvato-apiculatis integerrimis vel remote denticulatis, subtus rugoso-venosis adpresse villosis subsericeis postremo glabratis, stipulis semi-ovatis rectis. *Koch, Syn. p.* 655, *Comment. p.* 49. *S. plicata, Fries, Novit. p.* 284. *S. incubacea, Fries, Mantiss.* 1, *p.* 66. *S. repens? Bigel. Fl. Bost. edit.* 3, *p.* 392. *S. fusca, Oakes, Pl. N. Eng. (l. c.) p.* 7.

Hab. White Mountains, in moist alpine ravines; abundant about the outlet of the Lake of the Clouds, and in Oakes's Gulf. Our White Mountain Willow was pronounced by Prof. Fries to be the S. incubacea of his first Mantissa, which I follow Koch in arranging as above. Leaves elliptical, acute or somewhat obtuse, commonly about an inch and a half in length by about half an inch in breadth, glaucous on the under surface, which is more or less covered with silvery silky hairs. Aments rather short, and the style exceedingly so. Our plant occurs with the leaves almost glabrous, and again with somewhat smaller glabrous leaves with the margins reflexed.

S. PHYLICIFOLIA, (L.): foliis ovatis lanceolatisve remote repando-serratis glabratis, subtus glaucescentibus, stipulis semicordatis apice obliquo, amentis bracteatis masculis sessilibus, capsulis pedicellatis conico-elongatis subsericeis stylo longo. *Fries, Mantiss.* 1, *p.* 50.

Hab. White Mountains, in moist alpine ravines; Lake of the Clouds; Great Gulf, (called Gulf of Mexico.) A handsome, low, spreading shrub, with rather large generally broad-elliptical very smooth leaves, which are remotely repand-serrate, and glaucous on the under side. I have never found the aments. Specimens of this were examined by Prof. Fries, and pronounced to be the S. phylicifolia of his Mantissa.

S. Cutleri, (mihi): foliis ellipticis acutis obovatisve obtusis basi semper acutis glanduloso-denticulatis supra lævibus subtus glaucis glabriusculis, (junioribus sericeo-villosis,) amentis pedunculatis elongato-cylindraceis compactis, capsulis ovato-conicis breviter pedicellatis glabris, squamis obovatis atris sericeis, stylo mediocri stigmate bifido lobisque demum fissis. E. T.—*S. prostrata, Muhl. Catal. p.* 95*? (forsan ex Cutler.) S. retusa, Oakes, herb. S. Uva Ursi, Pursh? Torr. Catal. N. Y.*, 1840, *p.* 170, *Oakes, Pl. N. Eng. (l. c.) p.* 7, *Barratt in Notes of a Tour, &c. p.* 8, *Oakes, Catal. Verm. p.* 25, (non Pursh.)

Hab. White Mountains; abundant about rocks in the micaceous soil of Mount Franklin, Mount Pleasant, Mount Monroe, &c.; Cutler, Oakes, E. T. Also on the Great Haystack, (var. infra laudat,) and mountains of Essex, New York, Mr. Macrae.—A much depressed, commonly almost prostrate alpine shrub, variable in some respects, but always distinguished by the glossiness and glaucous under side of its elliptical or obovate leaves. These are by no means constant in size, and sometimes occur an inch and a quarter long by half an inch in breadth. I have gathered a curious form on the Great Haystack, all the leaves being small and very narrow, averaging indeed little more than a third of an inch in length by a line and a half in breadth. The description of S. Uva Ursi of Pursh, does not agree with our Willow: nor do there appear to be any characters given by that author which will distinguish his species from S. retusa. I have two specimens without fruit, from the Lambertian herbarium, ticketed S. Uva Ursi, which also seem to me to be undistinguishable from S. retusa, with a fine set of specimens of which, from Switzerland, I have compared them. Our plant differs from this species in the acute habit of its leaves, which are also thicker, and its much elongated compact aments, the capsules being only half the size of those of S. retusa. It is very distinct. Hooker admits S. Uva Ursi doubtfully, in his Flora, while he enumerates S. retusa as belonging to our northern regions.

D. in honorem primi inventoris, b. Manassis Cutler, S. T. D., A. A. S., Botanicorum Novæ Angliæ sæc. xviii facile Principis, qui Montes Albos sedule explorans, species alpinarum nostrarum multas detexit, Floraque sua mscr. elaborate et optime descripsit.*

Populus candicans, (Ait.): *Hort. Kew. edit.* 1, 3, *p.* 406, *Willd. Sp.* 4, 806, *Pursh, Fl.* 2, 618, *Michx. f. Sylv. Amer.*, *Oakes, Pl. N. Eng.* (*l. c.*) *p.* 6, *ejusd. Catal. Verm. p.* 25.—Hab. Many parts of Vermont, native; Oakes. Also in the Notch of the White Mountains, E. T.

P. balsamifera, (L.): *Michx. Fl.* 2, 244, *Willd. Sp.* 4, 805, *Pursh, Fl.* 2, 618, *Hook. Bor. Amer.* 2, 153, *Oakes, Pl. N. Eng. (l. c.) p.* 6, *ejusd. Catal. Verm. p.* 25.—Hab. Vermont, Oakes; St. Johnsbury, Vt., a very fine large tree, E. T.

Juncus Greenei, (Oakes and Tuckerm.): culmo erecto stricto rigido subcompresso striato nudo basi foliorum vaginis incluso, foliis linearibus canaliculatis rigidis apice subulatis erectis culmi medium vix superantibus, anthela terminali composita pauci-radiata bractea culmum superante suffulta, radiis erectis ramis corymbosis multifloris, sepalis acutis mucronatis scariosis oblongo-ovatis capsulam ovato-ellipticam mucronatam haud æquantibus. E. T.

Hab. Sands, Tewksbury, B. D. Greene, Esq.; Ipswich, Plymouth, W. Oakes, Esq.; Cambridge, Needham, Dover, &c. E. T.

This handsome rush resembles the foreign J. squarrosus in many respects, and is perhaps the species so named by Muhlenberg in his Catalogue. At the same time it seems to differ from J. squarrosus in some of its most striking features. In our plant the leaves are erect and not spreading; the anthela is shorter and more corymbose, with an elongated bract. In J. squarrosus the bracts and margins of the sepals are white, giving a marked character to the plant; in J. Greenei all these parts are brown. The two species differ also in their capsules; those of J. squarrosus being slightly obovate, and nearly double the size of those of our plant. This can hardly be confounded with any other of our Junci. The more naked culm at once distinguishes it from the other allied species.

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* In 1789 Cutler had distinguished and described Microstylis and Comandra as new genera; Orchis fimbriata is referred by him to O. psycodes, L., to which Dr. Gray has recently shown it to belong. An account of the labors of this botanist will be attempted on some future occasion.

In honorem cl. inventoris, Floræ Novanglicanæ jam diu illustratoris et fautoris D.

Potamogeton pulcher, (mihi): foliis omnibus petiolatis, submersis lanceolatis natantibus ovatis oblongo-ovatisve cordatis petiolis sæpius longioribus, seminibus ventricosis lunatis dorso acute carinatis. *P. natans, Bigel. Fl. Bost.*

Hab. Ponds and slow streams, Medford, Stoneham. With the floating leaves of P. natans, this species possesses the lunate and ventricose fruit of P. lucens and P. prælongus. From these species, both of which inhabit Fresh Pond, in Cambridge, it is distinguished by its much larger seeds, and its beautifully cordate broad-ovate coriaceous floating leaves, often on very short petioles. From P. natans the structure of the fruit at once separates it; that of the former being not lunate, obtuse at the margins, shining and finely linear-punctulate; while in ours, besides the difference of shape, the surface is dull and somewhat roughened by elevated anastomosing veins. *Conf. Koch, Syn.*

P. Claytonii, (mihi): foliis submersis membranaceis anguste linearibus longis acutis margine undulatis sparsimque minutissime spinuloso-scabris versus basim vix attenuatis sessilibus, natantibus petiolatis (petiolis nunc breviusculis) oblongis lanceolatisve vix coriaceis (nervis non nisi versus lucem conspicuis,) caule ramoso.—*P. foliis lanceolato-oblongis, etcætt., Clayt. & Gronov. Fl. Virg. edit. 2, p. 23, ex parte certe. P. fluitans, Pursh, Fl. 1, 120, Bigel. Fl. Bost. p. 63, Torr. Fl. 1, 196.*

Hab. Ponds and slow streams, Roxbury, Cambridge. Very different from P. fluitans. From P. heterophyllus, to which it has been latterly referred by our authors, though apparently with doubt by Dr. Torrey, it seems to me to differ as much as from P. natans. In a large set of the European Potamogetons, I have not found any which agree with our plant in the peculiar features of its submersed leaves. The P. heterophyllus of Pursh, collected by him at "Walker's meadows," seems to be also the P. hybridus, β. of Michaux, and to differ but little from this species. It may perhaps be proper to consider it a variety: β. foliis submersis numerosioribus angustissimis. But it is possible that this latter plant will be found to be a distinct species.

P. lucens, (L.) *Bigel. Fl. Bost.* This agrees in every respect with the foreign plant, and is easily distinguished by its leaves, which in P. prælongus are ovate and amplexicaul at base, while in P. lucens they are oval or lanceolate, and petiolate. I have found both species in Fresh Pond.

P. Robbinsii, (Oakes.) This very curious species is quite abundant in Fresh Pond, Cambridge, and will, probably, as Mr. Oakes has suggested, be found by no means rare in New England.

Carex paniculata, (L.) In a cold swamp between Concord and Lexington, on the turnpike, with C. exilis, and Eriophorum alpinum. This is the true plant, an opinion sustained by Dr. Gray when he examined my specimens. The *extrene* regularly paniculate form did not occur; nor is this uniformly found in Europe. It seems quite possible that this state may yet be found at our station.

C. alopecoidea, (mihi): spica composita oblonga, spiculis 8–10 ovatis aggregatis superne masculis, stigmatibus 2, perigyniis ovatis plano-convexis fere enerviis in rostrum mediocrem bifidum margine serrulato-scabrum acuminatis, sqaumis ovatis mucronatis fructum subæquantibus, culmo triquetro angulis scabro.—C. cephalophora, var. maxima, Dew.

Hab. Penn Yan, New York, Dr. Sartwell. Resembling C. cephalophora, but quite different in the fruit. It seems to me more difficult to distinguish it from C. vulpina. In that species and C. stipata, the fruit is ovate and scarcely margined. But in C. cephalophora it is somewhat tapering towards the base, and conspicuously margined. By this character, perhaps, C. cephalophora, C. Muhlenbergii, C. sparganioides, and C. rosea, may be separated to form a distinct group; for which the name Muhlenbergianæ is not inappropriate, especially as most if not all the species were discovered by Muhlenberg. From the Multifloræ of Kunth these seem to differ as much as from the Vulpinæ.

C. canescens, (L.)—*β. alpicola*, (Wahlenb.): spiculis superioribus aggregatis, capsulis patentibus acutis convexo-planiusculis subacutangulis. *Wahl. Monogr. Car. no.* 49, (1803,) *ejusd. Fl. Suec. p.* 595. *C. curta, β. brunnescens, Pers. Syn., Koch Syn. C. Gebhardi, Hopp. non Schk.—γ. sphærostachya*, (mihi): spiculis 3–4 subrotundis paucifloris, perigyniis oblongioribus in rostrum conspicuum acuminatis.

Hab. (*α.*) Mountains; White Mountains, Great Haystack, Grand Monadnoc, Green Mountains, Aschutney. Spikelets approximated, shorter. Glumes brown with a white margin. Fruit commonly also brown. Our plant agrees with original specimens of Wahlenberg's variety, and of the C. Gebhardi of Hoppe. Koch adopts Persoon's name, citing for Wahlenberg's only the Flora Suecica, but this last was first published in the Monograph of the illustrious Swede, which appeared before the Synopsis of Persoon.—(γ.) Mountains; White Mountains, Green Mountains, &c. Also in swamps, Phippsburg, Me., Nuttall; Penn Yan, New York, Dr. Sartwell. This differs still more from the true C. canescens than even the variety β. The form and important characters of the fruit seem however to forbid a separation of either. In γ. I have observed the color of the fruit to be always green. The latter variety does not probably occur in Europe; but I think it passes into the former on our mountains. That this species is the true C. canescens of Linnæus, independently of the descriptions, is the opinion of Wahlenberg, Fries, Koch, and Torrey & Gray. The unanimous opinion of the great botanists of Sweden, with respect to a Linnæan Swedish plant, would seem perhaps to be of more weight than even the Linnæan herbarium; a contrary opinion has however prevailed.

C. NEGLECTA, (mihi): spica composita, spiculis 3–4 subrotundis remotiusculis paucifloris inferne masculis, stigmatibus 2, perigyniis oblongo-lanceolatis plano-convexiusculis enerviis in rostrum conspicuum scabriusculum margine ciliato-serratum integrum acuminatis squama acuta hyalina ⅓ longioribus, culmo tenui erectiusculo scabro.

Hab. Rocky hills, near Montpelier, Vt., 1839. This plant has the peculiar habit of inflorescence of C. trisperma, by which it is distinguished from the variety of C. canescens. In the fruit it differs very much from C. trisperma. The stem in my specimens is very scabrous.

C. RIGIDA, (Gooden.): spica mascula solitaria, fœmineis 2–4 erectis inferiore pedunculata oblongis, stigmatibus 3 abortu sæpius 2, perigyniis ellipticis obtusis nervosis obscure trigonis punctulis minutis conspersis rostro brevi tereti integro mucronatis, culmo angulis scabriusculo s. glabro. E. T.—*C. rigida, Gooden. in Linn. Tr.* 2, 193, *Koch. Syn. p.* 755, *Boott. in Hook. Bor. Amer.* 2, 217. *C. saxatilis, Willd. Sp.* 4, 275, *Wahlenb. Lapp. p.* 247, *Torr. Cyp. p.* 397, *Kunth, Cyp. p.* 411, *Drej. Rev. p.* 41, (non Linnæi, test. Hartman in Koch, Goodenough, et Boott; descriptioneque Linnæana (in Fl. Lapp.) ut mihi videtur, ipsa.)—

β. Bigelovii, (mihi): spicis fœmineis 2–5 elongatis remotiusculis laxis inferioribus patentibus longe pedunculatis. *C. Bigelowii, Torr. in Schwein. Anal. Tab. C. Washingtoniana, Dew. Car. in Sill. Jour.* 10, 262, *C. saxatilis, β. Torr. Cyp. ex parte.*

Hab. (α.) Greenland, Vahl; Arctic America, Drummond; Labrador, Schlechtendal; White Mountains, and Great Haystack, N. H.; Chin of Mansfield, Camel's Rump, and other high peaks of the Green Mountains, Vt. Also on the mountains of Essex County, N. Y., Mr. Macrae. (β.) Mount Washington, and other of the White Mountains; Chin of Mansfield.

It seems probable that the normal state of Carex rigida is tristigmatical. In ten mature achenia from the Lapland plant, from a Norwegian specimen, and from the Scottish C. rigida, I have observed in all the same approximation to a three-angled shape, which is noticeable in our plant, and in it becomes at last conspicuous, and the angles quite distinct. It is worthy of note, moreover, that this Carex very often fails to perfect its fruit. In the greater part of my specimens, from Scotland, Lapland, Germany, Greenland, and New Hampshire, the perigynia are shrivelled, and without apparent vestiges of any achenium. These observations are confirmed by Koch, who introduces the character "subtrigonis" in his diagnosis of the species; and by Drejer, (l. c.) who inserts "stigmata 2, rarius 3," in his description of it. The variety β. is distinguished as being perhaps the most luxuriant and developed state of the species known, and is probably confined to this continent. It attains to a height of 18 inches, with spikes often an inch and a half long, which are commonly loosely flowered; the lower ones somewhat remote, and on spreading peduncles from half to more than an inch long. The fruit of C. rigida seems to vary considerably. A perigynium of the Scottish plant agreed so nearly with one of ours, as to be almost undistinguishable under the microscope, while neither perfectly agreed with the fruit of the Lapland and Norwegian forms. In the last the perigynium is conspicuously nerved; in the Scottish and ours much less so, and sometimes not at all; in the Norwegian, the whole surface is covered with dark reddish points; in the Scottish these are nearly, but not wholly (as Schkuhr would seem to intimate, 1, 55,) wanting, or rather their color is more or less wanting, which is also the case in ours. The achenia also differ considerably, which is in a measure owing to the difference in size and shape of the perigynium. Our plant, particularly in β, runs into variations. The spikes are sometimes wholly female, with only a few male flowers at the top of the highest; and again they are almost entirely male. Sometimes the long spreading spikes are crowded toward the top, with a somewhat paniculate aspect. All this seems to show that the species is with us in a peculiarly developed and luxuriant state.

Agrostis Pickeringii, (mihi): culmo erecto, foliis planis linearibus, panicula ovata diffusa ramis verticillatis erectiusculis scabris, glumis subæqualibus subbidentatis, carina inferioris apice mucronata superiori acuta glabriuscula, palea inferiori ovato-lanceolata acuta s. erosa punctatula nervata, superiori exacte ovata obtusa enervi, arista e medio dorsi tortili scabra florem bis superante.—β. *rupicola*, (mihi): minor panicula contracta glabriuscula, floribus plerumque albo-purpurascentibus. *A. canina, var. alpina, Oakes, Catal. Verm. p.* 32.

Hab. White Mountains, Great Haystack. (β.) White Mountains, Pickering and Oakes; Camel's Rump, Vt. What seems the typical state of this plant is a rather tall alpine grass with an elegant diffuse panicle.* The variety is a much smaller plant, frequently not over three inches high, when it much resembles in habit such specimens as I have seen of the European A. rupestris. The characters of our plant will not, however, be found to agree with those of A. rupestris. It seems even more different from A. canina, of which I have good foreign specimens, and which is well marked in its habit.

D. in honorem cl. inventoris, Floræ nostræ eximii illustratoris.

A. concinna, (mihi): culmo humili erecto, foliis filiformi-setaceis, panicula ovata patente glabra, glumis haud æqualibus, inferiori acuta mucronata versus apicem scabriuscula, superiori acuta glabra, palea superiori vix ulla, inferiori glabra infra medium arista tortili scabra florem superante basique pilis paucissimis instructa.

Hab. White Mountains; stony alpine moor on Mount Monroe, with Carex scirpoidea and Potentilla minima. Somewhat resembling A. alpina in habit, but that is remarkable for the two bris-

* I have gathered this on the sandy plain of the Ammonoosuck, where indeed Arenaria Grœnlandica may also be found; both being doubtless brought down from the mountains by the spring freshets.

tles at the top of the inferior palea, and the awn at its base. It is quite different from A. rupestris and A. canina.

Trichodium, *Auctt. Amer.* The following species constitute, I believe, all of those grasses which have in this country been referred to the genus Trichodium. Wishing to ascertain several New England plants, and finding that I possessed all the species mentioned in our books, I resolved to study the whole. The result of no little labor is given below, where it will be found, I hope, that the arrangement and synonymy of the species is improved, however little the characters. The genus Trichodium is wholly disallowed by Hooker, (*Brit. Fl.* 1, 33,) and by Koch, (*Syn.* 780,) and seems, (now that more is known,) to be not only artificial, but even founded on an incomplete analysis. Still it may be said, that the two species which form the genus as constituted by Michaux, are distinguished by a habit almost as striking as that of A. Spica venti, which is separated from Agrostis by Trinius and Lindley; and by characters which seem, perhaps, to vary less in the original species, than in those other forms which have since been connected with them.

A. laxiflora, (Richards.): culmis erectiusculis basi purpurascentibus striatis glabris, foliis linearibus inferioribus angustioribus involutis breviusculis suberectis striatis utrinque scabris vaginis scabriusculis, panicula tenuissime capillari laxissima ramis verticillatis scabris summitatibus pauciter floridis, glumis inæqualibus vix lineam longis lanceolatis inferioris, carina scabra superiorisque versus apicem palea semilineam longa acuta glabra. E. T.—*Trichodium laxiflorum, Michx. Fl.* 1, 142, *Muhl. Gram. p.* 60, *Torr. Fl.* 1, 83, *Darlingt. Cest. p.* 54. *Agrostis laxiflora, Richards. App. Frankl. Narr. p.* 731. *A. Michauxii, Trin. A. Michauxii, var. laxiflora, Gray, Gram. & Cyp. (cit. Darlingt.)*—β. *montana*, (mihi): cæspitosa, panicula ovata patente demumque divaricata, palea arista tortili exserta e medio dorsi proveniente prædita.—*Trichodium montanum, Torr. (fide ips.) Torr. Fl.* 1, 84.

Hab. (α.) Dried up swamps and pastures. Plymouth, Ipswich, Cambridge, Burlington, Vt. (β.) Dry rocky precipices of the Notch of the White Mountains. The last seems almost a distinct species, and differs in the size at least of its flowers from α. It is not, however, now considered distinct by Dr. Torrey.

A. PERENNANS: culmis fere decumbentibus basi geniculatis ramosis glabris, foliis vage patulis planis glabriusculis s. scabris vaginis lævibus, panicula tenui-elliptica laxiuscula ramis verticillatis erectis scabriusculis, glumis haud æqualibus acutissimis carinis scabris circiter lineam longis, palea glabra vix lineam longa. E. T.—*Cornucopiæ? perennans, Walt. Fl. Carol. p.* 74. *Agrostis anomala, Willd. Sp.* 1, 370. *Trichodium decumbens, Michx. Fl.* 1, 42, *Muhl. Gram. p.* 60. *T. perennans, Ell. Sk.* 1, 99, (*Icon.*) *T. scabrum, Darlingt. Cest.* 1, 54, (*non Willd.*)

Hab. Carolina, Walter, Fraser, Elliott, Curtis; Pennsylvania, Darlington; Columbus, Ohio, Sullivant. The habit of this species is very marked, and it is pronounced "quite distinct" by Dr. Darlington. It is probable that it does not occur very far to the north.

A. ALTISSIMA: culmis erectis duris rigidis crassiusculis, foliis longis lato-linearibus scaberrimis vaginis vix glabris, panicula coarctata ramis verticillatis erectis rigidiusculis scabris summitatibus dense floridis, glumis magnis subæqualibus lanceolatis acuminatis carinis scabris circiter sesquilineam longis, palea glumam superiorem fere æquanti tenuissime pubescenti carina scabra. E. T.—*Cornucopiæ? altissima, Walt. Fl. Carol. p.* 74. *Agrostis dispar, Michx. Fl.* 1, 51. *Trichodium elatum, Pursh, Fl.* 1, 61, *T. n.* 4, (*anon.*) *Muhl. Gram. p.* 62, (*fide Torr.*) *T. elatum, Torr. Fl.* 1, 83.—*β. laxa*, (mihi): panicula laxiori ramis longioribus viridi.—*A. Novæ Angliæ*, (mihi MSS.)

Hab. (*α.*) Carolina, Walter, Curtis; New Jersey, Pursh, Torrey.—(*β.*) White Mountains; about brooks in the Notch. The description of Walter can hardly be improved as respects the prominent features of this very distinct species. Michaux has apparently described it under the name of Agrostis dispar; having detected (it would seem) two paleæ. I have observed in the New Hampshire plant, in a single instance, a membranaceous development at the inner base of the (inferior) palea, from which the bristles usually found on each side of the orifice of the palea, seemed to arise. These bristles, it may be remarked, occur in every American Trichodium; though from the generic character of Michaux, and the silence of other authors, we might suppose they were wanting. The variety *β.* above mentioned, is perhaps a distinct species, but I could not distinguish its florets from those of *α.* under the microscope. It is a coarse, green scabrous, rather erect grass, with somewhat broad leaves and large florets. The name given by Walter, the discoverer of this species, is the oldest, and, it would seem, very appropriate.

A. SCABRA, (Willd.): culmis erectis basi geniculatis glabris, foliis planis linearibus longiusculis striatis scabris vaginis glabris, panicula diffusa ramosa, ramis 4–6 verticillatis brevibus flexuosis patentibus divaricatis, glumis inæqualibus acutis inferiore carina scabra superiore glabra margine scariosis ⅔ lin. longis, palea longiuscula glumam superiorem vix haud æquante glabra. E. T.—*Agrostis scabra, Willd. Sp.* 1, 370, *(fide Muhl.) Trichodium scabrum, Muhl. Gram. p.* 61, *Torr. Fl.* 1, 83, *non Darlingt. Cest. l. c.*—*β. tenuis*, (mihi): vaginis scabris panicula tenui ramis erectis.

Hab. (*α.*) Pennsylvania, 'ubique in sylvis,' Muhlenberg; New York, in woods, common, Torrey; New Hampshire, in mountain forests; (*β.* a small delicate form with a very slender panicle,) rocks of the Flume, Lincoln, N. H. This and the last were not considered by Michaux as belonging to Trichodium; and Willdenow describes the present species as possessing two paleæ. Our plant is always distinguishable by its elegantly flexuous and spreading many-branched panicle, and erect habit.

CALAMAGROSTIS PURPURASCENS, *R. Br., Hook. Bor. Amer.* 2, 240.—Hab. White Mountains; moist alpine grassy places; September. I observed this grass for the first time the present season. It agrees in all respects with the description, and was pronounced to be the plant, at the time, by my excellent friend, Dr. Gray.

POA MODESTA, (mihi): culmo spithamæo basi geniculato ramoso compresso glabro, foliis linearibus tenuiter striatis rigidiusculis supra scabris 3–4 pollicariis circ. semilineam latis, vaginis striatis glabriusculis, ligula conspicua membranacea truncata erosa demumque irregulariter laciniata, panicula stricta vix demum oblonga 6–9 pollicari ramis solitariis filiformibus ramulisque ramosis rhachique scabris, spiculis sparsis breviter pedicellatis (pedicellis 1–3 lin. longis) bifloris, glumis inæqualibus oblongo-lanceolatis tenuissime striatis obtusis erosis glabris, flore inferiori majori sessili lanceolato enervi carina inferiori versus apicem scabriuscula eroso glabro ad basim interiorem pedicello florem alterum minorem fulcienti instructo, caryopsi ovato fusco. E. T.—*Poa? uniflora, Muhl. Gram. p.* 151, (*ex. descr.*)

Hab. Cambridge; wet margins of Fresh Pond brook. Muhlenberg mentions that his plant above mentioned, was sent him from New England; and it seems almost certain that it was a branch of this Poa, from which part of the florets had fallen off. He compares it with P. capillaris, but it seems very distinct from that species. In a large number of specimens of the plant, in several states of development, I observe no variation from the above characters.

Aspidium aculeatum, (Sw.) *Hook. Brit. Fl. edit.* 1, 1, 443, *Hook. Bor. Amer.* 2, 26. *A. aculeatum, Pursh, Fl.* 2, 662. Hab. Green Mountains, Vermont, Pursh, 1806. Moist rocky mountain forest, near the base of the Chin of Mansfield, the highest of the Green Mountains, Vt., Macrae and Tuckerman, 1840. Also at Indian Pass, in the highlands near Mount Marcy, New York, Mr. Macrae. The New York specimens were pronounced by Sir William Hooker to be exactly the plant of the British Flora. It is an interesting and very beautiful addition to the New England Ferns, and seems to have been lost since Pursh's time; having escaped the notice of Boott and Robbins, being wholly omitted by Bigelow and Torrey, and referred, as a doubtful synonym, to A. spinulosum by Beck.

Lycopodium annotinum, (L.): caule repente ramosissimo ramis adscendentibus bi-tri-partitis, ramulis simplicibus in spicas solitarias sessiles terminantibus, foliis quinquefariis lineari-lanceolatis mucronatis apice serrulatis patentibus acerosis ad incrementa annua contractis. *Wallr. Fl. Crypt.* 1, 33, *Michx. Fl.* 2, 283, *Torr. Comp. p.* 388.—*β. montanum*, (mihi): nanum quadrifolium. *L. sabinæfolium, Beck, Bot. p.* 461, *(non Michx. nec Hook.)*

Hab. (α.) Rocky and mountain forests; Manchester, Oakes; White Mountains. (β.) Alpine districts; White Mountains, Green Mountains. The leaves of my low country specimens from Scotland and Bavaria, as well as those from the base of the White Mountains, and from Manchester, are regularly, so far as I have examined, in fives. In β, on the contrary, they occur only in fours. In an alpine Scottish specimen, which seems to be marked by the same habit as our American plant, they are also disposed in fours. It is possible this character is not found to be constant in Britain, for the alpine form is not distinguished by British writers. That, however, it is not unknown to occur there, will appear from Hooker, (Brit. Fl. 1, 452,) who changes the specific character "quinquefariis" to "*about* five rows." In this view there is nothing to distinguish our variety but a slightly dwarf habit, which is just as noticeable in L. dendroideum, when it occurs in alpine situations, and indeed in most plants. I have not, however, as yet observed our plants to vary in this respect. It seems impossible that our Lycopodium should be the L. sabinæfolium of Willdenow, for that was referred by its discoverer, as well as by its describer, to the different group which includes L. complanatum. And it does not seem probable that Michaux, recognizing as he did L. annotinum as a Canada plant, would have referred a plant, wholly undistinguishable from it, to a different species of another section of the genus. The figure of Dillenius, cited by Michaux, seems also inapplicable to our plant in every respect. All my American specimens are noticeable for a cartilaginous mucre at the tip of the leaves, which is much less conspicuous in the alpine Scottish plant. But Wallroth mentions this in his specific character given above. The scale seems also to vary in the length of its acumination, and the serrulation of the leaves is more or less evident.

L. INUNDATUM, (L.): caule subramoso repente, ramis simplicibus solitariis erectis apice monostachyis, foliis linearibus sparsis acutis integerrimis supra curvis, spica sessili foliosa. *Willd. Sp.* 5, 25, *Torr. Comp. p.* 388.—β. *Bigelovii*, (mihi): majus, ramis subramosis elongatis, foliis acuminatis sparsim denticulatis s. integris. *L. Carolinianum, Bigel. Fl. Bost. p.* 384.—γ. *alopecuroides*, (mihi): caule ramisque ut β. foliis lineari-subulatis basi sparsimque ciliato-dentatis. *L. alopecuroides, L. Sp. p.* 1565, *Dill. Musc. p.* 454, (*& Ic.*) *Clayt. & Gronov. Fl. Virg. edit.* 2, *p.* 168.

Hab. (α.) Swamps; Plainfield, Dr. Porter; Topsfield, Oakes; New York, Macrae.—(β.) Wet sandy margins of ponds; Plymouth, Oakes and Tuckerman; (also New Jersey?)—(γ.) Florida, Torrey. The two species, L. inundatum and L. alopecuroides, seem to have been originally distinguished by Linnæus, mainly on account of the ciliate-denticulation of the leaves in the one plant, the other being considered to possess leaves *integerrima*, as entire as possible. With respect to this character, it appears that many other botanists have not taken the same view as Linnæus, though the word in question is retained by most of our authorities. Vaillant, Dillenius, Haller, Necker, Weber, and Hooker, (Brit. Fl. 1, 452,) all either omit to notice this character, or have particularly altered in this respect the Linnæan phrase. In like manner, Michaux and Torrey have substituted "integris" in their descriptions. I have also observed in our plant, and in French specimens, a very marked approach to denticulation, and in several Bavarian specimens, regular teeth. A. alopecuroides is a much larger plant, and the teeth seem to be always present and conspicuous, as Dillenius remarks, to the naked eye. The plant here considered the variety β. is sometimes as large as that just mentioned, but the leaves are less subulate, with but few teeth, or often all quite entire. The variety alopecuroides, if this view be correct, is the extreme southern American form of the species, the variety Bigelovii intermediate, and perhaps not occurring north of Massachusetts, and α. the extreme northern state, common to us with Europe.

L. Selago, (L.) : foliis sparsis octofariis lineari-lanceolatis acuminatis integerrimis imbricato-patulis rigidis lepidotis, caule dichotomo erecto, ramis fastigiatis summis fertilibus. *Wallr. Crypt. Fl.* 1, 32.—α. *densum*, (Wallr.) : foliis omnibus adpressis. *L. densum, Lam. L. Selago, Engl. Bot. t.* 233, *Bigel. Bost. p.* 386, *Torr. Comp. p.* 389.—β. *recurvum*, (Wallr.) : foliis omnibus patenti-squarrosis ramisque subrecurvis. *Wallr. Crypt. Fl.* (1831,) *Hook. Fl. Bor. Amer.* 2, 266. *L. recurvum, Kitaib. in Willd. Sp.* 550.

Hab. (α.) Alpine summits of high mountains ; White Mountains, Green Mountains, in Vermont ; also in the Notch of the White Mountains, near the road. (β.) In the alpine regions of the White Mountains ; also beautifully distinct on rocks at the Flume, in Lincoln, N. H., where it was first found by J. Bradford, Esq., in 1839, and afterwards by myself, at the same spot, in 1840. This last is quite different in aspect, especially the Lincoln plant, and is distinguished by Hooker, in his Flora Bor. Americana. The leaves are narrower than in α, and all more or less patent, squarrose or recurved. The branches are also somewhat recurved.

Cetraria Tuckermanii, (Oakes) : thallo albo-virescente reticulato-lacunoso glabro subtus nigro fibrillis sparsis, laciniis complicatis adscendentibus sinuato-lobatis marginibus crispis, apophysibus minutis nigris punctiformibus instructis, apotheciis elevatis spadiceis margine thallode evanescente cinctis demum perforatis. *C. lacunosa, Hals. Syn. View, Hitchcock, Catal. Mass. p.* 124, *Tuckerman, Lich. N. Eng. in Jour. Bost. p.* 9, (non Ach.)

Hab. Trunks of trees and old rails; New England. The Cetraria lacunosa of Acharius, was a lichen discovered by the late Mr. Menzies, on the North West Coast. Having received from that venerable botanist a specimen of his plant, I find it is quite distinct from what has commonly passed for it here. The plant of Menzies and Acharius is well represented in the figure given of it by the latter author, in his Methodus. The thallus is broad and expanded, very deeply cellulose and reticulate, and very rigid; the apothecia large. Ours is noticeable for its complicated ascending lobes, which are crisped and beset with black grains at the margins, the apothecia becoming at length perforate. It has several points of resemblance to C. ciliaris, its constant companion, and also a lichen peculiar to this continent, but cannot be confounded with that species, which is always remarkable for its dark brown or bronze hue, and much shorter laciniæ. The under surface is most commonly white in the specimens of our plant, but I believe this is an accidental and atypical state.

Solorina saccata, (Ach.): thallo membranaceo appresso lobato cinereo-virescente lobis obtusis, subtus albo avenio fibrilloso, apotheciis laminæ frondis primum applanatis mox saccato-depressis nigro-fuscis. *DC.*, *Fries*, *Lichenogr. p.* 49.

Hab. Trenton, N. Y., Mr. Greene. This curious genus is new to the United States; and the species has not before been published as American. The plant is distinguishable by its rounded black apothecia, more or less sunk in the *surface* of the cinereous-virescent thallus.

Art. V.—*Remarkable example of the Force of Expansion and Contraction, exerted by bodies when subjected to alternations of Temperature,—with a reference to the question whether the freezing point of liquids is influenced by differences in pressure;* by Lewis C. Beck, M. D., Professor of Chemistry, &c. in Rutgers College, N. J.

TO PROFESSOR SILLIMAN.

At one of the docks in the city of New York, ships are raised from the water, for the purpose of being repaired, by hydraulic presses consisting of cylinders with pistons or rams, having cross bars or arms at the ends. To these arms are attached the iron chains which raise the vessel and the frame upon which it is supported. By forcing water, or a mixture of alcohol and water, (used in winter,) into the cylinder, the piston is forced outward, and thus the vessel, with necessary machinery, is brought to the required height.

During the month of December, 1834, a curious fact was noticed in regard to one of these presses, by Mr. Ring, the superintendent of the establishment, who had the kindness to apprise me of the occurrence, and to furnish me with all the particulars. I have delayed the publication, in the hope that some additional information might be obtained. But this expectation has not been realized, and I now send you the following note, nearly in the form in which it was prepared about eight years since.

On Saturday, the 13th of December, 1834, the ship Orleans, of six hundred tons admeasurement, was raised out of the water by means of two hydraulic presses, each of which contained a column of liquid, (common whiskey,) fourteen feet in length, and fourteen inches in diameter. Mr. Ring supposed that after making the proper allowance for friction, each press must have raised three hundred tons. The thermometer during the day ranged at about 40° F. A change in the weather occurred on Sunday, and on Monday, the 15th, the mercury fell as low as 7° F. On Tuesday, the 16th, when the presses were examined, it was found that in one of them the ram had been forced outward one inch and three quarters, raising with it the ship, and the cradle, which weighed about one hundred tons.

The following are the exact dimensions of the apparatus in which this change was observed.

Length of the cylinder, - -	17 feet,	10 inches.	
Diameter, - - - - - -	2 "	5 "	
Length of the column of spirits, -	14 "		
Diameter, - - - - - -	1 "	2 "	
Entire length of the ram, - -	16 "	2 "	
Diameter, - - - - - -	1 "	1 "	
Weight of the cylinder, - -	16 tons,	5 cwt.	

As the effect above described, was at once supposed to be due to the reduction of temperature which had taken place, the engineer was directed to make a moderate fire on each side of the cylinder throughout its whole length. Under this treatment the ram soon commenced a retrograde movement, which continued until it had receded an inch and three quarters, when the ship again rested on her *pawls*, where she was placed on the Saturday previous.

It may be added that the liquor used in the presses at this time was found, upon analysis, to contain about 43 per cent. of alcohol of the specific gravity of .825. Now it is well known that the rate of contraction of such a liquid, by the reduction of temperature which occurred in this case, at least at the ordinary pressure, is greater than that of iron. In order to satisfy myself of the correctness of this statement in the present instance, I took a two ounce bottle, to which a tube of about a foot in length, with a bore of one eighth of an inch, was attached, and filled the bottle and tube with the liquor employed in the press. The whole was then placed in a freezing mixture, and the temperature gradually reduced to 0° F., supposing that to be the lowest degree to which it had been exposed in the cylinder. With the reduction of temperature, the liquor constantly descended in the tube, as I had anticipated.

What then was the cause of the outward movement of the ram, by which such an enormous force was exerted, as to raise the whole of this vessel and the cradle which supported it? The column of liquid was certainly increased in length by an inch and three quarters. If the liquid had remained of the same bulk, the apparent increase might be ascribed to the contraction of the iron. But this view seems to be inadmissible, for the contraction of the iron would, under ordinary pressures at least, have been less than that of the liquid, as has been shown by repeated experiments.

The explanation which I would offer is, that the mixture of alcohol and water, under the enormous pressure to which it was subjected in the hydraulic press, reached its point of maximum density at a higher temperature than under the ordinary pressure. The liquor in the cylinder during the time above mentioned, was either congealed or was near the point of congelation, and thus increased in bulk. Hence the outward movement of the ram, and the raising of the ship.

In the discussions which were had in regard to the present mode of graduating thermometers, I know it was suggested that the freezing point of water varies with the latitude; which, however, was proved not to be the case. But I am not aware that any detailed and accurate experiments have been performed, to determine whether it is influenced by the pressure of the atmosphere; that is, whether water is frozen, or ice liquefied, at the same temperature on the summits of the Alps or Andes, as in the lowest valleys. The known effect of pressure on several of the gases which are thus condensed, would seem to lead us to the conclusion that the congelation of liquids, and the liquefaction of solids, must also be influenced by this cause. If this is so, the occurrence which I have now described, will probably be considered as a remarkable illustration.

I will only add, in conclusion, that the force of expansion and contraction, as measured by the raising and lowering of this ship and its cradle, is more strikingly exhibited, than by any experiment with which I am acquainted.

Rutgers College, March 1, 1843.

Art. VI.—*An effort to refute the arguments advanced in favor of the Existence, in the Amphide Salts,* of Radicals consisting, like Cyanogen, of more than one element;* by Robert Hare, M. D., Professor of Chemistry in the University of Pennsylvania.

[Republished from a pamphlet printed by the author to accompany his "Compendium of Chemistry," and for distribution.]

The following is a summary of the opinions, which it is the object of the subsequent reasoning to justify.

(*a*.) The community of effect, as respects the extrication of hydrogen by contact of certain metals with aqueous solutions of sulphuric and chlorohydric acid, is not an adequate ground for an inferred analogy of composition, since it must inevitably arise that any radical will, from any compound, displace any other radical, when the forces favoring its substitution, preponderate over the quiescent affinities.

(*b*.) But if, nevertheless, it be held that the evolution of hydrogen from any combination, by contact with a metal, is a suffi-

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* An amphide salt is one consisting of an acid and a base, each containing an amphigen body, either oxygen, sulphur, selenium, or tellurium, as its electronegative ingredient.

cient proof of the existence of a halogen* body, simple or compound, in the combination, the evolution of hydrogen from water, by the contact with any metal of the alkalies, must prove oxygen to be a halogen body; also the evolution of hydrogen from sulphydric, selenhydric, or telluhydric acids, by similar means, would justify an inference that sulphur, selenium, or tellurium, as well as oxygen, belong to the halogen, or "salt radical" class.

(*c.*) The amphigen bodies being thus proved to belong to the halogen class, oxides, sulphides, selenides, and tellurides, would be haloid salts, and their compounds double salts, instead of consisting of a compound radical and a metal.

(*d.*) The argument in favor of similarity of composition in the haloid and amphide salts, founded on a limited resemblance of properties in some instances, is more than counterbalanced by the extreme dissimilitude in many others.

(*e.*) As, in either class, almost every property may be found which is observed in any chemical compound, the existence of a similitude, in some cases, might be naturally expected.

(*f.*) As it is evident that many salts, perfectly analogous in composition, are extremely dissimilar in properties, it is not reasonable to consider resemblance in properties, as a proof of analogy in composition.

(*g.*) No line of distinction, as respects either properties or composition, can be drawn between the binary compounds of the amphigen and halogen bodies, which justifies that separate classification which the doctrine requires; so that it must be untenable as respects the one, or be extended to the other.

(*h.*) The great diversity, both as respects properties and composition of the bodies called salts, rendering it impossible to define the meaning of the word, any attempt to vary the language and theory of chemistry, in reference to the idea of a salt, must be disadvantageous.

(*i.*) There is at least as much mystery in the fact, that the addition of an atom of oxygen to an oxacid, should confer an affinity for a simple radical, as that the addition of an atom of this element to such a radical, should create an affinity between it, and an oxacid.

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* The epithet halogen, is applied to bodies whose binary compounds with metals are deemed salts, and which are consequently called haloid salts.

(*j.*) If one atom of oxygen confer upon the base into which it enters, the power to combine with one atom of acid, it is quite consistent that the affinity should be augmented, proportionably, by a further accession of oxygen.

(*k.*) It were quite as anomalous, mysterious, and improbable, that there should be three oxyphosphions, severally requiring for saturation one, two, and three atoms of hydrogen, as that three isomeric states of phosphoric acid should exist, requiring as many different equivalents of basic water.

(*l.*) The attributes of acidity alleged to be due altogether to the presence of basic water, are not seen in hydrated acids, when holding water in that form only; nor in such as are, like the oily acids, incapable of uniting with water as a solvent. Further, these attributes are admitted to belong to salts which, not holding water as a base, cannot be hydrurets or hydracids of any salt radical; and while such attributes are found in compounds which, like chromic, or carbonic acid, cannot be considered as hydrurets, they do not exist in all that merit this appellation, as is evident in the case of prussic acid, or oil of bitter almonds.

(*m.*) It seems to have escaped attention, that if SO^4 be the oxysulphion of sulphates, SO^3, anhydrous sulphuric acid, must be the oxysulphion of the sulphites; and that there must, in the hyposulphites and hyposulphates, be two other oxysulphions.

(*n.*) The electrolytic experiments of Daniell have been erroneously interpreted, since the electrolysis of the base of sulphate of soda would so cause the separation of sodium, and oxygen, that the oxygen would be attracted to the anode, the hydrogen and soda being *indirectly* evolved by the reaction of sodium with water; while the acid, deprived of its alkaline base, would be found at the anode in combination with basic water, without having been made to act in the capacity of an anion.

(*o.*) The copper in the case of a solution of the sulphate of this metal and a solution of potash, separated by a membrane, would, by electrolyzation, be evolved by the same process as sodium, so long as there should be copper to perform the office of a cathion; and when there should no longer be any copper to act in this capacity, the metal of the alkali, or hydrogen of water, on the other side of the membrane, would act as a cathion; the oxygen acting as an anion from one electrode to the other, first to the copper, and then to the potassium.

(*p.*) The allegation that the copper was deposited from the want of an anion (oxysulphion) to combine with, is manifestly an error, since, had there been no anion, there could have been no discharge, as alleged, to hydrogen as a cathion, nor any electrolysis.

(*q.*) The hydrated oxide precipitated on the membrane, came from the reaction of the alkali with the sulphate of copper; the precipitated oxide of this metal from the oxygen of the soda acting as an anion; and the deposit of metallic copper from the solutions performing, feebly, the part of electrodes, while themselves the subjects of electrolyzation.

(*r.*) The so called principles of Liebig,* by which his theory of organic acids is preceded, are mainly an inversion of the truth, since they make the capacity of saturation of hydrated acids dependent on the quantity of hydrogen in their basic water, instead of making both the quantity of water, and, of course, the quantity of hydrogen therein, depend on their capacity.

(*s.*) All that is truly said of hydrogen, would be equally true of any other radical, while the language employed would lead the student to suppose that there is a peculiar association between capacity of saturation, and presence of hydrogen.

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1. Some of the most distinguished European chemists, encouraged by the number of instances in which the existence of hypothetical radicals has been rendered probable, have lately inferred the existence of a large number of such radicals in a most important class of bodies, heretofore considered as compounds of acids and bases. It has been inferred, for instance, that sulphur, with four atoms of oxygen (SO^4) constitutes a compound radical, which performs in hydrous sulphuric acid, the same part as chlorine in chlorohydric acid.

2. Graham has proposed sulphatoxygen as a name for this radical, and sulphatoxide for any of its compounds. Daniell has proposed oxysulphion and oxysulphionide for the same purposes. In reasoning on the subject I shall use the nomenclature last mentioned, not, however, with a view to sanction it, as I disapprove altogether of this innovation, and deny the sufficiency of

* Traité de Chimie Organique, tom. 1, page 7.

the grounds upon which it has been justified. Consistently with the language suggested by Daniell, hydrous sulphuric acid, constituted of one atom of acid and one of basic water, (SO^3+HO) is a compound of oxysulphion and hydrogen (SO^4+H.) Nitric acid (NO^5+HO) is a compound of oxynitrion and hydrogen (NO^6+H.) In like manner we should have oxyphosphion in phosphoric acid, oxyarsenion in arsenic acid, and in all acids, hitherto called hydrated, whether organic or inorganic, we should have radicals designated by names made after the same plan. Their salts having corresponding appellations, would be oxysulphionides, oxynitrionides, &c. Also, in any salt in which any other of the amphigen class of Berzelius is the electro-negative ingredient, whether sulphur, selenium, or tellurium, all the ingredients excepting the electro-positive radical, would be considered as constituting a compound electro-negative radical.*

3. It may be expedient to take this opportunity of mentioning that the advocates of this new view, disadvantageously, as I think, employ the word radical, to designate the electro-negative, as well as the electro-positive ingredient. Agreeably to the nomenclature of Berzelius, the former would be a compound halogen

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* The conception of the existence of salt radicals seems to have originated with Davy. It was suggested by Berzelius, in his letter in reply to some strictures which I published on his nomenclature, in the following language:—

"If, for instance, the true electro-chemical composition of the sulphate of potash should not be $KO+SO^3$, as is generally supposed, but $K+SO^4$, and it appears very natural that atoms, so eminently electro-negative as sulphur and oxygen, should be associated, we have, in the salt in question, potassium combined with a compound body, which, like cyanogen in $K+C^2N$, imitates simple halogen bodies, and gives a salt with potassium and other metals. The hydrated oxacids, agreeably to this view, would be then hydracids of a compound halogen body, from which metals may displace hydrogen, as in the hydracids of simple halogen bodies. Thus we know that SO^3, that is to say, anhydrous sulphuric acid, is a body whose properties, as respects acidity, differ from those which we should expect in the active principle of hydrous sulphuric acid.

"The difference between the oxysalts and the halosalts is very easily illustrated by formulæ. In KFF (fluoride of potassium,) there is but one single line of substitution, that is to say, that of K|FF; whilst in KOOOOS (sulphate of potash,) there are two, K|OOOOS and KO|OOOS, of which we use the first in replacing one metal by another, for instance, copper by iron; and the second in replacing one oxide by another.

"I do not know what value you may attach to this development of the constitution of the oxysalts, (which applies equally to the sulphosalts and others;) but as to myself, I have a thorough conviction that there is therein something more than a vague speculation, since it unfolds to us an internal analogy in phenomena, which, agreeably to the perception of our senses, are extremely analogous."

body. Cyanogen being analogous, is by him placed in the halogen class. I shall, therefore, in speaking of "*salt radicals*," improperly so called, employ the appellation contrived by the great Swedish chemist.

4. Nevertheless it seems to be conceded, that however plausible may be the reasons for inferring the existence of halogen bodies in the amphide salts, it would be inexpedient to make a corresponding change in nomenclature, on account of the great inconvenience which must arise from the consequent change of names.

5. Under these circumstances, it may be well to consider how far there is any necessity for adopting hypothetical views, to which it would be so disadvantageous to accommodate the received language of chemists. In the strictures on the Berzelian nomenclature, which drew from Berzelius the suggestions contained in the quotation at the foot of the preceding page, I stated it to be my impression that water should be considered as acting in some cases as an oxybase, in others as an oxacid; and, in my examination of his reply,* I observed that *hydrous sulphuric acid might be considered as a sulphate of hydrogen, and that when this acid reacts with zinc or iron, the proneness of hydrogen to the aëriform state enables either metal to take its place, agreeably to the established laws of affinity.*

6. There appears to have been a coincidence of opinion between Kane, Graham, Gregory, and myself, as respects the electro-positive relation of hydrogen to the amphigen and halogen elements, which I have designated collectively as the basacigen class; also in the impression that hydrogen acts like a metallic radical, its oxide, water, performing the part of a base. I agree perfectly with Gregory in considering that hydrated acids may be considered as "*hydrogen salts.*" But when the learned editor proceeds to allege that "*acids and salts, as respects their constitution, will form one class,*" I consider him, and those who sanction this allegation, as founding an error upon an oversight. Because the salts of hydrogen, or such as have water for their base, have heretofore been erroneously called acids, we are henceforth to confound salts with acids, and, instead of correcting one wrong name, cause all others to conform thereto!

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* Silliman's Journal, for 1835, Vol. XXVII, page 61.

7. I fully concur with Gregory and Kane, in considering that water in hydrous sulphuric acid, in nitric acid, chloric acid, and in organic acids, generally acts as a base; also, that in this basic water hydrogen performs a part perfectly analogous to that of a metallic radical; but, agreeably to this view, I cannot perceive any difficulty in accounting for the evolution of hydrogen, as suggested in the quotation above made, (6,) agreeably to which, when diluted sulphuric acid reacts with zinc or iron, the liberation of hydrogen results from the superiority of the forces which tend to insert either of these metals in the place occupied by the hydrogen, over those which tend to retain it *in statu quo.*

8. When oxide of copper is presented to chlorohydric acid, it is inferred that the hydrogen unites with oxygen, and the chlorine with the metal; and hence it seems to be presumed, that when oxide of copper is combined with sulphuric acid, a similar play of affinities should ensue: but would it be reasonable to make this a ground for assuming the existence of a compound radical, when the phenomena admit of another explanation quite as simple and consistent with the laws of chemical affinity?

9. Whether hydrogen be replaced by zinc, or oxide of hydrogen by oxide of copper, cannot make any material difference. In the one case, a radical expels another radical, and takes its place; in the other, a base expels another base, and takes its place.

10. There can be no difficulty, then, in understanding wherefore, from the compound of sulphur and three atoms of oxygen, and an atom of basic water, hydrogen should be expelled and replaced by zinc, or that water should be expelled and replaced by oxide of copper; the only mystery is in the fact, that SO^3, as anhydrous sulphuric acid, will not combine with hydrogen, copper, or any other radical, unless oxidized. But this mystery equally exists on assuming that an additional atom of oxygen converts SO^3 into oxysulphion, endowed with an energetic affinity for metallic radicals, to which SO^3 is quite indifferent.

11. In either case, an inexplicable mystery exists; but it is, in the one case, associated with an hypothetical change, in the other, with one which is known to take place.

12. But if hydrous sulphuric acid is to be assumed to be a hydruret of a compound halogen body, *(oxysulphion,)* because it evolves hydrogen on contact with zinc, wherefore is not water, which evolves hydrogen on contact with potassium, sodium, ba-

rium, strontium, or calcium, to be considered as a hydruret of oxygen, making oxygen a halogen body?

13. Boldly begging the question, Graham reasons thus: "*the chlorides themselves being salts, their compounds must be double salts.*"

14. But if the chlorides are salts, the chloride of hydrogen is a salt; and if so, wherefore is not the oxide of hydrogen a salt, which, in its susceptibility of the crystalline form, has a salt attribute which the aëriform chloride does not possess?

15. Further, if the oxide of hydrogen be a salt, every oxide is a salt, as well as every chloride. Now, controverting the argument above quoted, by analogous reasoning, it may be said, "*the oxides themselves being salts, their compounds are double salts.*" Of course sulphate of potash is not a sulphatoxide, as Graham's ingenious nomenclature would make it, but must be a double salt, since it consists of two oxides in "themselves salts."

16. I trust that sufficient reasons have been adduced, to make it evident that the common result of the extrication of hydrogen, during the reaction of zinc or iron with sulphuric or chlorohydric acid, is not a competent ground for assuming that there are, in amphide salts, "compound radicals" playing the same part as halogen bodies.

17. Let us, in the next place, consider the argument in favor of the existence of such radicals, founded on the similitude of the haloid and amphide salts, which is stated by Dr. Kane in the following words:—

"It had long been remarked as curious, that bodies so different in composition as the compound of chlorine with a metal, on one hand, and of an oxygen acid with the oxide of the metal on the other, should be so similar in properties, that both must be classed as salts, and should give rise to a series of basic and acid compounds, for the most part completely parallel."—*Elements*, *p.* 681.

18. Upon the *similitude* and *complete parallelism* of the amphide and haloid salts, thus erroneously alleged, the author proceeds to argue in favor of the existence in the former, of compound halogen bodies, analogous in their mode of combination to chlorine or iodine.

19. I presume it will be granted, that if similitude in properties be a sufficient ground for inferring an analogy in composition, dissimilitude ought to justify an opposite inference. And

that if, as the author alleges, certain bodies have been classed as salts, on account of their similarity in this respect, when dissimilar they ought not to be so classed. Under this view of the question, I propose to examine how far any similitude in properties exists between the bodies designated as salts by the author, or any other chemist.

20. The salts, hitherto considered as compounds of acids and bases, are by Berzelius called amphide salts, being produced severally by the union with one or other of his amphigen class, comprising oxygen, sulphur, selenium, and tellurium, with two radicals, with one of which an acid is formed, with the other a base. The binary compounds of his halogen class, comprising chlorine, bromine, iodine, fluorine, and cyanogen, are called by him haloid salts. I shall use the names thus suggested.

21. Among the haloid salts we have common salt and Derbyshire spar; the gaseous fluorides and chlorides of hydrogen, silicon or boron; the fuming liquor of Libavius; the acrid butyraceous chlorides of zinc, bismuth, and antimony; the volatile chlorides of magnesium, iron, chromium, and mercury, and the fixed chlorides of calcium, barium, strontium, silver, and lead; the volatile poison prussic acid, and solid poisonous bicyanide of mercury, with various inert cyanides like those of Prussian blue; likewise a great number of ethereal compounds.

22. Among the amphide salts are the very soluble sulphates of zinc, iron, copper, soda, magnesia, &c., and the insoluble stony sulphates of baryta and strontia; also ceruse and sugar of lead; alabaster, marble, soaps, ethers, and innumerable stony silicates, and aluminates. Last, but not among the least discordant, are the hydrated acids, and alkaline and earthy hydrates.

23. When the various sets of bodies, above enumerated, as comprised in the two classes under consideration, are contemplated, is it not evident that, not only between several sets of haloid and amphide salts, but also between several sets in either class, there is an extreme discordancy in properties; so that making properties the test, would involve not only that various sets in one class could not be coupled with certain sets in the other, but, also, that in neither class could any one set be selected as exemplifying the characteristics of a salt, without depriving a majority of those similarly constituted, of all pretensions to the saline character?

24. Now, if among the bodies above enumerated, some pairs of amphide and haloid salts can be selected, which make a tolerable match with respect to their properties, as in the case of sulphate of soda, and chloride of sodium, while in other cases there is the greatest discordancy, (as in the stony silicate felspar, and the gaseous fluoride fluosilicic acid gas; as in soap and Derbyshire spar; as in marble and the fuming liquor of Libavius, the sour protochloride of tin, and sweet acetate of lead,) is it reasonable to found an argument in favor of a hypothetical similitude in composition, on the *resemblance* of the two classes in properties? Does not the *extreme* dissimilitude in some cases, more than countervail the limited resemblance in others? And when the great variety of properties displayed both by the amphide and haloid salts is considered, is it a cause for wonder or perplexity, that in some instances, amphide salts should be found to resemble those of the other kind?

25. Again, admitting that there was any cause for perplexity agreeably to the old doctrine, is there less, agreeably to that which is now recommended? Is there no ground for wonder that oxygen or sulphur cannot act as simple halogen bodies? By what rule are their binary compounds to be excluded from the class of haloid salts? Wherefore should chlorides, bromides, iodides, and fluorides, however antisaline in their properties, be considered as salts, while in no case is an oxide, a sulphide, selenide or telluride to be deemed worthy of that name.

26. I challenge any chemist to assign any good reason wherefore the red iodide of mercury is any more a salt than the red oxide, or the protochloride is more saline than the sulphide: or why the volatile oxides of osmium or of arsenic are less saline than horn silver or horn lead; or the volatile chloride of arsenic, than the comparatively fixed sulphides of the same metal: why gaseous chlorohydric acid is more saline than steam or gaseous oxhydric acid.

27. It much surprises me, that when so much stress is laid upon the idea of a salt, the impossibility of defining the meaning of the word escapes attention. How is a salt to be distinguished from any other binary compound? When the discordant group of substances which have been enumerated under this name is contemplated, is it not evident that no definition of them

can be founded on community of properties? and, by the advocates of the new doctrine, composition has been made the object of definition, instead of being the basis; thus, agreeably to them, a compound is not a salt, because it is made of certain elements; but, on the contrary, an element, whether simple or compound, belongs to the class of salt radicals, because it produces a salt. Since sulphur, with four atoms of oxygen, SO^4, produces a salt with a metal, it must be deemed a salt radical.

28. In proof that the double chlorides are not united in a way to justify the opinion adopted by Bonsdorff, Thomson, myself, and others, it is alleged by Graham, "*that in such compounds the characters of the constituent salts are very little affected by their state of union.*"

29. This allegation being, in the next page, admitted to be inapplicable in the case of the double cyanides; an effort is made to get over this obstacle, by suggesting the existence of another compound radical. But the allegation of the author is erroneous as respects various double haloid salts, especially the fluosilicates, the fluoborates, fluozirconiates, the chloroplatinates, chloroiridiates, chloroosmiates, chloropalladiates, &c., all of them compounds in which the constituent fluorides and chlorides exist in a state of energetic combination, by which they are materially altered as to their state of existence.

30. Evidently the word salt has been so used, or rather so abused, that it is impossible to define it, either by a resort to properties or composition; and I conceive, therefore, that to make it a ground of abandoning terms which are susceptible of definition, and which have long been tacitly used by chemists in general, in obedience to such definition, would be a "*retrograde movement in the science.*" I hope Dr. Kane will pardon me for employing the language to which he has resorted, in speaking of the opinions of Bonsdorff.

31. If this doctrine, as it has been stated, is to prevail, I do not perceive how it is to be prevented from claiming an inconvenient extension. The hydrates, as well as the sulphates, must have pretensions to contain salt radicals. Hence in the hydrated alkalies and alkaline earths, there would be a compound radical, consisting of hydrogen, with two atoms of oxygen, hydroxion, and these compounds would be hydroxionides; nor can I con-

ceive that the haloid compounds, erroneously called double salts, but more correctly considered as single salts, can be exempted.

32. Between the reaction of fluoboric acid with fluobases, and sulphuric acid with oxybases, is there not a great resemblance?

33. I am unable to understand how, if the existence of salt radicals in oxysalts be inferred, the other salts of the amphigen class can be exempted from a corresponding inference. But if the existence of salt radicals in the double sulphides be admitted, can it be consistently denied that they exist also in double chlorides, iodides, &c.? Is there not the greatest analogy between the habitudes of sulphur, selenium, and tellurium, with metals, and those of the halogen bodies?

34. Would not the modification of the ethereal oxysalts, to comport with the new hypothesis, be disadvantageous, both as respects our mental conception of those compounds, and the names which would be rendered appropriate? Would not the transfer of the oxygen from the ethereal oxide to the acid, and the creation, thus, of new salt radicals for the organic acid salts, be objectionable; such as oxyoxalion for oxalates, oxytartarion for tartrates, oxyacetion for acetates; while, for their compounds, we should have oxyoxalionides, oxytartarionides, oxyacetionides, &c.?

35. If sulphates are to be considered as oxysulphionides, by what names are we to designate the sulphites, hyposulphites, and hyposulphates, SO^2, S^2O^2, S^2O^5? SO^3 may, perhaps, with more propriety be considered as consisting of a compound radical, SO^2, and oxygen, forming an oxide of sulphurous acid; but in a sulphite, anhydrous sulphuric acid, SO^3 becomes a species of oxysulphion itself, being as much the oxysulphion of the sulphites, as SO^4 is of the sulphates. Of course SO^3 should have a direct affinity for radicals, contrary to fact. I presume that sulphites would have to be trioxysulphionides; hyposulphites, sesquioxysulphionides; sulphates, quadroxysulphionides; while the hyposulphates would, I suppose, be demiquintoxysulphionides!!!

36. Analogous complication in nomenclature would arise in respect to the nitrites and nitrates, phosphites and phosphates, arsenites and arseniates; also as respects the carbonic and oxalic acids.

37. It is true that nature has not so made her bodies as that they can be separated into classes, between which any distinct line

can be drawn, still it has been found advantageous to classify them to the best of our power. Accordingly it appears to me expedient, in the first place, to distinguish elements (or those compounds which act like them) according to their electro-chemical relations to each other, or their habitudes with the voltaic electrodes. Consistently, chemists have tacitly adopted the plan of treating the compounds formed by electro-negative elements with anions, as acids; those formed with cathions, as bases; while the combinations formed by the union of such acids and bases have been considered as simple salts. Thus four classes are constituted, consisting of electro-negative elements, of acids, bases, and single salts, while, by the union of the latter, a fifth class of double salts is formed. Whether the words acid, base, and salt, be adhered to, objectionable as they are in some respects, and especially the latter, or some others be contrived, it would seem to me disadvantageous to merge them in one name, pursuant to the views of the advocates of salt radicals, as stated by Gregory in his edition of Turner's Chemistry, 572.

38. The objection, that not being electrolytes the relation of acids and bases to the voltaic electrodes cannot be discovered, is easily remedied; since, on the union of a common ingredient with an anion and a cathion, there cannot be any doubt that the resulting compounds will have the same electro-chemical relation as their respective heterogeneous ingredients; so that, with the anion, an acid or electro-negative body will be formed; with the cathion, a base or electro-positive body. Moreover, as respects organic compounds which cannot be subjected to the electrolytic test, whatever saturates an inorganic acid must be a base, and whatever saturates an inorganic base must be an acid.

39. The word salt, I have shown, is almost destitute of utility, from the impossibility of defining it, and the amplitude of its meaning. A word that means every thing, is nearly as useless as that which means nothing.

40. As respects the three phosphates of water, PO^5+HO, PO^5+2HO, PO^5+3HO, the argument used by Dr. Kane cuts both ways; although, by its employer, only that edge is noticed which suits his own purpose. It is alleged that the difference of properties, in these phosphates, is totally inexplicable upon the idea of three degrees of "hydration;" but that all difficulty vanishes, when they are considered as three different compound salt

radicals, oxyphosphionides of hydrogen, PO^6+H, PO^7+2H, PO^8+3H.

41. To me the formation of three compound elements, by the reiterated addition of an atom, of which five of the same kind were previously in the mass to which the addition is made, seems more anomalous, mysterious, and improbable, than the existence of three compounds of phosphoric acid with water, in which the presence of the different proportions of water is the consequence of some change in the constitution of the elements, which is referred to isomerism.

42. No reason can be given why the addition of *one*, *two*, and *three atoms* of *oxygen*, to the "radical," should convey a power to hold a proportional number of atoms of hydrogen. Such an acquisition of power is an anomaly.

43. In the case of radicals formed with hydrogen in different proportions, as in acetyl and ethyl, formyl and methyl, the number of atoms of oxygen in the peroxides, is the inverse of the hydrogen in the radical.

44. Ethyl, C^4, H^5, unites, at most, with one atom of oxygen, while acetyle, C^4, H^3, takes three atoms to form acetic acid, C^4, H^3, O^3. Methyl, C^2, H^3, forms, in like manner, only a protoxide, while formyl, C^2, H, takes three atoms to constitute formic acid.

45. Besides the three oxyphosphions, of which the formulas are above stated, there would have to be another in the phosphites; so that instead of the hydrated acid, or phosphite of water, being PO^3+HO, it would have to be PO^4+H, a fourth oxyphosphionide of hydrogen.

(*To be concluded.*)

Art. VII.—*On the Rotary Action of Storms;* by Charles Tracy.

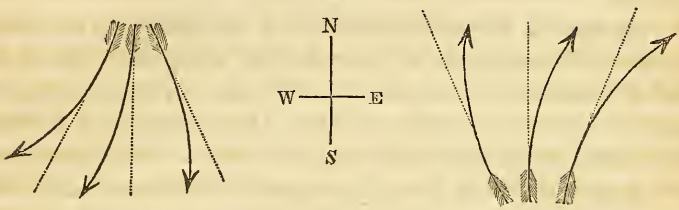
(Read before the Utica Natural History Society.)

The investigations of Mr. Redfield and Col. Reid have accumulated a vast amount of evidence in favor of the propositions they maintain. The tendency of this evidence is to demonstrate, that in the large storms which affect extensive districts, and also in the violent tornadoes which devastate a brief path, there are two motions, the rotary and the progressive; and that the rotary is by far the most violent, and has an uniform direction of revolution, being from right to left if the storm is in the northern hemisphere, and the reverse if it is in the southern hemisphere. That is to say, on our side of the equator the rotation is about the centre through the points of compass, in the order of N. W. S. E., or contrary to the movement of the hands of a watch lying on its back; and south of the equator the rotation is through the points in the order of N. E. S. W., or conformable to that of the hands of a watch.

These propositions, although authorized by induction, have encountered doubts or gained a feeble faith in many minds, for the want of a good cause to assign for the production of the alleged phenomena. Hence the occurrence of rotary storms, and the uniformity of direction of revolution, have been too readily attributed to mere accident; and the notion that a whirlwind, once started by mere chance, contains the elements of growth and stability of motion, has been too easily admitted. An active whirlwind, great or small, undergoes a constant change of substance. As the central portions waste into the ascending column, supplies from the adjacent tranquil air must be drawn into the vortex and set in motion; and if the fresh air is neutral to the circular movement and must acquire velocity from the whirling mass itself, then since "action and reaction are equal and in opposite directions," the whirling mass itself must lose just so much velocity as the fresh supply gains. By such a process the forces of the whirlwind would be rapidly exhausted, and its existence must speedily cease. A stable source of momentum, adapted to originate and sustain the uniform rotary movement, is still required: and it is now proposed to develop such a source of momentum in the forces generated by the earth's diurnal revolution.

The velocity of the earth's surface in the daily revolution being at the equator more than one thousand miles an hour, in latitude 60° half as much, at the pole nothing, and varying in intermediate places as their perpendicular distances from the earth's axis, and the atmosphere near the ground every where taking in part or wholly the motion of the surface it rests on, important consequences upon aërial currents must follow. A body of air set in motion from the equator northward maintains the equatorial eastward velocity, and when it passes over regions of slower rotation deviates eastward from the meridian, and ultimately describes over the earth's surface a curved line bearing towards the east. A current of air from latitude 45° north, having a due south direction, soon reaches regions moving faster to the east, falls behind them and describes a curve to the west. Winds oblique to the meridian are similarly affected. These familiar matters are referred to here, and illustrated by figure 1, to elucidate what follows.

Fig. 1.



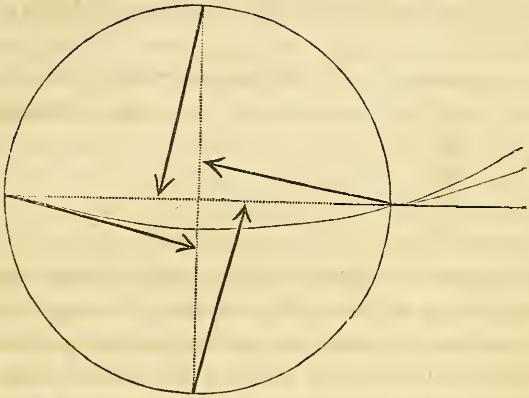
The influence of the figure and revolution of the earth upon east and west winds, must also be considered. A parallel of latitude, being a lesser circle of the globe, and at all points equally distant from the pole, necessarily describes upon the earth's surface a curved line. But a direct course, due east at the commencement, follows a great circle, and parting from the parallel reaches a lower latitude. The due east course continued in a right line describes a tangent to the curve of the latitude. The velocity of the earth's surface at any place, by virtue of the diurnal revolution, has for its direction the line of that tangent; and when the air reposing over any spot is transferred to a region of diverse motion, the *direction*, as well as the *degree*, of its previous force is to be taken from that of the soil on which it previously rested. Hence a wind from due west, if in our hemisphere, will soon be found pursuing a southeasterly course, and crossing successive parallels of latitude.

The labors of Mr. Espy have been directed to the hypothesis of a central ascending column of rarefied air, and centripetal currents from every side rushing towards its base. Without pursuing his reasoning, it will be safe to assume that his collection of facts established the existence of a qualified central tendency of the air, in both the general storms and the smaller tornadoes.

He presents a theory to account for such motion, which it is not necessary now to examine. Dr. Hare has proposed another method of accounting for tornadoes—a truly brilliant suggestion—of which it is only to be remarked, at present, that it proceeds on the assumption of a rush of air from all quarters to a central point. It has been attested also, that at large clearing fires in calm weather, creating centripetal currents, the whirlwind and mimic tornado have been produced. In accounting for the whirling motion therefore, the central tendency of the air will be presupposed.

In the case of a large fire kindled in an open plain on a calm day, a small circle about the fire is first acted on by the abatement of pressure on the side next the fire, and thus receives an impulse towards the common centre. As this moves in, the next outer circle loses support and begins to move. Each particle of air is moved at first by an impulse towards the centre, and during its approach to the central region it receives fresh impulses of the same direction; and if it comes from some distance its velocity is in this way accelerated, until it reaches the space where the horizontal is broken by the upward motion. It is obvious that particles propelled by such impulses would seek the common centre in the lines of its radii, and their horizontal forces would be neutralized by impact, if no cause for deviation was at hand. But the great law of deflection which affects the course of the winds, applies to the movements of these particles. The particles which seek the centre from the northern points are deflected west, while those from southern points are deflected east. The whole rush of air from the northern side of the centre, coming like a breeze, bears west of the centre, while an equal breeze from the southern side bears east of the centre. The consequence is that the central body of air, including the fire, is acted upon by two forces which combine to make it turn round to the left. These forces are aided by the deviation of the currents from the easterly and westerly parts of the circle. The breeze from the west extreme inclines to the tangent of the parallel of latitude at its original place of repose, and therefore strikes south of the centre, into which the impulses it receives would otherwise carry it. The air from the east side also inclines toward the tangent of the parallel of latitude there, which is oblique to the north from the radius, and therefore is deflected northwards and strikes north of the centre. The breezes from all quarters thus co-operate to produce the result; and all their forces are constant, and act with precision and at great advantage to cause and maintain a whirlwind. A diagram presenting the lines of approach of the particles or streams of air, will explain this result. The black lines in figure 2, show the deviating currents, from the cardinal points alone, when the area affected by the fire is so small as to require no perceptible curve in those lines.

Fig. 2.



Upon the same principle, the tornado, the typhoon, and the wide-spread storm of the Atlantic, if their currents move towards a central spot, must have a rotary character. The circular motion in the outer portions may be slight, but it is stronger near the centre. In every such case the incoming air may be regarded as a succession of rings taken off the surrounding atmosphere, and moving slowly at first, but swifter as they proceed towards the centre. Each such ring is affected by the law of deviation during its passage. The particles are veering from the radii, in its northern quarter westward, in its southern quarter eastward, in its eastern quarter northward, and in its western quarter southward; and hence the ring begins to revolve when far from the centre, turns more and more as it draws near it, and finally as it gathers about the central spot all its forces are resolved into a simple whirl. Ring after ring succeeds, and the whirling action is permanent.

The deflecting power thus applied is not small. The rotary motion of the earth varies as the cosine of latitude, and the differences of velocity for any differences of latitude are easily computed. The following are samples; being differences of velocity for 1° or 69½ miles of latitude.

Between lat.	2°	and	3°	diff. of velocity	0.79	miles	per hour.
"	" 3°	"	4°	" "	1.11	"	"
"	" 10°	"	11°	" "	3.31	"	"
"	" 23°	"	24°	" "	7.25	"	"
"	" 42°	"	43°	" "	12.28	"	"

The differences of velocity for one mile, or 51.84″ of latitude, are as follows.

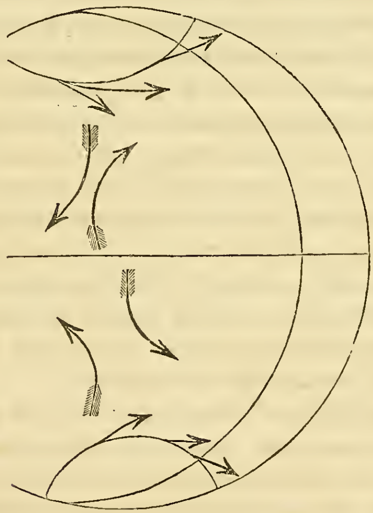
Latitude.	Difference of velocity for one mile north.
10°	4 feet per minute.
23°	9 " "
42°	15.4 " "
43°	15.7 " "
45°	16.3 " "

The deflection of easterly and westerly breezes by reason of the spherical form of the earth, also, can be computed; and it is obviously no less important than the deflection produced in meridional winds. The angle between the courses north and east, at any point, is a right angle; and if two points in the same latitude are taken, it is evident that the obliquity of the north courses from the two points, equals the obliquity of the east courses from the same points.

These results show that in the northern states a fire large enough to affect the atmosphere over a few acres may possess the essential force for generating a whirlwind, and may produce it in fact if the day be calm. A large storm, covering the whole country with its centripetal currents, must produce a vortex about the centre, which will combine the principal energies of the storm. The tornado and water spout must revolve with terrific violence.

The necessary condition, centripetal motion, may arise whenever a central spot subjected to intense heat is surrounded by a cool atmosphere. This state of things, on a small scale, may occur in a summer's day, upon a ploughed field surrounded by extensive pastures; upon a black and charred clearing in the midst of a cool forest; or at a large clearing fire. Upon a great scale—if an island beneath the tropical sun received upon rocks and sands the intense radiance of a succession of clear, calm, and hot days, and consequent sea breezes from the deep and cool ocean pressed in upon all its shores with the violence of a high wind, it should not cause surprise if these various breezes combined to generate a vast whirlwind; nor if the lofty revolving column should at last leave the place of its origin and traverse the sea, a hurricane. The cause which first excited the centripetal tendencies of the storm, might be renewed as the upper current of the atmosphere bore it over other heated spots; and the law of deflection will inevitably transform the central into circular motion. The destructive storms of our sea-coast may have such an origin among the eastern islands of the West Indies, from which they appear to proceed.

Fig. 3.



In the southern hemisphere the same law of deflection produces contrary results. There the wind which first moves north bends to the west, and the wind which moves south at first turns towards the east, that from the east turns south, and that from the west turns north. Figure 3 represents these effects. Hence south of the equator storms revolve from left to right, or conformably to the movement of watch hands. Figure 4 exhibits the rotary action of a storm in the northern hemisphere; figure 5 the same in the southern hemisphere.

Fig. 4.

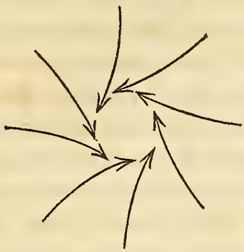
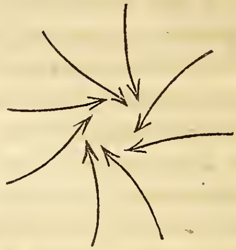


Fig. 5.



The relative motions of the parts of a small circular space on the earth's surface, by reason of the diurnal revolution, are precisely what they would be if the same circular space revolved upon an axis passing through its centre parallel to the axis of the globe. If such space be regarded as a plane revolving about such supposed axis, then the relative motions of its parts are the same as if the plane revolved about its centre upon an axis perpendicular to the plane itself; with this modification, that an entire revolution on the axis perpendicular to the plane, would not be accomplished in twenty four hours. Such plane daily performs such part of a full revolution about such perpendicular axis, as the sine of the latitude of its centre is of radius. The plane itself—the field over which a storm or a tornado or a water-spout is forming—is in the condition of a whirling table. Hence the tendency to rotary action in every quarter of the storm is equal, and all the forces which propel the air towards the centre coöperate in harmony to cause the revolution.

Water discharging from a broad basin through a central orifice, is subject to the same law. It forms a vortex which in our hemisphere turns to the left, or against the sun, and in the southern hemisphere must turn to the right or contrary to the sun there.

These rotations of the atmosphere and of water, being from west to east about lines inclined to parallelism with the earth's axis, are singularly coincident in direction with the rotation of the globe, and harmonize with the general mechanism of the heavens.

Utica, Feb. 27, 1843.

Art. VIII.—*Corrections and Additions to the Monography of Cuscutineæ, in Vol. XLIII. of this Journal;* by George Engelmann, M. D.*

A careful re-examination of this tribe during the past season, as well as the increased opportunity of examining specimens from different parts of North America, have discovered some errors, and made some corrections and additions necessary, which I should, indeed, prefer to withhold for the present, and subject to the test of another season's study, if it were not important to correct such errors as soon as possible. A fuller description of the new species, with figures, I defer to another time.

I am now convinced, that, although many Cuscutæ prefer some plants to others, yet there is no constancy in this respect, but the same species often grows upon a great variety of widely different plants. I did wrong, therefore, to name them from the genera upon which they grew; and I should much prefer to see the names of *C. Cephalanthi* changed into *C. tenuiflora*, *C. Coryli* into *C. incurva*, *C. Saururi* into *C. umbrosa*, Beyr.? *C. Polygonorum* into *C. chlorocarpa*, and *Lepidanche Compositarum* into *L. squarrosa*, if they had not yet been published.

I. Cuscuta, *Linn.*

1. Cuscuta Cephalanthi.—Mostly 4-parted; frequently only 3-parted.

2. Cuscuta Coryli.—Found in many places near St. Louis, on Hazel, Willow, Desmodium, Teucrium, Solidago, etc. The *long* styles observed in some *dried* specimens of this as well as other species, are the consequence of a continued vegetation in the plant-press! The variety β. must therefore be stricken out. Flowers frequently 5-parted.

3. Cuscuta vulgivaga.—Certainly the most common species. The stylopodium is very remarkable in the living specimens which I have examined; and the capsule is oval, even a little pointed, less globose than any other of our Cuscutæ; but I am not prepared to say that this is the case with all varieties of this

* The characters of the new species, &c. here described, have been published in the *London Journal of Botany* for April, 1843, as an appendix to the original monograph, there reproduced.—Eds.

very variable species. The stamens and pistils are as long, or rather a little shorter than the corolla, but the latter are elongated after flowering. (*Cuscuta Americana*, Hooker?)

4. Cuscuta Saururi.—It is very probable that *Cuscuta umbrosa*, Beyrich, ex Hooker, is the same; which name must therefore be substituted for mine, though not quite appropriate. This plant is very nearly related to the former species, but can always be distinguished by the more open, campanulate corolla, which in *C. vulgivaga* is globose-campanulate, the thinner texture of calyx and corolla, which is destitute of the pellucid dots, and the oblong lobes of calyx and corolla, which are always more or less orbicular in *C. vulgivaga*. Large, overgrown specimens of *C. vulgivaga* have sometimes the lobes of calyx and corolla as long as the tube, but can always be recognized by the above characteristics. Such specimens are those from Alabama and Texas, mentioned in this Journal, Vol. xliii, p. 340. The true *C. Saururi* I have only received from western New York, and from this neighborhood; where it grows in abundance on Polygonum, Saururus, etc. in a few localities.

I must mention here two specimens of a Cuscuta received from Mr. M. A. Curtis, collected, one in Massachusetts, the other in North Carolina. In their principal characters they agree with *C. Saururi*, but the flowers are much smaller and frequently 4-parted; the linear oblong, obtuse lobes of calyx and corolla are rather longer than the tube; the filaments subulate, shorter than the limb; ovary with a stylopodium; styles short and thick; capsule ?

An examination of more complete specimens and the living plants must show whether there is a constant difference between this eastern plant and the western *C. Saururi*. But I may here remark, that the eastern form of *C. vulgivaga* is also mnch smaller than our western form, and from Connecticut I have also received a tetramerous *C. vulgivaga!*

5. Cuscuta verrucosa.—Under this name I have confounded two Texan species: the description is chiefly taken from the following species, but the figure refers to this one, which was first collected by Drummond and afterwards by Mr. Lindheimer, both times on *Petalostemon multiflorum*. The description must be altered:—C. verrucosa, cymes umbelliform, compound; flowers peduncled (small), 5-parted; calyx campanulate, verrucose; segments ovate, somewhat obtuse, shorter than the globose-campa-

nulate tube of the corolla; lobes of the corolla long acuminate, somewhat longer than the tube; stamens half as long as the limb; scales ovate fimbriate, rather larger than the tube; ovary globose, depressed, without stylopodium; capsule depressed.—The tissue of the corolla is composed of large irregular cells.

6. Cuscuta hispidula, *n. sp.*—Stem low; cymes loose, few flowered, hairy or nearly smooth; flowers very long peduncled (small), 5-parted; tube of the corolla turbinate-campanulate, twice the length of the ovate subacute segments of the calyx, shorter than the long acuminate somewhat crenulate spreading lobes; stamens half as long as the limb; scales ovate, fimbriate, nearly equaling the tube; ovary with a stylopodium and short styles.

Texas, in dry and sterile prairies west of Houston. Flowering in April and May. Compare the remarks made in Vol. xliii, p. 341, under *C. verrucosa.*

7. Cuscuta neuropetala, *n. sp.*—Cymes umbelliform, smooth, flowers pedunculate (large), 5-parted; tube of the corolla campanulate, nearly equal in length to the ovate-lanceolate acute carinate segments of the calyx, and the ovate short-acuminate one-nerved crenulate spreading lobes; stamens rather shorter than the limb; scales ovate, fimbriate, incurved, as long as the tube; styles rather longer than the ovary with the stylopodium.

Texas, in wet prairies near Houston; on different Compositæ, such as Liatris, Solidago, Helianthus, Rudbeckia, and on Myrica cerifera; flowering in August; *F. Lindheimer.*

Flowers rather large, but variable in size; segments of calyx always very acute, ovate or ovate-lanceolate, somewhat shorter or a little longer than the tube of the corolla. Anthers yellow or purple; stigmas purple.

This and the last species resemble in the structure of the corolla the more northern *C. Coryli;* they have the same crenulated margin, the same fleshy cellular texture, similar incurved tips of the acute lobes, and the same white color, which is not altered in well-dried specimens.

C. neuropetala is distinguished from *C. hispidula* by its perfect smoothness, its flowers being twice or three times as large, its more compact, umbelliform cymes; the whole plant is taller, (in my specimens twelve to eighteen inches high.) The calyx segments, at least the three outer ones, are carinate; the lobes of the corolla are broader, shorter, composed of small linear cells, which are contracted in the middle into a distinct nerve. Stylopodium large in proportion to the ovary. Capsule not seen. The purple anthers and stigmas in the white flowers, give this species a very pretty appearance.

8. Cuscuta pentagona.—Capsule globose, somewhat depressed, without a stylopodium.

The description is taken from the Virginia plant; the forms from Illinois and Texas constitute two distinct varieties.

β. microcalyx: flowers shorter peduncled; calyx not remarkably 5-angled, much shorter than the tube of the corolla.—Illinois.

γ. calycina: flowers shorter peduncled; calyx not remarkably 5-angled, longer than the tube of the corolla, which is equal to the acute lobes.—Texas.

This species bears some resemblance to *C. Polygonorum* on one side, and to the three foregoing species on the other; to these by the acuminate lobes of the corolla, to the first by the depressed ovary and pale greenish-yellow capsule;* but it is distinguished from both by the orbicular lobes of the generally large and more or less pentagonal calyx. The inflorescence represents little umbels in *γ*, or approaches the glomerules of *C. Polygonorum* in *β.* and *γ*. The lobes of the corolla are acute, resembling in shape those of the following species, in the Texan variety; or longer and finely acuminate, (similar to *C. verrucosa* and *C. hispidula*,) in the more northern forms. Stamens short, only half the length of the limb; anthers nearly globose. Scales large, ovate, fimbriate, sometimes exceeding the tube. Ovary and capsule depressed.

This is probably the earliest species in North America; in Texas it has been found in bloom in April and May, and near Bardstown early in July; while here, one hundred miles further south, hardly any other species begins to open its flowers before the last days of that month.

9. Cuscuta Polygonorum.—Segments of calyx generally as long as the tube of the corolla, mostly subacute, but occasionally also somewhat obtuse; the corolla is thin, membranaceous, composed of a very fine cellular tissue; stamens broad at base, subulate; scales smaller than in any other species, except *C. Coryli.*

* In all other species which are here mentioned, it is of a brown color, except perhaps in *C. Cephalanthi*, where it is also light-colored.

II. Lepidanche.

Last autumn I discovered a second species of this genus, which imposes the necessity of altering the generic character. It must now read: Capsule 2-celled, 1–4-seeded.

The *facies* of the genus refers principally to the first species; the second has more the appearance of a Cuscuta, but the flowers are also closely sessile.

1. Lepidanche Compositarum.—Stems before flowering orange colored, soon decaying. (*Cuscuta glomerata*, Choisy, Mem. Soc. Nat. Hist. Genev., ex adnot. A. Gray.)

2. Lepidanche adpressa, *n. sp.*—Flowers sessile, glomerate, 5-parted; calycine scales seven to nine, imbricated, appressed, ovate or orbiculate, slightly crenulate, the outer ones the largest; tube of the corolla cylindric, a little longer than the calyx, twice as long as the oblong obtuse spreading lobes; stamens shorter than the limb; scales laciniately pinnatifid, convergent, covering the ovary; ovary with the stylopodium equaling the styles; capsule globose, shortly acuminate, covered by the marcescent corolla; 2–4-seeded. (*Cuscuta compacta*, Choisy, l. c., ex adnot. A. Gray. *C. coronata*, Beyr. ap. Hook.?)

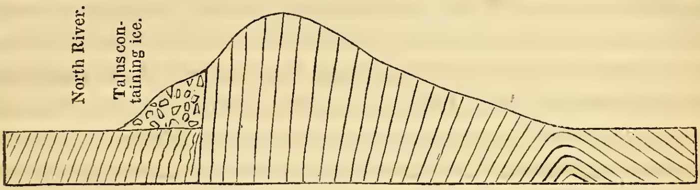
I discovered this species last autumn, in the fertile shady woods on the banks of the Mississippi, amongst a most luxuriant growth of vines and underbrush, on Bignonia radicans, Rhus toxicodendron, Laurus Benzoin, Vitis, Cornus, etc. Choisy describes it from specimens collected in Alabama.

The flowers are closely sessile, but distinct, and not in such dense clusters as in *L. Compositarum*. The glomerules either form a continuous line round the stem of the parent plant, or they are separate, consisting of from five to ten or more greenish white flowers. The filiform stout stems are whitish, and do not entirely disappear at the flowering time. The capsules are generally 2-seeded; but as they are not so crowded as in the other species, they are also found 3–4-seeded.

Plate VI, Vol. xliii.—The tube of the flower, fig. 4, ought to be a little shorter. The lobes of the corolla, fig. 18, are too wide at base; they should be more oblong. The ovary, fig. 24, should be depressed like that in fig. 28. The calyx-segments ought to be marked in fig. 25.

Art. IX.—*On the Ice Mountain of Hampshire County, Virginia, with a proposed explanation of its low temperature;* by C. B. Hayden.

A mountain possessing a temperature so independent of all external causes, as to *permanently preserve ice*, within a few inches of its surface, unaffected by the vicissitudes of the seasons, or the diurnal variations of temperature, was too singular and striking a phenomenon, not to have early attracted observation. The Ice Mountain has hence received frequent notice, but of so indefinite and frequently exaggerated a character, as to fail to produce a general belief in its existence, or to secure it that interest which this rare curiosity so richly merits. The Ice Mountain is one of the subordinate ridges of the Cacasson Mountains, and is a continuation of the North River Mountain; the latter consists chiefly of sandstones, and constitutes the western portion of an anticlinal axis, which at its commencement, many miles south of the Ice Mountain, is low and symmetrical. As this axis proceeds north it becomes more developed, and loses its symmetry, the rocks on the western side having a much greater inclination than the corresponding ones on the eastern. This inclination of the rocks, constituting the western side of the axis, rapidly increases with its development, until they become perpendicular, and form a distinct ridge, which in its continuation forms the Ice Mountain. It rises to the height of seven or eight hundred feet, forming a mural precipice, whose cragged summits split and rent, shoot suddenly up into sharp turreted spires, or jagged pinnacles, resembling the battlements of a Gothic castle, or the minarets of a mosque. At other times, losing this wildness, it is as remarkable for its singular symmetry, as before for its fantastic irregularity. Still retaining its precipitousness, it rises to the height of several hundred feet; its uniform summit, and rude massive symmetry, its steep rocky sides, devoid of vegetation, save where some stinted pine has "cast anchor in the rifted rock," all combine to give it the character of a huge Cyclopean wall. This singular structure has been thus minutely described, both from the unique and imposing scenery to which it gives rise, and from the connexion it is supposed to have with the phenomenon of the Ice Mountain. At the Ice Mountain, the steepness and walled structure is retained, and the mountain forms an abutment or support to an enormous glacis or bank of rocks, which is thrown up against it on its western side. The following section, without pretending to topographical accuracy, will show the structure of the mountain and the relative position of the talus heap containing the ice.



Ice Mountain.

This natural glacis lies along the direction of the mountain, reaching high up towards its summit, and extending laterally several hundred feet from its base; the debris consists of fragments of sandstone, varying in size from a few inches to many feet in diameter, loosely heaped together, and from their irregular angular shape generally separated by large interstices. The main ridge seen in the section is known as the Ice Mountain, though it is only in the interstitial cavities of the talus, that the ice is formed and preserved.

The Ice Mountain was visited by the writer in the summer of 1838, a season memorable in the annals of western Virginia for its long and distressing drought, so fatal to the crops. The heat of this season, though unparalleled in that region for duration and intensity, but slightly affected the temperature of the Ice Mountain, as ice was found in great abundance by the writer, by removing the rocks to the depth of a few inches. A thermometer on being introduced into one of the cavities between the rocks, so as to be exposed to the air without being in contact with the rock, rapidly sunk to below 40°, and would doubtless have been still further depressed had it been permitted to remain. The general low temperature of the rocks was evinced by the moisture which either bedewed their surface, or trickled from their sides; the result of the condensation of the atmospheric vapor by the low temperature of the rocks, although at the time, the dew point must have been extremely low. During the previous winter, the rocks had been removed from a portion of the heap, to the depth of three or four feet, and the cavity thus formed filled with snow, and loosely covered with planks, but so slightly that the snow could be seen through the crevices of the covering; but though so imperfectly protected from atmospheric agencies, the snow exhibited not the slightest traces of the heat of the past summer, and was as dry, friable, and crystalline, as if new fallen. The dairy mentioned by Kerchival,* has three of its sides surrounded by the heap of rocks, and hence partakes of the low temperature of the mass. The sides of the dairy were not however, as in ordinary seasons, encrusted with ice, nor were icicles pendent from its roof, but its temperature was still sufficiently low to subserve all the purposes of a dairy and refrigerator. The temperature of the spring which issues from the base of the talus is unaffected by the temperature of the overlying mass, and though reputed to be but slightly above the freezing point, is in reality but one degree lower than the springs of the vicinity, and no lower than some others in the same county, which vary from 51° to 52°. The scene, as viewed from the base of the mountain, was as interesting as paradoxical. On the one hand was the North River converted into a stagnant pool, its indurated bottom exposed at short intervals—the drooping foliage of the forest, the blighted grain, tinged not with autumn's golden yellow, but a sickly hue, denoting that it had prematurely fallen into "the sere and yellow leaf"—all too plainly indicating the long continued action of summer's heat. On the other hand was a mass of rocks below the freezing point, enclosing in its cavities snow and ice, while the spectator himself enjoyed an atmosphere whose bland, spring-like softness formed an agreeable contrast to the distressingly hot one, (96°,)† for which it had a few minutes before been exchanged.

Having thus given a detailed description of the Ice Mountain, it may not be uninteresting to inquire into the causes which give it a temperature so singularly independent of all those influences which usually determine the temperature of terrestrial bodies—a temperature upon which the summer's heat, neither in ordinary, nor in unusually long, and intensely hot seasons, exerts the slightest influence. The solution, I conceive, is to be found in the large and unusual collection of rocks, which from their porous

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* Kerchival's History of the Valley of Virginia.

† The temperature a few moments before ascending the mountain, at 2½ P. M., was 96° in the shade.

homogeneous texture are extremely poor conductors of heat. By reference to the description and section, it will be seen that on one side is the mountain, consisting of a massive wall many hundred feet in thickness, and heaped up against this as an abutment, a mass of rocks containing several thousand cubic feet. As the mountain has a general direction from N. E. to S. W., the talus heap containing the ice has a N. W. exposure. The cavernous nature of this heap would admit the free entrance of atmospheric waters, which during the winter would form ice in the interior of the mass. The ice thus situated would be protected from external heat by the surrounding rocks, as ice in a refrigerator is isolated and protected from the external temperature, by the non-conducting sides of the refrigerator. The Ice Mountain only requires for the explanation of its phenomenon, the application of the familiar principle upon which is constructed the common refrigerator, which temporarily effects what the Ice Mountain permanently does—a temperature independent of external causes. The Ice Mountain is in fact a huge sandstone refrigerator, whose increased and unusual effects beyond those of the ordinary refrigerator, are due to the increased and unusual collection of poor conducting materials which form its sides.

Similar, though inferior accumulations to that of the Ice Mountain, from geological causes, frequently occur in Hampshire, and the adjoining counties. Observation showed them in every instance to have a temperature far below that of the atmosphere. That this low temperature is permanent, is proved by the universal custom of individuals residing in their vicinity so constructing their dairies, that three of their sides are enclosed by the rocks, in the same manner as the one already described at the Ice Mountain. Even a thin layer of poor conducting materials, affords a much greater protection than would be anticipated by those whose attention has not been given to the subject. The means resorted to by the shepherds of Mount Etna, for supplying their flocks with water, exhibits the protecting influence of a bad conductor. The shepherds during the winter, cover the snow with a layer of volcanic sand and ashes, a few inches in thickness, which protects it from the sun, and preserves it throughout the summer, thus affording them an abundant supply of water for their flocks, where it could be obtained from no other source.

A still more interesting and striking proof of the perfect isolation from external causes, by a poor conducting covering, is attested by the fact, that a large glacier of ice and snow was overflowed by a stream of hot lava from Mount Etna, without being destroyed.* The ice thus covered by the lava, was protected by it from the summer's heat, and continues thus preserved to the present day. This can only be explained by supposing that the lower portion of the lava current, immediately upon its contact with the ice, was reduced to the temperature of the glacier, and that this reduced stratum, from its imperfect power of conducting heat, protected the ice from the hot lava above. Whatever may be the explanation of it, or however paradoxical it may appear, the fact is attested by too high authorities to be doubted. Public attention was first called to this interesting fact in 1828, when the discovery was made by Signor Gemmellaro, in searching after ice. It has been subsequently examined by Lyell and other distinguished geologists, who confirm the report of Signor Gemmellaro. Excavations made for removing the ice, have exposed the lava for several yards, overlying the glacier, and so superimposed, that the relative position of the lava and glacier can only be accounted for by supposing that the latter was overflowed by the former in a melted state. *Monte Testaceo* may be instanced as presenting a phenomenon more strictly parallel with that of the Ice Mountain, and as affording a happy illustration of the principle so frequently alluded to. Monte Testaceo is situated in one of the suburban *riomi* of Rome. It is merely a large mound, composed of fragments of earthenware vases and urns, and is supposed to mark the site of an extensive ancient pottery. This accumulation of bad conducting materials preserves a uniform temperature, many degrees below the main temperature of Rome, and on this account artificial cavities formed by digging in the sides of the mound, are used as wine vaults. In July, 1773, Prof. Pictet found by observation, the temperature of one of the caves to be 44°, while that of the external atmosphere was 78°.† If this comparatively small accumulation produces so great a depression in Rome, where the mean temperature is 60°, it can be readily conceived that the still greater accumulation at the Ice Mountain,

* Lyell's Principles of Geology, London edition, Vol II, p. 124.
† Edinburgh Philosophical Journal.

would reduce the temperature to 32°, in a climate where the mean temperature is but 52° or 53°.*

In endeavoring to explain the low temperature of the Ice Mountain, the effect resulting from the bad conducting nature of the mass, and its protection by similar materials on all sides except the N. W., have alone been considered. The nature of the rocks as absorbents of heat should also be estimated, as from their dull white color, most of the heat would be reflected, leaving but a small portion to be absorbed. It should also be borne in mind, that the air immediately in contact with the ice would be, from its lower temperature, specifically heavier than the external atmosphere, except in midwinter, and could only be replaced by an atmosphere heavier than itself, and therefore colder. It hence follows that the ice could only be affected by the hot air of summer, so far as its heat is conducted by the surrounding rocks, which, as will appear from the foregoing explanations, must be very inconsiderable.

Art. X.—*On the Errors of Chronometers, and explanation of a new construction of the Compensation-balance;* by E. J. Dent.†

It must, doubtless, be interesting to the public in general to have the opportunity afforded them of noticing the various statements of *reported improvements* in chronometers, that are, from time to time, set forth by their respective inventors. Such accounts moreover answer the desirable and double purpose of registering the several ingenious contrivances, as well as of exhibiting in a clear light the nature of the difficulties usually encountered in this important branch of the mechanical arts. By such a work, too, the public obtain a more distinct knowledge of the subject, and at the same time receive a more attractive idea of human ingenuity striving to attain mechanical perfection. It must be confessed, however, that the result of the skill, labor, and expense which have been bestowed within the last fifty years on the improvement of chronometers, affords but little room for congratulation, and must convince every one acquainted

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* Deduced from observations on the temperature of the springs of that region.

† Communicated by the author.

with the historical details of the subject, that the road to perfection in the art of chronometer-making is, as in most other arts, a wearisome one, more frequently leading to profitless trouble, than contributing either to the interest of the contriver or the benefit of the public. Nevertheless, by such investigations has been obtained the knowledge of a curious fact, which has lately excited the attention and ingenuity of various persons engaged in the manufacture of chronometers.

The fact alluded to is this—that if chronometers, as generally constructed, be regulated to mean time at *mean* temperature, the chronometer will *lose* at the extremes of heat and cold; or, if adjusted to keep mean time at the extremes, they will have a tendency to gain at the intermediate temperatures.

This fact, although in all probability known to others, was first pointed out by myself in No. 14 of the Nautical Magazine, in the year 1833, but I am not aware that the slightest hint has ever yet been given as to its *true cause.* In order to explain it, we must bear in mind, that no chronometer can keep a uniform rate, unless the tension, or moving force of the balance-spring, has an invariable ratio to the resistance of the inertia. Now in chronometers, as usually constructed, this ratio cannot, from the nature of the construction of the balance, be maintained at different temperatures; since the tension of the balance-spring, when influenced by a change of temperature, varies according to a law different from that observed in the simultaneous variation of the inertia. We cannot, indeed, assign with any great precision the law which connects the tension of the balance-spring with the temperature. That the force of tension, however, varies very nearly as the temperature, within ordinary limits, may be seen from the following experiments made with a chronometer having a glass disc for the balance, and a balance-spring of hardened and tempered steel.

Thermometer.	Hourly rate.	Number of vibrations in one hour.
32	+ 5.74	3605.74
66	− 1.80	3598.20
100	−10.30	3589.70

Now since the force of tension of the balance-spring (the inertia and friction remaining the same) varies as the square of the number of vibrations made in the same period, we have the following results from the above, taking the force of tension at 32° to be unity.

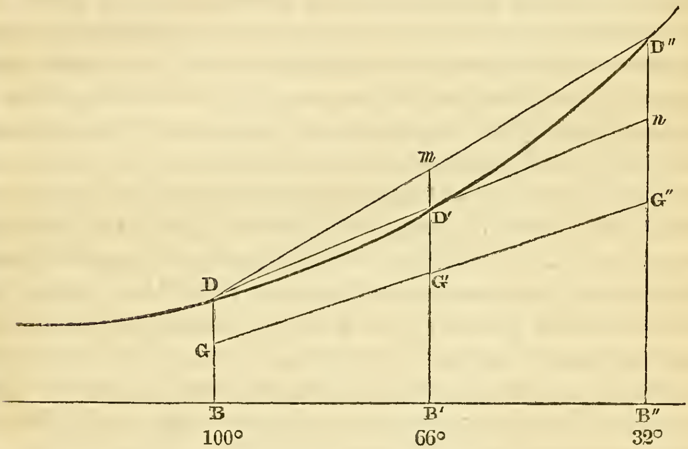
Thermometer.	Tension of balance-spring.
32	1.0000
66	0.9958
100	0.9911

Thus the experimental tension at the mean temperature of 66° Fahrenheit is 0.9958; and the tension computed upon the supposition that it varies as the temperature, is 0.9956; differing only by the quantity .0002th part of the whole force, corresponding to about 2° of the thermometer, which, considering the difficulty experienced in maintaining an equality of temperature, in the individual experiments, is not a greater difference than might be reasonably expected, in all probability therefore the tension varies nearly as the temperature, within ordinary limits; but with regard to the variation in the inertia, we know that the effect produced by the compensating weights, by their approach and recession from the centre of the balance, varies as the square of the central distance; and therefore it is not to be wondered at, that the required ratio betwen the tension and inertia should occur only at two temperatures: nor is it surprising that when chronometers are regulated for mean temperatures only, they should lose at the extreme ones; since in the case of an *increase* of temperature, the approach of the weights to the centre is not sufficiently great to effect the compensation, and in the case of a *decrease* of temperature their recession from the centre is too great to compensate for the increased rigidity of the balance-spring. It is true, that this law of variation in the inertia applies only to *each particle* of the balance in reference to its distance from the centre of motion, and not to a mass, unless referred to the centre of gyration; and as the whole inertia of the balance is made up of the inertia of the fixed arms, as well as the movable compensating weights and rim, it is plain that any attempt to exhibit by computation the variation of the whole inertia due to a change of temperature, would involve not only a consideration of the figure of the balance, but also a knowledge of the law of variation in the central distance (as depending upon temperature) of the weights and rim, of which we are at present more in ignorance than of the law that exists between the temperature and the tension of the balance-spring. The inertia of the balance is a more complicated function of the temperature than the tension of the balance-spring, and involves a higher power of it: and this is still a source of difficulty.

Another circumstance that tends to aggravate the error arising from the defect of compensation for the diminished tension of the balance-spring at high temperature, and the excess of compensation for the increased tension at low ones,—is, the unfolding or straightening of the circular rim of the balance at reduced temperatures, and the contrary action at high ones. By this action of the rim, the compensating weights are made to describe portions of a spiral curve, whereby the variations in the central distance, due to a given change of temperature, are greater at the low than at the high temperature, which is the reverse of what is required in order to effect the compensation; and although such deviations from the required *law of approach* of the compensating weights may be rendered less apparent by increasing the weights, yet, in this case, other errors are introduced (which it will be needless here to allude to) that render this mode of proceeding inadmissible without much limitation. In the construction of the balance I shall here describe, it is not pretended, indeed, that the *law of approach* is *mathematically* what it ought to be, in order that the proper ratio may be obtained at all temperatures between the tension of the balance-spring and the inertia of the balance,—yet it may be safely affirmed that, in this construction, the variations in the central distance of the weights increase at the higher and diminish at the lower temperatures; which is exactly *the reverse* of what has hitherto generally taken place in chronometers, and therefore will doubtless afford a much nearer approximation to the truth than heretofore attained. Moreover, the correction of the error alluded to, will be a *continuous correction;* an object of no little importance, and which is not effected in the contrivances lately put forth to remedy the defect by means of *supplementary weights*, which weights are brought into contact with the balance rim at a mean temperature. In these contrivances by *contact*, although chronometers may be adjusted to equal rates at one of the extremes, and also at a mean temperature, yet between these limits, they are obviously subject to an error of the same nature as before, though of one half the amount only; and in the other half of the range of temperature, when the supplementary weights are brought into contact with the rim of the balance, the law of approach is the reverse of what it ought to be. Besides, the friction at the point of contact is highly objectionable in this mode of correction, and will not only destroy all confidence in the performance of such chronometers at mean temperatures, (the very temperatures at which their services are most required,) but it is also a gross violation of the law of continuity, upon the maintainance of which, the correct performance of chronometers must depend.

In order that what I have stated with respect to chronometers of the usual construction may be the more apparent, we will, for the sake of illustration, suppose the tension of the balance-spring to be in proportion to the temperature; then in the accompanying figure, let B B′ B″ be a scale of equal parts, and representing the scale of a thermometer.

Fig. 1.



At the extreme temperatures, B and B″, suppose a chronometer to be regulated to mean time; then since at these temperatures, the tension of the balance-spring must have the same ratio to the inertia of the balance, take B D and B″ D″ at right angles to B B″, in proportion to the inertia at these temperatures; and also the parts B G and B″ G″, in proportion to the corresponding tensions of the balance-spring. Join D D″ and G G″. Since the tension is proportional to the temperature, the locus of G will be the *straight line* G G″, and from the relation which exists between the inertia and the temperature, the locus of D will be a *curve line*, as D D′ D″. Let B′ D′ be another ordinate to the curve, at an intermediate temperature, which produced meets D D″ in the point *m*, and cuts G G″ in G′. Now in order that the chronometer may go mean time at the mean temperature, as in the extreme temperatures, the tensions of the balance-spring, which are here represented by the lines B G, B′ G′, and B″ G″, should be in proportion to the ordinates B D, B′ D′, and B″ D″, which cannot be the case unless B′ D′ is equal to B′ *m*, or unless the point D′ coincide with the point *m*, or the curve D D′ D″ coincide with the straight line D *m* D″—which is impossible. The quantity *m* D′ or difference between the inertia of the balance and *what it ought to be*, for the chronometer to go mean time, is seen in the diagram to be the greatest at the intermediate temperatures, which in the actual performance of the chronometer is the case—and as they are found to gain at these temperatures, it is clear that B′ D′ is less than B′ *m*, or the curve is convex towards the axis B B″. If the chronometer, instead of being adjusted to the extreme temperatures, be adjusted to the mean, and one of the extreme temperatures, (as the highest for instance,) join D D′ and produce it until it meets D″ B″ in the point *n*; then since D″ B″ is greater than *n* B″ by the difference D″ *n*; the inertia will be greater *than it ought to be*, to an increased amount, corresponding to a diminished gaining, or an increased losing rate of the chronometer, which is also found to be the case. I shall now proceed to show the mode of construction of the balance which I have adopted in order to obviate the error; and I have accomplished this, not by supplementary weights, but by effecting a more perfect conformity with the proper law of approach in the compensating weights themselves; the correction being, thereby, both continuous and simultaneous.

Before entering on a description of my improvements, I will explain, from the following diagram, the defects in the construction of the ordinary compensation-balance, and show its inadequacy to accomplish the required correction for the varying tension of the balance-spring.

Fig. 2.

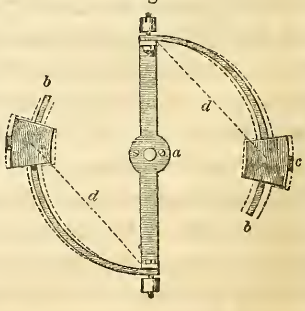


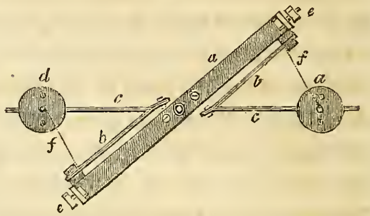
FIGURE 2.—The ordinary compensation-balance: *a*, the balance; *b*, two segments of compensating laminæ of brass and steel, brass being on the outside of the segments, and steel on the inside; *c*, compensating weights.

On an increase of temperature, the movable extremities of the segments approach the centre of motion, as represented by the dotted inner curve lines, and the reverse effect takes place on a decrease of temperature. Now, that the inertia may correspond with the tension, the compensation-weights, *c*, upon an increase of temperature, should approach the centre of the balance with an *accelerated motion*, and, upon a decrease of temperature, with a *retarded* one. On examination of this ordinary balance, it is evident that its action is directly opposed to the above requisition. And before further investigation of the subject, it is important to remember, that when metals of unequal expansion, such as brass and steel, are united, (as in the compensation-balance,) the extremities of the laminæ move in a spiral curve, on being influenced by change of temperature. I will now proceed to the explanation:—If we connect, by means of the dotted straight line *d*, the centre of gravity of the compensation-weight, with the junction of the laminæ, at the arm of the compensation-balance, and suppose a change of temperature from heat to cold to take place, the result will be, that the brass, which is on the outside of the segments, contracts more by the increase of cold than the steel on the inside; hence the distance between the centre of gravity of the compensation-weights, and the junction of the laminæ at the arm, is increased: in other words, the length of the chord of the arc, or dotted line, is, by the unfolding or straightening of the segment, augmented. Under such circumstances the radius of motion and the increment of distance are increased, whilst from an increase of temperature the converse takes place, which is the very *reverse* of what should occur. For, by an increase of cold, the chord of the arc *d* should be *shortened*, and *lengthened* by an increase of heat; a result which my present invention is designed to effect by applying to the ordinary compensation, which may be termed primary compensation, the addition of a secondary *continuous* compensation, which will move the compensation-weights over a space more calculated to accommodate the force of the inertia to the varying tension of the balance-spring.

In the drawings annexed, are representations of different modifications of my invention, given as exemplifications of the principles upon which my improvements are effected.

Fig. 3, represents the plan of a compensation-balance, in which the two compensation-weights are each carried by a primary and a *continuous* secondary compensation-piece, which pieces are shown straight, in order to facilitate the clear understanding of the principles of my invention: although, in practice, I frequently use a curved figure for the pieces, or make the primary and *continuous* secondary compensations in one curved piece.

Fig. 3.



a, is a simple balance-bar, made of brass or other non-magnetic metal or metallic compound.

b, two primary compensation-pieces of brass and steel, or other suitable metals, which pieces are firmly fixed on the balance-bar *a*, nearly at the extremities, and run parallel with it towards the centre.

c, two *continuous* secondary compensation-pieces attached to the free ends of the primary pieces *b*, and proceeding in a direction from the centre; the brass of these pieces is, in both cases, at the inside of the angle, and the steel at the outside.

d, the compensation-weights.

c, the timing-weights.

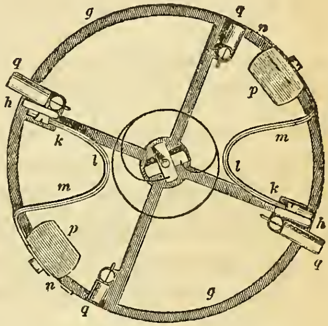
The pieces *b* I term the primary compensation, because their action is to vary the inertia by bringing the compensation-weights *d* nearer to the centre of motion for an increase of temperature, and the reverse for a decrease; and it is to be distinctly understood, that this may be fairly considered as the only adjustment which the ordinary chronometer possesses, to correct the errors of the balance-spring. I have before remarked, that the compensation-weights, in the usual construction, do not go sufficiently in towards the centre of motion, on an increase of temperature; while they come out too far on a decrease. I will now explain how the correction of this fault is to be accomplished by my invention.

The secondary compensation-pieces *c* move the compensation-weights *d* on a change of temperature, in a direction nearly concentric with the centre of motion, and thus produce but little variation as regards the times of vibration. These pieces I denominate the " secondary compensation-pieces," and their position is such, that the variation in the central distance of the compensation-weights, due to a given change of temperature, is a maximum; that is, the variation which causes the secondary compensation only.

For example; on an increase of temperature, the weight *d* is moved further from the junction of the primary compensation-piece *b* with the bar; and as the length of the dotted line *f*, drawn from the centre of gravity of the compensation-weight *d*, to the junction with the bar,—as the length of this line, I say, is augmented by increase of temperature, the compensation-weight *d* makes a quicker and nearer approach to the centre of motion than in the old compensation-balance; whereas, on a decrease, the contrary takes place.

Fig. 4, shows the plan of a balance, in which the primary and *continuous* secondary compensation is obtained by means of one curved piece on each side of the balance.

Fig. 4.



g, the balance, made of brass or other non-magnetic metal.

h, two blocks or studs raised above the face of the balance, to form the supports of the compensation.

k, *l*, *m*, two laminæ, each curved in such a manner as to combine the joint effect produced by the primary and secondary compensation-pieces shown at *b* and *c* of Fig. 3, the part from *k* to *l* (Fig. 4) corresponding to the primary compensation-piece, and that from *l* to *m* corresponding to the secondary compensation-piece of Fig. 3.

n, two prolongations from the ends of the compensation-pieces; of steel only.

p, two compensation-weights, screwed to the prolongations *n*.

q, four timing-weights.

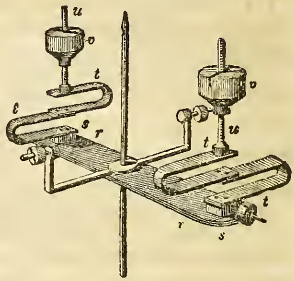
Having thus fully explained the principle of my primary and secondary compensation, I would remark that my invention embraces every modification of this principle, by which the compensation shall diminish the distance of the compensation-weights from the junction of the laminæ with the arm upon a decrease of temperature, and produce the converse upon an increase; which is the reverse of what has generally been done in the ordinary construction of the balance.

In order to adjust this balance, as regards the secondary compensation, if the chronometer gains at the extremes of temperature, compared with the mean, the secondary compensation-piece must be shortened and the time restored, by adding to the nuts at the ends of the bar; if the chronometer loses at the extremes of temperature, the reverse operations must be resorted to. The ordinary adjustments for temperature are made by sliding the weights *p* along the prolongations *n*.

In figures 3 and 4, the compensation-weights are represented as moving in the plane of the balance; but I produce a similar effect by causing the compensation-weights to move in a plane passing through the axis of motion; the mode of accomplishing which may be seen in the subjoined explanations.

Fig. 5, represents a balance formed according to this mode.

Fig. 5.



r, a compensation diameter bar, fixed on the balance axis; it is composed of brass and steel, the latter being nearest the compensation-weights. This bar carrying a weight upon an upright rigid support, is the only compensating power hitherto employed in chronometers.

s, two blocks attached to the ends of the bar, to receive the secondary compensation-pieces.

t, two secondary compensation-pieces, each constituted of two pair of laminæ bent into the form of staples and riveted together, with the bows lying in opposite directions, one end of the laminæ being fixed upon the block *s*, the brass being in the insides of the staples, and the steel on the outsides.

u, two pillars fixed on the end of the upper pieces of these laminæ; to carry the weights, these pillars are furnished with screws, on which the weights turn for adjusting their heights.

v, the two adjustable weights.

By this arrangement the weight always moves in a line nearly parallel to the axis of the balance.

On elevation of temperature, the distance between each staple is increased in height, and by this means the compensation-weight is raised from the balance-bar; under these circumstances, the augmentation thus effected by my secondary compensation enables the primary compensation to carry the weight over a greater space and with accelerated velocity, towards the centre of motion; the reverse effect of course taking place on a decrease of temperature. This variation of velocity to and from the centre of motion, could not possibly be brought about if the weights were placed on the before-mentioned rigid immovable supports, at the extremities of the balance-bar, as is usually done in the ordinary balances of this construction.

It may be remarked, that the bows of the secondary compensation-pieces may stand across the length of the bar *r*, obliquely, or at any angle, without varying the perpendicularity of the motion of the weights.

Fig. 6.

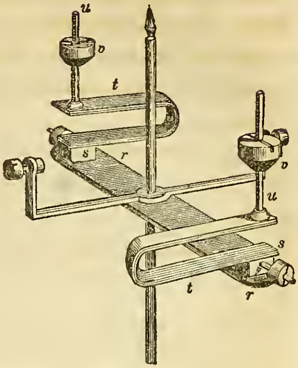


Fig. 6, is a perspective view of a balance of the same kind as Fig. 5, but in this case, the *continuous* secondary compensation-pieces, each made in the form of one staple only, stand across the primary compensation-bar at right angles; which is an essential condition of this construction, because a single staple compensation will not raise the weight perpendicularly from the end of the bar; therefore the bow of the staple should be placed in a position which will raise the weight, without producing more variation in the time than is unavoidable.

In order to adjust the secondary compensation of the balances shown in figures 4 and 5, if in excess, the staples must be shortened or thickened; and the reverse must be done, if in defect: the primary compensation is adjusted, by varying the height of the weight *v*, on the screw *u*, according to the usual practice.

My patent further consists in the introduction of a remontoire-escapement into a chronometer or other portable timekeeper. The remontoire-spring being wound up at regular intervals by the main-spring through the train of wheels, gives an invariable impulse to the balance by means of an impulse-escape-wheel.

The principle of this escapement, now introduced into a chronometer, may be considered the same as that lately invented by G. B. Airy, Esq., Astronomer Royal, who furnished me with the drawings from which I recently constructed the first astronomical clock containing his escapement.

Mr. Airy having generously given his invention to the public, I have adopted those parts of it which were applicable to a chronometer; and have succeeded in preserving the ordinary detached escapement, (which has so long and so deservedly maintained its undisputed preëminence,) and uniting in conjunction with it such adaptations as to convert it into a remontoire-escapement for a chronometer.

The result of this combination is, that a constant impulse is given to the balance by the impulse-escape-wheel, without its receiving any lateral pressure from the usual train of wheels; for that of the remontoire-spring can hardly, with justice, be so called.

London, Nov. 1, 1842.

Art. XI.—*Description of a "Blind Fish," from a cave in Kentucky;* by Jeffries Wyman, M. D., Member of the Boston Society of Natural History.

The specimen from which the following description is drawn, was presented to the Boston Society of Natural History by J. G. Anthony, Esq. of Cincinnati. It corresponds for the most part with the description of the *Amblyopsis spelæus*, described by Dr. Dekay in the Fauna of New York, but in some particulars it differs.

The specimen here described was $4\frac{1}{10}$ inches long, and characterized by a broad vertically compressed head, covered with a whitish integument entirely destitute of scales, but on it are seen numerous elevations or ridges, most abundant on the lateral portions; some of them intersecting each other at right angles. The lower jaw is more prominent than the upper; no appearance of eyes; nostrils double, the anterior ones tubular, the posterior nearly circular, about $\frac{1}{10}$ inch behind the preceding. Both jaws are provided with folds of skin, or lips; intermaxillaries and lower jaw armed with minute slender and slightly recurved teeth, most abundant at a short distance from median line—a group of teeth on palatines on each side; also two groups in pharynx above, and four below. Upper maxillaries concealed by integuments, and destitute of teeth. Intermaxillaries form the borders of mouth above, and extend nearly to its angles. Branchial aperture large, branchiostegous rays 6 on each side.

Body covered with circular scales which terminate abruptly at the posterior limit of the head; the scales are smaller on back than on the sides, and are so enveloped in the cuticle as not to present free edges. Lateral line occupies the middle of the lateral region, commencing under the anterior extremity of the dorsal fin, passes directly backwards. First ray of dorsal, a little posterior to the middle of body; anal commences a little behind the dorsal; abdominals very small.

Fin rays. Pectoral, 10; dorsal, 10, first very minute; caudal, 17 or 18; anal, 9; abdominal, 4.

Anus very far forwards, about $\frac{2}{10}$ inch behind the angle formed by the union of branchial membranes.

Alimentary canal; entire length less than that of the body. Œsophagus very short; stomach cylindrical, terminating posteriorly in a short triangular cul de sac, the point of which reaches the posterior limit of the cavity of the abdomen. Stomach contracted, and mucous membrane thrown into longitudinal folds. Pylorus situated near posterior extremity of stomach; has a distinct valve which projects into cavity of duodenum; two short pyriform cæcal appendages, open by distinct orifices on opposite sides of intestine. Mucous membrane of small intestine arranged in reticulated cells, which become less distinct towards termination. Length of small intestine 1½ inches; of large intestine ½ inch; the two separated by a distinct valve.

Liver consists of two lobes; left extending nearly whole length of abdominal cavity, right very short. Gall-bladder distinct.

Air bladder cordiform, deeply cleft anteriorly.

Brain; from anterior extremity of olfactory lobes to posterior portion of cerebellum, 0.2¼ inch. Olfactory lobes in contact with and just in front of cerebral hemisphere, of slightly pyriform shape, and giving off large olfactory nerves. Cerebral lobes, nearly spherical, slightly compressed on median line, where the right and left unite. Optic lobes much smaller than preceding, and partly concealed by the cerebellum. Cerebellum nearly spherical, slightly divided on median line, giving it a somewhat cordiform appearance. Fourth ventricle completely exposed, and widely open. Posterior pyramidal bodies distinct, projecting over the cavity of the ventricle near its middle. External to these last arise the branchio-gastric nerves. Auditory sacs large; ampullæ of semicircular canals containing otolites, one of which is of a trapezoidal shape, and nearly equalling in size one of the cerebral hemispheres. The inferior optic lobes, "lobi inferiores," very small, not larger than a pin's head; in front of them rests the pituitary body. No optic nerve was found. Branchio-gastric and fifth pairs of nerves of the usual size.

Internally the nostrils consist of an ovoidal cavity, $\frac{1}{10}$ inch in longest diameter; olfactory membrane arranged in seven folds or digitations of unequal length, and radiating from a point in the anterior portion of the cavity. At the anterior extremity of this cavity is a small orifice opening into a blind sac or canal, which passes at first directly backwards and then ascends upon the upper surface of the cranium. On the most careful dissection no traces of eyes were found.

From the above description it appears that this fish, inhabiting a dark cavern, is reduced, as regards its organs of vision, to a much more imperfect condition than the *Proteus anguinus*, inhabiting the subterranean caverns of Illyria, or the common mole, in both of which eyes exist, although of a microscopic size. Dr. Dekay has placed this fish among the Siluridæ; though, as he distinctly states, only provisionally. The presence of scales and cæcal appendages to the pylorus, as well as the absence of cirrhi about the mouth, would seem to indicate feeble affinities with the Siluridæ. The parts entering into the composition of the brain, when compared with those of the Pimelodus, present many differences in the size and proportions. Its true affinities cannot be well determined until an opportunity shall be afforded by future dissections for the examination of its osteology.

Art. XII.—*On the Adverbial Genitive Case in English;* by Prof. J. W. Gibbs.

The genitive case in English is usually regarded as altogether adnominal, i. e. as used only in connection with a noun. Hence the only rule in our common grammars concerning this case is, that it is governed by a substantive, either expressed, or implied by the context. In the other Teutonic dialects, however, this case is also used adverbially, i. e. in connection with a verb, and that to indicate various relations. This adverbial use of the genitive, although generally overlooked, and often misunderstood, may be shown to exist also in English in several classes of words.

I. This genitive is found in a few substantives, and that without any preposition preceding.

1. *Needs,* (Old Eng. *nedes*, *needes;*) of or from necessity. Thus,

Soche thinges muste *nedes* be.—*Tyndale*, 1534. Mark 13: 7.

I must *needes* goe forth and see it.—*Rhemish Version:* Luke 14: 18.

He will *needs* be a judge.—Gen. 19: 9.

Needs here is the genitive of *need*. Comp. Anglo-Sax. *nedes* or *nydes*, of necessity, composed of *ned* or *nyd*, necessity, and *es*, the termination of the genitive singular masculine.

2. *Ways*, in *noways*, *straightways*, *otherways*, *longways*, *sideways.*

Ways here is the genitive of *way.* Comp. Germ. *keines weges*, noways, genitive of *keiner weg; gerades weges*, straightways, genitive of *gerader weg.*

Note.—*Ways* in *always* is probably plural. Comp. Anglo-Sax. *ealle wæga*, in all ways, the adjective *ealle* and the substantive *wæga* being both in the accusative plural.

3. *Gates*, in Old English *othergates*, in another manner. Thus,

If Sir Toby had not been in drink, he would have tickled you *othergates* than he did.—*Shaksp.*

Gates here is the genitive of *gate*, i. q. *gait*, way, manner. Comp. Scott. *thus gatis*, after this manner, both words being in the genitive singular.

Note.—*Gates* in *algates* is probably plural. Comp. Anglo-Sax. *algeats;* also Scott. *mony gatis*, in various ways; also *always* supra.

4. *Times* in *sometimes*, at one time.

Here *times* is the genitive of *time.*

Note.—*Times* in *sometimes*, at some times or intervals, is plural.

II. This genitive is found in some substantives with a preposition preceding.

1. *Adays*, (Old Eng. *adayes*, *adaies*,) in or on day, i. e. by day. Thus,

Aday when hyt is lygt.—*Syr Launfal.*

So in the phrase *now adays.*

Days here is the genitive of *day.* Comp. Anglo-Sax. *dæges*, by day, genitive of *dæg*, day; Germ. *dags*, by day, genitive of *dag*, day.

Note.—The idea that *days* is plural, seems sometimes to have affected its use. Thus,

What men of spirit now *adays*,
Come to give sober judgment of new plays?—*Garrick.*

2. *Anights*, in or on night, i. e. at night. Thus,

I bid him take that for coming *anights.*—*Shaksp.*

Such as sleepe *anights.*—*Shaksp.*

Nights here is the genitive of *night.* Comp. Anglo-Sax. *nihtes*, Germ. *nachts*, by night, where *s*, or *es*, is the termination of the genitive singular masculine.

3. *Besides*, (Old Eng. *bisidis*,) by the side, over and above. Thus,

In that dai Jhesus ghede out of the hous, and sate *bisidis* the see.—*Wiclif:* Mat. 13: 1.

Sides here is the genitive of *side.* Comp. Germ. *beiseits*, aside, where *s* is the termination of the genitive singular masculine.

4. *Ships*, in *midships*, *amidships*, *thwartships*, *athwartships*, is the genitive of *ship.*

III. This genitive is found in a few adjectives, either with or without a preposition preceding.

1. *Askance*, obliquely. Comp. Dutch *schuins*, obliquely, where *s* is the genitive termination.

2. *Soons* in Old English *eftsones* or *eftsoons*, soon afterwards, compounded of Anglo-Sax. *eft*, afterwards, and *sones*, soon. Thus,

Moyses *eftsones* resorting to Damascus.—*Gower.*

Crying *eftsoons* alowd.—*Holland.*

Eftsoons the father of the silver flood.—*Thompson.*

Soons here is the genitive of *soon.* Comp. Anglo-Sax. *sones*, with the termination of the genitive.

3. *Unawares*, or *at unawares*, (Old Eng. *unwares ;*) unexpectedly. Thus,

That daye come on you *unwares.*—*Tyndale:* Luke 21 : 34.

Jacob stole away *unawares* to Laban.—Gen. 31 : 10.

Let destruction come upon him *at unawares.*—Ps. 35 : 8.

Unawares here is the genitive of *unaware.* Comp. Anglo-Sax. *unawares*, which is in the genitive.

4. *Wards*, in *inwards*, *outwards*, *towards*, *fromwards*, *onwards*, *upwards*, *downwards*, *forwards*, *backwards*, *afterwards*, *sidewards*, *hitherwards*, *homewards.*

Wards here is the genitive of *ward*, Lat. *versus.* Comp. Goth. *andvairthis*, *jaindvairths*, *vithravairths.* Old Germ. *inwertes*, *uzwertes*, *anawertes*, *heimwartes.* Germ. *einwärts*, *auswärts*, *abwärts*, *aufwärts*, *unterwärts*, *niederwärts*, *vorwärts*, *ruckwärts*, *seitwärts*, *herwärts*, *thalwärts.* Anglo-Sax. *uteweardes*, *towardes* or *toweardes*, *upweardes*, *fromweardes*, *hameweardes.*

The termination *s* in these examples from the kindred dialects is evidently the sign of the genitive case.

IV. This genitive is found in some numerals.

1. *Once*, (Old Eng. *onys*, *oonys*, *onis ;*) one time, formerly. Thus,

For and thy wyfe may *onys* aspye.—*Poem in the time of Henry II.*

He was deed *oonys.*—*Wiclif:* Rom. 6 : 10.

Once here is the genitive of *one.* Comp. Dutch *eens*, once, genitive of *een*, one; Old Germ. *eines*, genitive of *ein*, one; Germ. *einst*, (for *eines*,) formerly.

2. *Twice*, (Old Eng. *twies*, *twyes*,) two times. Thus,

As presente *twies*.—*Wiclif:* 2 Cor. 13: 2.

Twyes is somer in that lond.—*Kyng Alisaunder.*

Twice here is the genitive of *two*.

3. *Thrice*, (Old Eng. *thries*, *thryse*,) three times. Thus,

Thries I was betun.—*Wiclif:* 2 Cor. 11: 25.

Thou shalte denye me *thryse*.—*Bible*, 1551.

Thrice here is the genitive of *three*.

V. This genitive is found in some pronouns.

1. *Else*, (Old Eng. *elles*, *ellys*, *ellis*, *els*; Scott. *ellis*;) otherwise.

Elles wyder.—*R. Gloucester.*

Let honge me *ellys*.—*Piers Plouhman.*

Ellis ye schuln have no mede at youre fadir that is in hevenes.—*Wiclif:* Mat. 6 : 1.

Or *els* ye get no rewarde of youre father which is in heven.—*Tyndale:* Mat. 6 : 1.

All that *els* I saw.—*Spenser.*

Else here is the genitive of the root of Gr. ἄλλος, Lat. *alius*, Goth. *alis*. Comp. Anglo-Sax. *elles*, Old Germ. *alies*, *elies*, *alles*, *allas*, *ellies*, Dan. *ellers*; in all which forms *s* is the termination of the genitive.

2. *Hence*, (Old Eng. *hennes*, *hennis*, *hens*; also *han*, *henne*;) from this place.

Holynesse and love han ben longe *hennes*.—*Piers Plouhman.*

Passe thou *hennes*.—*Wiclif:* Mat. 17 : 20.

Ye schulen not se me fro *hennes* forthe.—*Wiclif:* Mat. 23 : 39.

Hens over a mile.—*Chaucer.*

Hence here probably has the termination of the genitive. Comp. Anglo-Sax. *heona*, (Lat. *hinc*, Provenç. *hereance*;) Germ. *hinnen*.

3. *Thence*, (Old Eng. *thennes*, *thennis*, *thens*;) from that place.

And he ghede out fro *thennes*.—*Wiclif:* Mark 6 : 1.

They *thennes* went.—*Chaucer.*

From *thens*forth.—*Chaucer.*

Thence here probably has the termination of the genitive. Comp. Anglo-Sax. *thanan*; Germ. *dannen*; Provenç. *thereance*.

4. *Whence*, (Old Eng. *whennes*, *whethence*;) from what place.

Of *whennes* to this, alle these thingis.—*Wiclif:* Mark 6 : 2.

From *whens* hath he these thinges?—*Tyndale:* Mark 6:2.

Whens that she came.—*Gower.*

Whence here probably has the termination of the genitive. Comp. Anglo-Sax. *hwonan*, *hwanon*, Old Germ. *hwanan*, Germ. *wannen.*

5. *Since*, (Old Eng. *sens*, *sence*, *sithence*, *sithens;*) from the time.

How longe is it a goo, *sens* this hath happened him?—*Tyndale:* Mark 9:21.

For *sence* the fathers dyed, all thinges continue.—*Tyndale:* 2 Pet. 3:4.

And therefore *sithence* the bishop of Rome will now adaies be so called.—*Jewell.*

For *sithens* shootinge was neglected.—*Ascham.*

Sithence the verie apostles owne times.—*Hooker.*

Before or *sithence.*—*Hooker.*

Since here probably has the termination of the genitive. Comp. Anglo-Sax. *sithen*, *siththan*, *syththan;* Dutch *sinds;* Germ. *seit.*

6. *Thus*, (Old Eng. *this;*) in this manner.

He hath lain *this* long at great costes and charges and canne not have hys matter come to the hearynge.—*Latimer*, 1562.

"*Thus* much" for "*this* much."—*Webster.*

Thus here is the genitive of *the* or *that.* Compare Anglo-Sax. *thus*, *thæs;* Dutch *dus.* The Anglo-Sax. *thæs*, *this*, is the genitive singular masculine and neuter of *se*, *theo*, *that.*

VI. This genitive is found in some words, in which *s* the sign of the genitive is now hardened into *st.*

1. *Against*, (Old Eng. *agens*, *ageins;*) in opposition to.

He that is not with me: is *agens* me.—*Wiclif:* Mat. 12:30.

Ageins nature.—*Chaucer.*

Against here is probably the genitive case of an old noun, whose meaning cannot be exactly defined. Comp. Anglo-Sax. *to-geanes*, *to-genes*, *to-gægnes*, *to-gegnes*, Dutch *tegens.* These Anglo-Saxon and Dutch forms commence with a different prefix, but have the genitive termination.

Note.—The convenient distinction made in English between *again* and *against* does not exist in the other dialects.

2. *Alongst*, (obsolete, see Dr. Webster; Old Eng. *alongest;* Scott. *langis;*) by the length.

To sayle *alongest* by the lande.—*Nicolls:* Thucyd. 1550.

Alongst the sea-coast.—*Knolles.*

Langis the ryvere of Anien.—*Douglas:* Virgil.

Alongst here is the genitive case of *long*. Comp. Germ. *längs*, along; Old Germ. *langes*, and Germ. *längst*, a long time; Dutch *onlangs*, recently, *langs*, along; Swed. *længs*, along.

3. *Amidst*, in the midst or middle.—See *Midst.*

4. *Amongst*, (Old Eng. *amanges*, *amonges*, *amongest;* Scott. *amangis*, *amangys;*) in the crowd.

To halden *amanges* yeu ine hord.—*Old English Letter of the year* 1258.

Amonges other of his honest thinges.—*Chaucer.*

I stonde as one *amongest* all.—*Gower.*

Amangys thame.—*Scott. Acts*, 1567.

Amongst here is probably the genitive case of an old noun, denoting *a crowd* or *multitude.*

5. *Atwixt*, (obsolete, see Dr. Webster,) between.—See *Betwixt.*

Great love was *atwixt* hem two.—*Chaucer.*

With dreadful thunder and lightning *atwixt.*—*Spenser.*

6. *Awhilst*, (not in Webster, nor in Richardson.) See *Whilst.*

7. *Betwixt*, (Old Eng. *bituex*, *bytwixe*, *betwix*, *bitwixen*, *bytwyx*, *bytwyt*, *betwyx;* Scott. *betweesh;*) between.

Bituex them.—*R. Brunne.*

Bytwixe us and you.—*Wiclif:* Luke 16: 26.

Betwix all maner folk.—*Chaucer.*

> This was the forward pleinly t' endite,
> *Bitwixen* Theseus and him Arcite.—*Chaucer.*

Betwixt here is the genitive case of an old noun signifying *two.* Compare Anglo-Sax. *betweohs*, *betweox*, *betwux*, *betwuxt*, *betwixt.*

8. *Midst*, in the phrases *amidst*, *about the midst*, *from the midst*, *in the midst*, *into the midst*, *of the midst*, *out of the midst*, *through the midst*, etc. (Old Eng. *myddes*, *myddest*, *myds*, *middes*, *middest*, *mids;* Scott. *myddis;*) the middle.

In the *myddes* of the world.—*R. Gloucester.*

Yet was he caught *amiddes* all his pride.—*Chaucer.*

And the vayle of the temple dyd rent even thorow the *myddes.* —*Tyndale:* Luke 23: 45.

Which is in the *myddes* of the paradice of God.—*Tyndale:* Rev. 2: 7.

The shippe was now in the *middes* of the see.—*Tyndale:* Mat. 14: 24.

For lykewise as God is in the *myds* of the good counsayle, so in the *myddest* of an evyl counsayl, is ther undoutedly the dyvel.—*Sir T. Moore.*

When Calidora
Him overtook in *middest* of his race.—*Spenser:* Faerie Queene.

Among the *middest* crowd.—*Spenser.*

And the vaile of the temple was rent in the *mids.*—*Original Edition of King James's Bible:* Luke 23 : 45.

Which is in the *middest* of the paradise of God.—*Original Edition of King James's Bible:* Rev. 2 : 7.

In *myddis* of the land.—*Wyntown.*

Midst is rarely used as a nominative, or as an accusative without a preposition.

Midst here is the genitive case of *mid*, the middle. Comp. Anglo-Sax. *to-middes*, where *middes* is the genitive of Anglo-Sax. *midd*, the middle; Germ. *mittelst*, by means of, for *mittels*, the genitive of Germ. *mittel*, the middle or means.

Note.—Dr. Webster supposes *st* in *midst* to be the sign of the superlative degree. So Sir John Stoddart, art. *Grammar*, in Encyc. Metrop. p. 129.

9. *Whilst*, *awhilst;* (Old Eng. *whiles*, *whilest;* Scott. *quhiles*, *whiles;*) while.

Wat sholde we women, worche the *whiles.*—*Piers Plouhman.*

Whilest good men wanted it.—*Beaumont and Fletcher.*

Whiles he tasted the wine.—*Some Editions of King James's Bible:* Daniel 5 : 2.

Quhiles wandering, *quhiles* dandring.—*Burel's Pilg.*

Whilst here is the genitive case of *while*, time.

Note.—On the *st* generally, comp. Germ. *nebst*, (from *neben*, *nebens;*) *anders* and *anderst; selbst*, (Old Germ. *selbs*, Dutch *zelfs*.)

Art. XIII.—*On Phosphate of Lime (Apatite), in the Virginia Meteoric Stone; by* Charles Upham Shepard, M. D., Prof. of Chemistry in the Medical College of the State of S. Carolina.

M. Rumler, in a recent number of Poggendorff,* in enumerating certain ingredients in meteorites, after the mention of phos-

* Ueber die bei Jwan, in Oedenburger Comitate Ungarns, am 10. August Abends zwischen 9 und 10 Uhr aus der Luft gefallenen und für Meteorsteine ganz neuer Art. ausgegebenen Körner: von Karl Rumler, Kustos-Adjuncten am k. k. Hof-Mineralien-Kabinette zu Wien, s. 279. Annalen der Physik und Chemie von Poggendorff, Band LIV, Stuck 2, 1841.

phoric acid, adds, "for SHEPARD's discovery of this acid in the meteoric stone of Richmond, is still doubtful, (*denn* SHEPARD's *Entdeckung dieser Saüre in Meteorsteine von Richmond ist noch zweifelhaft*)." Although this observation occasioned in me no surprise, since I had stated at the conclusion of my remarks on the mineral,* my regret "that the smallness of the quantity, prevented me from making still further experiments by means of which my conclusion concerning its nature might have been rendered certain," still it determined me to make new trials for placing the subject if possible, beyond dispute.

Through the kindness of Prof. SILLIMAN, who possesses nearly the whole of the Richmond stone, I was permitted to detach a fresh fragment which brought into view several points of the yellow mineral in question. The most perfect of these, having the size of half of a pin's head, was crushed to powder on a small piece of clean platinum foil, previously fitted to the bottom of an agate mortar. The foil with the crushed mineral thereon, was then shaped into a little cup, and a freshly cut piece of potassium pressed into it, so as to be in immediate contact with the powder. The platinum cup and its contents were then forced to the bottom of a test tube (¼ of an inch in diameter and 2½ long); and after heating the tube in contact with a live coal, until a slight flash of light was witnessed in the platinum cup, a few drops of water were let fall into the tube. On holding the open end of the tube beneath the nose, a distinct odor of phosphuretted hydrogen was recognized. A few drops of dilute nitric acid were subsequently added; and after digestion for a few moments and neutralization by ammonia, oxalate of ammonia threw down an evident precipitate.

The foregoing experiment clearly establishes the presence of phosphoric acid in the mineral; and the precipitate with oxalate of ammonia, taken with all the circumstances detailed in my mineralogical account of the substance, leave scarcely a doubt of its being combined with lime, in the form of phosphate of lime.

Charleston, S. C., March 18, 1843.

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* See Vol. XVI, p. 199, of this Journal.

Art. XIV.—*On the Analogies between the Modern Igneous Rocks and the so-called Primary Formations, and the Metamorphic changes produced by heat in the associated sedimentary deposits;* by James D. Dana, Geologist of the late U. S. Exploring Expedition.

[Read before the Association of American Geologists and Naturalists at Albany, April 26th, 1843, and published by their authority.]

The conclusions to which I arrive in the remarks that follow, are the result of observations made by me in the course of the cruise of the Exploring Expedition. In illustrating the subject, I have drawn but little upon the facts of the Expedition, as these are by authority reserved for the government publications now in process of preparation. I would however state, to justify myself against the imputation of haste in my generalization, that the regions offered for examination during the cruise, were of varied character and unusual interest; that the Andes of Chili and Peru, the mountains and plains of Oregon, the coral, basaltic and volcanic islands of the Pacific, and the regions of sandstone, coal and basalt in New South Wales, and portions of New Zealand, have all contributed to these results, offering rocks for examination of all ages from the burning lavas and forming coral rocks to the deep-seated granite and the associated schists; and there are scarcely any of these different formations which do not furnish something in elucidation of the subject under discussion. This may possibly be deemed sufficient to acquit me of presumption if I dare to differ from some names high in authority. I would disclaim ever having been actuated by a desire to seek out novel facts or novel principles, being satisfied that the common things which meet the eye, are more replete with instruction than the unusual and strange which only create surprise.

The principles in view bear upon the metamorphic theory of Mr. Lyell, and they have been deduced by comparing the Plutonic rocks—the various granites and associated schistose formations—with igneous rocks of all ages down to the modern lavas, together with their effects upon sedimentary strata. It will hence appear that although I may dissent from some of Mr. Lyell's views, I am still carrying out his grand fundamental canon, that existing causes explain past phenomena, than which nothing has done more to advance and elevate the science of geology.

I shall endeavor to establish

1st, That the schistose structure of gneiss and mica slate, is no satisfactory evidence of a sedimentary origin;

2d, That some granites with no trace of a schistose structure, may have had a sedimentary origin;

3d, That heat producing the changes that are termed metamorphic, was not applied from beneath by conduction from some internal source of heat; on the contrary it was applied through the waters of the ocean, covering and permeating the deposits which received their high temperature from the eruption itself. In other words, the metamorphic rocks so called are not *hypogene*, as explained by Mr. Lyell, but—to use corresponding phraseology—*epigene*, or analogous to other rock formations, deposited and solidified on the surface of the earth.

The argument for the sedimentary origin of gneiss, mica slate, etc., is based upon the assumption that known igneous rocks do not assume a schistose structure. But this is far from true, for the descriptions of most volcanic regions mention the occurrence of laminated trachytes, basalts and porphyries. It is by no means unusual to find basalts and basaltic lavas with parallel lines of lamination, sometimes appearing only after being weathered, and at others so distinct as to admit of easy cleavage. This takes place both in massive and columnar basalt. At the Cape Verds, on the shores just below the town of St. Jago, the columns are gradually falling to pieces, owing to an exfoliation of the summit, from which curved plates separate easily, usually from a fourth to half an inch thick. Such instances, which are not uncommon, are imputed to a concentric structure. This is no doubt true, but the concentric structure is but one mode of crystallization, and crystallization as we believe, is the cause of the schistose structure in all igneous rocks. Massive basaltic lavas splitting into straight laminæ an inch or so thick, are met with at the Sandwich Islands. Laminated or slaty trachytes are too well known to require more than a mere allusion to them. The structure is far more thinly schistose than any gneiss, and often nearly as much so as many mica schists. Laminated porphyry is described by Prof. Emmons as occurring at Cannon's Point in northern New York, which splits into plates from a fourth to an inch in thickness.

A schistose structure then is certainly no evidence that the rock was not originally igneous; and if we consider how exactly this structure corresponds with the constitution of the rock and proceeds from its mineral composition, we shall see farther reason for rejecting this assumption. The general principle upon which this structure depends, appears to be simply this:—An igneous rock is in general more or less schistose or slaty, according to the cleavability of its constituent minerals. It is one of the general principles of crystallography, that when crystals of any mineral, form simultaneously, they tend to assume parallel positions. Faces of like cleavage lie in the same direction. No one can have glanced his eye over a druse of crystals, without being struck with the successive flashes of light that sparkle over the surface as its position is changed; and if he has observed attentively, he has perceived that a similar face in each of the crystals reflects simultaneously, and thus produces this beautiful effect. This is an instance of that parallelism in the position of crystals to which I have alluded. The same parallelism takes place in mineral aggregates, such as basalt or granite. Basalt is often described as having a regular cleavage and its columns as crystals. There is no proper analogy between the forms assumed by mineral aggregates and crystals; for such mixed compounds cannot crystallize as a whole. Each constituent mineral of basalt or granite crystallizes independently, and one or the other, according to that which predominates, impresses its cleavage upon the rock, or at least governs it to some extent in its fractures. The common cleavage of granite illustrates these facts. The rock consists of quartz, feldspar and mica—the first has no cleavage and the last yields in quantity to the feldspar, which is therefore the mineral upon which the cleavage of the rock depends. The unequal rectangular planes of fracture in granite rock known by every quarryman, correspond therefore, as has been before suggested, with the cleavages of the contained feldspar.

If we examine the various igneous rocks with reference to this principle, we shall find them supporting it throughout. Basalt consisting of feldspar and augite, and generally more or less chrysolite, is usually like granite, one of the uncleavable igneous rocks, or possesses it but indistinctly. Either of these minerals may determine the lines of fracture producing the columnar structure. In New South Wales, my attention was directed to a bed of what was called *mica slate*, overlying basalt. In hand-specimens it could hardly be distinguished from a rusty decomposing mica slate, but on examination at the locality, it proved to be nothing but decomposed basalt. The chrysolite, which was very indistinctly seen in the basalt itself, had become stained with iron through partial decomposition, and was split into thin scales, and the whole deposit had received, in consequence, the foliated structure of mica slate. This foliation had taken place parallel with the top of the bed of basalt—a semicolumnar variety—and seemed to evince that the crystals of chrysolite, while forming, assumed parallel positions as above explained, with the face of most perfect cleavage horizontal. The compact basalt could be chipped off with the hammer more easily at right angles with the columns than in other directions. The chrysolite therefore, was in this case the mineral on which the cleavage depended. In instances of what are called concentric structure, the cause is the same. The mineral upon which the concentric lamination depends, lies with its plane of most perfect cleavage, coincident with the plane of lamination.

In slaty trachytes, the lamination may often be distinctly traced to the feldspar or crystals of hornblende or mica. Large crystals of glassy feldspar, often lie in the plane of lamination, beautifully illustrating these principles. The hexagonal tables of mica have the same position, and when abundant, it produces the most slaty trachytes that are known. A fine illustration of the whole series of rocks from granite to mica slate, is presented by an extinct volcano in the Sacramento Plains, in Upper California. Much of the rock resembles granite or gneiss—although properly a trachytic porphyry. It consists of large crystals of glassy feldspar, disseminated thickly through a greyish base, which is speckled with small black crystals of hornblende and mica. In another part of this extinct volcano, the rock breaks into laminæ a third of an inch thick, and contains tables of mica lying as usual in the plane of fracture, while the feldspar is in very small crystals, imbedded in a compact feldspathic base.

We might cite examples from the volcanic regions of Europe, but what has been already said, appears sufficient to establish the fact that modern igneous rocks are laminated, and in general, more or less so according to the quantity and cleavability of the cleavable minerals they contain. Mica, the most perfectly foliated mineral, produces when abundant, and when not overruled by the other constituents, the most perfectly laminated rock.

If these principles are applied to granitic or ancient Plutonic rocks, including the associated schists, we may explain all their peculiarities of structure without other aid. In common granite the feldspar predominates much over the mica, and fixes the direction of its cleavage planes. Gneiss which contains more mica, has both the cleavage of mica and feldspar, the former at right angles with the latter. The mica was so abundant that the forming crystals felt that mutual influence, which causes them to take parallel or homologous positions, and so by arranging itself in planes, gave rise to that appearance of stratification which distinguishes gneiss from granite. In mica slate, the feldspar is wholly subordinate to the mica, and the structure is very distinctly foliated, almost like mica itself. A very little mica with quartz in grains, produces a rock with a micaceous structure, because quartz has no cleavage of its own.

Hornblende rocks, from syenite to hornblende slate, form a parallel series to the above, explained on the same principles. But as hornblende is less easily cleavable than mica, so hornblende slate is just so much less cleavable than mica slate.

We hence conclude, and not without reason, that the schistose structure of these rocks results from their constitution, and that a fine-grained granite with the amount of mica in mica slate, could no more exist without a foliated structure, than mica itself could crystallize in blocks like feldspar.

We may derive another argument on this subject from the metamorphic theory itself, which supposes that gneiss and mica slate were once beds of clay or argillaceous sandstone. Judging from the nature of such deposits—say, for example, those of the carbonaceous era—we should never believe that the elements of mica contained in them, lie in alternating layers, and in so thin alternations as mica presents in micaceous rocks. It is far more probable, if the rock be considered an altered clay, that the mica, when crystallizing, sought out its own positions upon the crystallographic principles already explained, and the same result would take place, and a rock equally foliaceous be formed, whether the beds of clay were stratified or compact.

Not to delay longer on this branch of the subject, I proceed with my second proposition, that *some granites may have had a metamorphic origin.*

In the remarks which have been made, I have no where denied that gneiss and mica slate may not sometimes be metamorphic, but have endeavored to show merely that their stratiform structure is no evidence of it. There is reason to believe that some of them are altered sedimentary deposits, and to these we may add, with as much reason, some granites.

It has been shown that the foliated structure of mica slate is the result of crystallization, whether metamorphic in its origin or not. If then certain clayey deposits are so constituted as to form, when heated, a large amount of mica, and give rise to mica slate—others to form less and produce a gneiss—may there not have been other deposits which should have assumed under the transforming heat the irregular structure of granite? But without laying much stress on this kind of reasoning, let us appeal to modern igneous formations for analogies.

In basaltic, porphyritic, trachytic and recent volcanic regions, there often occur deposits of argillaceous sandrocks of great extent, which have been derived from these igneous formations. The basaltic sandrock—called wacke or tufa—frequently so resembles basalt in structure and appearance, that the observer hesitates long before he decides upon its nature, and is not fully satisfied, till he can discern in some part of the formation, an imbedded pebble to assure him of its derivative origin. The only peculiarity it presents is a more earthy texture, but this belongs to some true basalts. I have met with such a rock in Oregon. The Andes are full of similar deposits, both of basaltic, greenstone and porphyritic origin, and often the closest examination is required to distinguish these sedimentary formations. They consist solely of earth or sand, of basaltic or porphyritic origin, which has been rehardened through volcanic action, and thus made to resume the compactness that belonged to the parent rock. Much of the so-called porphyry of the Andes is a porphyry sandrock, or a sedimentary rock of porphyry origin. It is as hard and firm in its texture as true porphyry, a trachytic variety of which it much resembles; moreover small crystals of feldspar are thickly disseminated through it, and aid in the deception. Were it not that an occasional pebble may be detected on the weathered surface, no one could doubt its being actually an igneous rock.

If sedimentary rocks of porphyry and basaltic origin may be so remodeled or rehardened by heat, as to be scarcely distinguishable from the parent rock, may not the same be true of sedimentary deposits of granitic origin? Does not analogy therefore authorize the conclusion that granite rocks may be metamorphic as well as gneiss and mica slate?

The nodules of syenite, granite, &c., found in granite, have long been a puzzle to geologists. They are imputed to the power of segregation, and many are no doubt due to this cause—at least those which by their concentric structure, show that they were formed by crystallization around a centre. But there are others without a trace of a concentric structure, of rounded and cobble-stone shapes, like the stones or boulders of the roads and fields. Why are they *not* imbedded stones or boulders? and why do they not prove that the granite which contains them is as much a metamorphic granite, as the pebbles in a porphyry bed prove it to be a metamorphic porphyry? The proof is at least more satisfactory than can be derived from a stratiform structure.

The granitic materials have been subjected to a higher heat than the porphyritic, and to this we impute the more perfect resumption of the features belonging to the parent igneous rock. It may be remarked that a running lava stream sometimes includes pebbles or boulders that may lie in its course. But these are rare and there is no danger of being led astray by such isolated cases.

Our argument thus far appears to have established these principles: that mica slate, gneiss and granite may be igneous rocks or they may be metamorphic rocks, and that the action of heat producing the metamorphic changes has been so effectual in some instances as to disguise entirely their derivative origin. To the rocks enumerated, the associated formations of syenite, protogine, talcose rock, argillite, &c. should be added, as they are part of one and the same series and come under the same general laws.

It has always been difficult to determine what place should be assigned to gneissoid granite—whether with granite as a purely igneous rock, or with gneiss as a metamorphic rock. But these views if true, show that the gneissoid or stratiform structure is no evidence of a deposit origin, and the question can no longer be, whether it should be associated with gneiss or granite. The nature of each is to be settled independently. It may be said that we place things in more doubt than they were before. It is admitted. The more reason for doubt we know, the better, if they actually exist, for there is little satisfaction and little profit in arriving at conclusions that are false. But before my remarks are closed, I shall hope that some of these doubts may be removed.

I proceed next to the third point before us—that the heat producing metamorphic changes, has been applied through the waters heated by the eruption itself. It is a little surprising that this cause of change in rocks should have been so generally disregarded or rejected, by the geologists of the day. It bears a slight tinge of Wernerism, and this may be its repulsive feature. One of our own most distinguished geologists, Prof. Silliman, first brought forward its claims, and urged them with conclusive arguments in the geological discourse appended to the American edition of Bakewell's Geology. Prof. Silliman has drawn his arguments from what is believed to have been the condition of the globe while the granite rocks were forming. I shall pursue farther the same mode of reasoning, and deduce other evidences from the analogies which may be found in regions of acknowledged igneous action.

Mr. Lyell in his metamorphic theory, treating of formations remote in origin from our own era and the present order of things, has necessarily indulged more freely in hypothesis than is to be found elsewhere in his geological writings. If any well-ascertained facts could be pointed to as a basis for his hypothesis, it might be received with less caution than it now demands. But in truth there are no changes known to be in progress of the character supposed, and although possible, analogies do not authorize us to consider them by any means probable. How is it in active volcanoes? Lavas may be heated to a red heat within a yard of the surface and still be so cool above, that the bare foot may walk upon them. To produce metamorphic changes in a deposit a hundred feet thick, the whole must consequently be in a state of fusion, or the upper crust will not be done through. We all know how small a thickness of fire-brick it requires to confine the heat of the hottest furnace. And if, as we believe, the heat attending granite eruptions far exceeded common volcanic temperature, our conclusions are still the same. The excessive heat in a furnace first fuses the inner surface, or the inner bricks, and thus by melting its way along, slowly commences a change on the outer row, and three inches only may intervene, between the heat of fusion and the temperature which does not pain the hand.

These views are further sustained by the action of heat on the walls of dykes. In subaerial eruptions of recent volcanic regions, the effect is usually slight; sometimes none is apparent, and at the most only for a few feet. When streams of lava have overflowed tufas, it has baked them only for one or two feet and perhaps altered the color to red, for one or two feet more; but beyond this, it is seldom that any effect is perceived. Clay will insulate the fused rock as completely in nature as when moulded into the shape of a furnace. The same effect must take place under water, except such modifications as may arise from heating the ocean itself, and the waters transfused through the stratified deposits; but these belong to the theory which I shall endeavor to sustain. The pressure of an ocean upon the erupted lavas, will not vary the result. Heat cannot be conducted to any extent except by fusing its way along, and in order to bake the rocks for twenty yards from a dyke by conduction alone, the first fifteen at least must be in a state of complete fusion. There are numerous examples of alterations in rocks to a greater distance than this; but how few of them give indications that the walls of the dyke, have been in fusion even for one yard?

But if the surrounding and permeating waters are heated at the time of a submarine eruption the heat may then be conveyed to great distances, and rocks may be discolored, baked or recrystallized, according to the temperature or extent and depth of the eruption.

That waters are heated by submarine eruptions, is a matter of observation. Dead fishes thrown up on the shores after eruptions are proofs of it. But it is needless to waste words upon this point, for we know that water and fire cannot come in contact without this effect. It is evident too that the amount of heat imparted to the waters, will depend on the extent of the eruption, on the time of its continuance, and more especially on the pressure of the ocean above. For the dense waters at great depths require a high temperature for ebullition.

To produce boiling the superincumbent waters must be so raised in temperature, that the vapor formed below, may pass up through it and escape; or in other words, ebullition will not take place till the heat be so raised, throughout the whole by communication from below, that the surface shall stand at 212° Fahr. At no very great depth, hence, the waters might be raised to the heat of ignition before ebullition will begin, and if the leaden waters of a deep ocean—for experiment as well as theory assures us of its great density—are for days in contact with the opened fires of submarine volcanoes, we can scarcely fix a limit to the temperature which they would necessarily receive. Why may they not be open for days and weeks? Why not hot springs in incessant action at the bottom of an ocean, as well as on our continents; and in the early times of violent igneous action might they not have poured out floods in intense ignition, exceeding by no little in extent and temperature the bubbling fountains of the present day? And if, as is believed, the ocean had in early times a higher temperature than now, the effects supposed would be the more easily produced.

We cannot doubt then, that here is heat sufficient to produce all the changes presented by the metamorphic rocks—heat enough to remould granite itself. In the words of Prof. Silliman, "we can see no reason for excluding water and other dissolving agents, acting with intense energy under vast pressure, and at the heat of even high ignition, from playing a very important part in crystallization;" and he continues by remarking that the metamorphic rocks of Lyell may thus have been crystallized.

The facts observed in the vicinity of dykes are well accounted for on these principles. Numerous instances of altered rocks might be cited from foreign publications; but our own country furnishes them in great numbers and of unusual interest.

Remarkable changes are described by Prof. H. D. Rogers, (Rep. N. J. p. 149,) as occurring at Rocky Hill, New Jersey, adjoining an extensive dyke of trap. The effects of heat in baking or hardening the intersected sandstone are distinct for a fourth of a mile from the dyke, beyond which the rock resumes its soft slaty structure and deep red color. Fifty feet from the trap, the sandstone is filled with various crystalline matters, which render it very unequal in texture and hardness. About one hundred feet off, the rock is a compact reddish or purplish sandstone, somewhat argillaceous, and is full of dark kernels or nodules of the size of a pea or less; one thin bed in the stratum contains small irregular cavities studded with crystals of tourmaline. The upper part of the same stratum is less altered in appearance but contains kernels of pure epidote, which continue to characterize the rock for a quarter of a mile, and at a quarry this distance off, besides the epidotic kernels there is a narrow band of nearly pure epidote about an inch and a half thick.

Many other similar examples are minutely described by Prof. Rogers in the same highly interesting report. In one, where the sandstone was altered to the same distance—about one fourth of a mile—the altered rock within fifty yards of the dyke, contained thickly disseminated crystals of tourmaline, some of them half an inch in diameter.

It is surely impossible that by conduction alone the heat of the ejected dyke should have been conveyed to so great a distance from the dyke, and in such a degree as to produce crystals of tourmaline and epidote, the latter one fourth of a mile from the source of heat. These effects must be imputed to the heated waters rendered hot by the eruption. The compact structure of the trap leaves no doubt that the eruption was submarine.

A moment's consideration of the circumstances attending such an eruption will place the subject in a clearer light. The submarine sedimentary deposit, whether a bed of sand or clay, is soaked with water; and between its layers, or in the cavities or caverns interspersed through or between these submarine beds, the waters are collected in large quantities. As the fissure opens, the melted rock flows up from below to fill it; the interspersed or permeating waters are heated by its sides and convey the heat far into the rock. The ocean's waters too enter the fissure as soon as opened, and meet the liquid fires on their ascent; hot, and in commotion from the ignition and violent ejection of the fluid rock, the waters are thrown into any open cavities in the walls, and thus aid in diffusing the volcanic heat. The superincumbent waters are next heated, and currents intensely hot spread around by the attendant convulsions, diffuse the heat far and wide over the surrounding sedimentary deposits, which are thus permeated and buried in the fluid heat. Through such influences, we may account for all the alterations and crystallizations above described.

The effects of the hot waters may probably be seen beyond the hardened portion of the rock, in the red color of the sandstone. We know that this is a common effect in modern volcanic regions, and can detect the same in many more ancient.

The blue and purple colors which the altered rocks assume near the dyke, arise, as in the common burning of bricks, from the excessive heat, which in part deoxydizes the iron or enables it to enter into new combinations. Tourmaline is one of the combinations which in the cases cited may have absorbed, or rather, have used up the iron, and as this mineral contains iron in the state of a protoxyd, we perceive that this deoxydation must actually have taken place.

Other examples might be cited from the sandstone and trap region of Massachusetts and Connecticut, but they illustrate no new principles. Besides tourmaline and epidote, garnets have been observed adjoining some foreign dykes.

I have purposely avoided mentioning facts collected during the cruise of the Expedition, but will cite one example in farther illustration of the principles here supported—the one which first suggested these views to my mind. It occurs on a small island at the mouth of Hunter River in New South Wales. A dyke of basalt, only eight feet wide, cuts vertically through the coal, clays and sandstone of the coal formation. The coal for six or eight feet is deprived of its bitumen, and as some of the layers contain considerable clay, it is baked to a hard black rock, containing masses of coal resembling charcoal. Beyond this distance it is unaltered. The soft clays are changed to a bluish chert, like flint in hardness and fracture, as far as the extremity of the island, which is about eighty yards from the dyke. The sandstones are also baked and hardened, but less distinctly at this distance than the clays. Such are the facts, and do we need other evidence that heated waters can and actually do alter rocks? Were the heat of the dyke *conducted* through the rocks from the dyke, to such an extent as to turn clays into flint eighty yards off, the coal surely ought to have been burnt or deprived of its bitumen to a greater distance than two or three yards? Moreover the clays show no evidence of fusion even in the vicinity of the dyke. Instead of the very intense ignition required to bake rocks so far from its source by conduction alone, without the intervention of water, a comparatively low temperature will heat the mobile waters sufficiently to produce the same effect.

Before applying these principles to granitic rocks, and the associated schists, I would request your attention to another mode in which heated waters modify the rocks that come under their influence.

We know that water intensely heated, will dissolve various earths and earthy compounds that are untouched by it when cold, especially salt water, which contains so largely of alkaline salts. The siliceous waters of the Iceland Geysers are examples of such effects at the present time; and judging from the deposit around these boiling springs, silica is here in solution and not siliceous compounds. The dissolving of silica must therefore be one of the first effects of the heated waters, and this petrifying or solidifying earth, as well as heat, is distributed through the adjoining rocks. In regions of eruptions, numerous quartz veins or silicified fossils often occur that may be imputed to this source, and the hardening of the sandstone or clay may depend to as great an extent upon the distributed silica as upon the heat. The silica is not all introduced from the external waters;—that in the clays themselves becomes partially dissolved and is redeposited as the water cools; in the same manner as the common waters of the ocean by washing through a bed of coral sand on the shores will after a while dissolve and deposit lime enough to cement it into a compact limestone. Why are the more ancient sandstones and grit rocks of our globe so much harder and so much more thickly intersected by quartz veins, if it be not due to the heated siliceous waters to which they have been for so many ages at various eruptions exposed? and at a period—*that* of their formation—when probably volcanic eruptions were more violent and numerous than now?

The heated waters at an eruption of greenstone, holding silica in solution which they have taken from the siliceous materials at hand, are in a favorable state also for the formation of the many zeolites and other trap minerals. In some amygdaloidal cavities the waters, as they cool, deposit silica alone. From a dense gelatinous solution layer is deposited on layer, and a coating of chalcedony or agate formed; afterwards the remaining silica, now in less dense solution, enters more slowly into regular crystals. The waters that penetrate to other cavities contain *compounds* of silica. Percolating through the rock it takes up lime, soda, potash, alumina, iron, &c. or the elements of the constituent minerals, and the solution thus formed fills the open amygdaloidal cavities, which finally on evaporation yield the various crystallized minerals common in these cavities. I do not attribute these crystallizations in all instances to the same period in which the eruption of the containing amygdaloid took place, and they may have been formed at a much later period. At some subsequent eruption in the vicinity, they may have been buried and permeated anew with hot siliceous waters, which thus gave rise to the amygdaloidal minerals. Some of these minerals are believed to be formed by the percolation of cold water through the rock, producing slow decompositions and forming new compounds. It should however be remarked that basalt or trap while still submerged undergoes but little change from cold water, except from abrasion. It is not until exposed to the atmosphere as well as moisture that they suffer much alteration or degradation from the processes of decomposition.

It is now very generally admitted that these amygdaloidal minerals are not of igneous origin cotemporaneous with that of the rock, and therefore no labored proof is required in this place. It is sufficient proof of this, that the cavities, which are inflations from steam or gas, must have been made before they could have been filled.

As has been stated, we suppose these trap minerals to be formed from the rock that contains them, while the hot waters are penetrating, and not to be taken or compounded from its surface; neither were they contained in the external currents of heated siliceous waters. The same minerals sometimes fill cavities in the adjoining sandstone, but to so short a distance from the trap, that we must believe them deposited from the waters that exuded from the sides of the dyke, and not from external currents.

Silica once received into solution will be held far below the temperature necessary for dissolving it. We might expect therefore various changes from the introduction of silica, where no evidence of heat can be detected. Some siliceous limestones may have been thus formed, without a crystallization of the lime; and cavities like those in the calcareous sandstone of central New York may have been filled in the same manner with quartz crystals, without any evidence of concomitant igneous action.

I do not claim that heated waters are the only means by which sedimentary rocks have been supplied with silica. The discoveries of fossil animalcules have opened a new source of silex, and this, as suggested, may possibly have been the origin of the flint in chalk.

On the principles explained we may account for the metamorphic porphyries and basalts of the Andes. They lie in a region of the most extensive volcanic action in the world, and while below the ocean—which was the case, as the tertiary rocks of the summit seemed to indicate, till the tertiary period had somewhat advanced—every eruption produced a heated sea around and through them, which hardened the porphyry conglomerates and sandrocks, till they were almost porphyry again. And it may be that the feldspar crystals imbedded in the metamorphic rock, instead of being the refuse from porphyry eruptions or porphyry degradations, were crystallized by the metamorphic heat.

Having discussed the action of heated waters on the various secondary rocks, and shown that the changes the structure of these rocks has undergone is attributable to this cause, we pass by a natural transition to granitic formations, and would endeavor to prove that no new cause is required for similar effects in them. With the knowledge of a power so efficient and so capable, so essentially connected with submarine eruptions and so frequent in its action, we need no other theory to account for any metamorphic changes. The same that holds good for red sandstone and will account for crystallizations of epidote and tourmaline, the same that accounts for metamorphic porphyry, is as good for metamorphic gneiss or granite. The structure of granitic rocks, their uniform compactness without an air-cell the world over—has often been urged as proof that they were formed under something more than atmospheric pressure. Beneath this pressure, whatever it may have been, we are safe in saying that the ocean was raised to a temperature far beyond that producing the crystallizations in the red sandstone. Mica and feldspar were also crystallized, and the sedimentary deposit was changed back to granite or to some of the associated rocks.

To explain this subject more completely, I will trace out some of the analogies that exist between the ancient granitic rocks, and the more modern igneous rocks and deposits.

A sedimentary basaltic sandrock or conglomerate is often so associated with basalt, as to make it obvious that they were formed together—the former arising from the sand or fragments carried off from the ejected basalt by the action of the water on the heated rock. An instance of this kind in Illawarra, New South Wales, is too plain to be mistaken. The basalt occurs in layers alternating with sandstone, the sandstone having been formed in the interval between different basaltic eruptions. The basalt is covered in places with a baked basaltic tufa or conglomerate, in some parts red and jaspery, and containing ragged masses of basalt, just as they were torn from the melted rock by the agitated waters.

The material of the metamorphic porphyries in the Andes was never clay like common clay deposits: it is merely fragmented or pulverized porphyry or basalt, either thrown out as a sand eruption, which is barely possible; or *secondly*, shivered from the rock while in fusion by contact with water; or *thirdly*, produced by subsequent abrasion. They underwent little if any decomposition before they were rehardened into rock, for as remarked, such decompositions go on but slowly if at all in cold water.

Let us now turn to the granitic series of rocks, and follow where analogy leads. Granite like porphyry is an igneous rock. In its era, granite sands were formed like porphyry sands, and restored by heat to metamorphic granite like metamorphic porphyry. Such are our conclusions. I use the word granite here, as a general term for this and the associated rocks, gneiss and mica slate, syenite and hornblende slate, &c. which I have shown may also be of igneous origin. These granite sands, like porphyry sands, were formed about the region of eruption in one of the modes pointed out, and in all probability were never clays, like the alluvial deposits of the present day. It has been too much the effort to make these schists out of common clays, and Boase, in his valuable work on Primary Geology, derives an argument against the metamorphic origin of the schists, from the fact that common argillaceous shales contain no soda or potash. But this argument will not hold if the view proposed be correct.

But let us trace out some of the changes which we may show to have thus taken place in the rocks now crystalline.

The change of deposited limestones to granular limestones has occurred in all ages of the globe, and is attributable, as in other metamorphic changes to heated waters, except in some instances where the alteration is confined to within a few feet of an igneous rock. With regard to primary limestones, a general survey of the facts, seems to evince that some of these were of igneous origin, like granite. If this were the case there must have been others of a sedimentary character formed at the same time with the deposits of granitic sand, and through the action of the same causes. These were recrystallized by the next discharge of heated waters.

The vaporization or exudation of magnesia from porphyries, as Von Buch supposes, in order to produce magnesian limestones, is a less satisfactory hypothesis than to suppose this earth introduced through heated waters containing magnesia in solution. Magnesia is one of the elements of sea-water, and when heated the water may have contained or received a much larger than the usual supply. But this and other theories are to a great extent if not entirely set aside by the discovery that recent coral rock in the Pacific often contains a large amount of magnesia. I suspected this fact when among the islands, both from their hardness and specific gravity; and having put some specimens into the hands of Mr. B. Silliman, Jr. for analysis, he has obtained the very interesting result, that carbonate of magnesia occurs in large proportions in some of these rocks. These analyses will be carried on with the different varieties of coral, and the conclusions which must necessarily be important, will appear in the Expedition publications. These facts will account for the occurrence of magnesia in limestones not crystalline, which is wholly unexplained by any theory of dolomization heretofore proposed.

But the coral rock examined and most compact magnesian limestones do not generally contain as large a proportion of magnesia as dolomite, in which there is about 45 per cent. of the carbonate. We may be compelled therefore to fall back upon a heated ocean—the same cause that crystallizes—for the source of the added magnesia.

A strong argument in favor of the metamorphic origin of much of the primary limestone and its dolomization by the method proposed is found in some of its associated minerals, and especially in the beds of serpentine or interspersed grains of this mineral.

Serpentine appears to be a deposit from the ancient ocean, connected with or proceeding from the granitic eruptions, and altered through the action of heated magnesian waters. Some evidence of this is seen in its position in beds and not in dykes; in its being so often associated with granitic and syenitic rocks, yet containing none of the elements of these rocks or but in small proportions, which should not be expected if they proceeded from simultaneous eruptions in the same regions; in its not altering the adjoining rocks like igneous ejections:—and more strongly still in its containing so large a proportion of water. It is a fact of much interest that rocks known to be igneous appear to contain no hydrated minerals except such as we may believe to have been introduced since their formation.

The origin of the zeolites and associated species has been shown to be subsequent to the ejection of the rocks containing them: that the same is true of the metallic salts is not doubted. Talc and chlorite may be suggested as exceptions; but there is reason to believe these minerals metamorphic. Von Buch suggested some years since that talc was mica altered by magnesian vapors; and in his account of the rocks of Norway and Lapland, as quoted by Lyell, gives several instances of the passage of mica slate into magnesian or talcose slate, and supposed to have been produced by the change of mica into talc. It is an interesting fact that chlorite is common in amygdaloids, filling cavities, where like other trap minerals it was deposited after the cavities were made, that is, after the ejection of the amygdaloid: and like zeolites it may be set down as one of the products resulting from the action of heated waters on the containing rock. It is therefore probable that in the granitic series it had the same origin. The chlorophyllite of Dr. Jackson, or hydrous iolite, is another hydrated mineral in the ancient rocks. Here the evidence, derived from its frequent association with iolite, is quite satisfactory that it has resulted from the alteration or the hydration of this mineral.

It has been argued that the water in hydrous minerals may be retained, under heavy pressure, in the same manner as carbonic acid is retained. But the analogy is false. Hot waters will not combine with carbonic acid; they even dissolve less than cold water. But water will combine with water or aqueous vapor none the less for being hot. There is no power in the pressure of an ocean to prevent one particle of water from combining with another. Any mineral species therefore, which in our furnaces may be deprived of its water, will be the sooner deprived of it in the same heat under a hot and heavy ocean.

The minerals in serpentine are mostly hydrous, and thus support the view of its hydro-metamorphic origin. These are Schiller spar, hydrate of magnesia, talc, nemalite, kerolite, Clintonite, &c. As both hydrous and anhydrous crystals are formed from aqueous solution we could not expect that all should be hydrous.

By the application of these views we may possibly discover hereafter, the cause of the peculiar distribution of specular and magnetic iron in New York, and of the hydrous oxyd of iron, or hematite, in New England, and why chromic iron is so common a serpentine mineral, while it is not found at all in other rocks. Indeed may we not now explain the occurrence of this chromic iron? Is it not for the reason that the green oxyd of chromium will not stand the dry fire, and cannot be formed, therefore, except through the agency of heated waters?

There is much reason, therefore, to believe that serpentine is a metamorphic rock, altered by heated waters containing magnesia and silica in solution. This rock has been compared to certain greenstones. Trap and the allied rocks have been shown by late analyses to consist of feldspar, augite or hornblende, and sometimes chrysolite and iron, together with *one or more zeolites*. The same hydrous minerals that fill amygdaloidal cavities, should be expected to fill all the pores or interstices in the rock itself, and this is what is now proved by analysis. The deposition of chlorite in the same cavities, shows that this mineral may also be disseminated through trap or basalt, and also that the basalt may possibly be more or less altered, and rendered more magnesian than is usual with this rock; and in this way we may conceive how either of these rocks should assume a serpentine character. I met with dykes of greenstone in Chili which could scarcely be distinguished from some serpentine.

It follows necessarily from these views that the granular limestone associated with serpentine must also be an altered or metamorphic rock. I do not mean that it was ever a compact limestone, like those of secondary formations; it may have been so or it may have been an aggregate arising from the wear or degradation of the igneous limestones. In either case it has been changed in its structure or recrystallized. Among the minerals in these rocks, the few hydrous species, are probably of aqueous origin. The rest may be either igneous or aqueous; more facts must be known before they can be distinguished.

There is evidence that some limestones after crystallization and the formation of some or all the imbedded minerals, were subjected anew to heat. The fused quartz, and rounded apatites of St. Lawrence Co., N. Y., have been so explained by Prof. Emmons, and I would only suggest as an addition to Prof. Emmons's explanation, that the heat was applied here through the waters heated by some eruption. As quartz is more soluble than feldspar, we may perhaps understand why we should find fused or rounded quartz enveloping unaltered feldspar.

Steatitic pseudomorphs of spinel, hornblende, pyroxene, &c. may perhaps be attributed to the same cause, that is, to heated magnesian waters, acting on spinels, &c. previously existing in the limestone. The steatitic spinels of Orange Co., N. Y.—which have a spinel skeleton, although mostly steatite—may have arisen either from a large intermixture of steatite with the material of spinel while in the act of crystallization, or from an incomplete alteration of the spinel into steatite; I suspect the latter to be the true explanation. The Rensselaerite of Prof. Emmons, appears to be a steatitic pyroxene, as suggested by Beck, or rather a pyroxene changed nearly to a compact steatite. Its crystals, which are often distinct, have the form and angles of pyroxene, and leave little doubt that such was its origin, although it constitutes rock deposits of great extent in northern New York.*

It is probably perceived that these views lead us to class the large family of talcose and chlorite rock, and steatite, among those that have been altered, like serpentine, by heated waters holding in solution magnesia, as well as silica. Excepting protogine, they are, in general, stratified rocks, and are classed by geologists in the metamorphic series. We do no violence therefore to existing theories in supposing them to have been altered by heat; and none we believe to reason or facts in supposing that this heat was administered in salt water. The granitic structure of protogine as has been shown is no evidence that it is not metamorphic. In a protogine in Northern California, I observed distinct clayey fragments, appearing to have been derived from some compact feldspathic rock, which had undergone partial decomposition. The fragments could not be mistaken, and marked the rock as undoubtedly of fragmentary origin, although I was but half willing to believe it at the time. The rock closely resembled granite, yet was more disposed to crumble down. Protogines generally undergo decomposition more readily than true granites. We find it difficult to account for this from their constitution, and may it not be owing to their metamorphic origin?

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* See Prof. Emmons's Geological Report.

Thus far in our argument, I have endeavored to show that as in those igneous regions where porphyries were formed, there are metamorphic phorphyries, so in those igneous regions where granites and the associated rocks were formed, there are metamorphic granites, gneiss, &c.; and we have considered the *evidences* that some granites, as well as some of the schistose associates, were originally of sedimentary origin, and have proved as we believe, that all talcose and chlorite rocks, steatites and serpentines, are undoubtedly metamorphic, and also some granular limestones. We have also argued, and may I not say proved, that heated waters, both the transfused and superincumbent, set in motion by the eruption, have produced changes in rocks at all ages of the world, and in the same manner as sandstones have been altered and filled with crystals, and porphyries remade, so the primary rocks have been recrystallized. Thus one and the same cause explains all igneous changes, and Lyell's grand principle, that existing causes explain past phenomena, is carried out almost to the very letter. We can no longer say in the words of Lyell, speaking of these early rocks, that "part of the living language of nature has passed away, which we cannot learn by our daily intercourse with what passes on the habitable surface." The language still lives—it is seen in every bed of once molten rock, that courses hill or plain throughout our globe; it is read in the many traces of fire, impressed in crystal characters on limestone, sandrock or shale; and is it not heard in those thunderings, muttered forth with the deep heavings of a hemisphere, which seem to tell of submarine eruptions, of ejected lavas beneath an unfathomed sea, of ignited fountains opened and waters in commotion, hot with lava fires, rushing through the rocks and over the regions around?

In drawing the last analogy between volcanic rocks and granitic, to which I would beg your attention at this time, I am venturing still farther and more deeply into the dark ages of our globe. Yet there is a ray of light penetrating even this obscurity:—at least, the light of volcanic fires, by which midnight views may be taken, and some glimpses caught of the operations that moulded a forming world.

In the volcanic regions of these modern days, as well as those of times past, when the fires now extinct were burning, the outer limits of the region of igneous action are more generally stratified, and more abound in tufas or sedimentary deposits than the centre, where the rocks are commonly igneous. Volcanic sands are blown by the winds to a greater distance from the centre of eruption than the lavas flow, or if submarine, are diffused farther by the waters. The centre, when exposed to view by subsequent convulsions and rents of the mountain, is sometimes pure solid basalt, with no trace of stratification, or division into lava beds.

I was particularly struck with this fact in the island of Tahiti, which is a type of many others in the ocean. The island has been so altered by convulsions and denudation, that no trace remains of an ancient crater. It is but a mass of sharp ridges and mountain peaks, the central about eight thousand feet high, and I never suspected its true nature till months afterwards, the modern and ancient igneous formations of the Sandwich Islands were examined. For six or eight miles towards the interior, the island consists of alternating basaltic conglomerates and tufas, dipping outward towards the shores at a small angle; beyond this, the basaltic layers are of great thickness—one hundred to two hundred feet being not uncommon; and the central peaks are solid to their summits without a trace of stratification—one solid mass of semicolumnar basalt—apparently the cooled interior of the volcanic mountain. I would refer to my forthcoming reports for a particular account of this interesting island.

In the Andes the same is every where exemplified. Ascending them, the traveller passes over conglomerates and pseudo-porphyries and allied rocks, till he nears the summit, where stand at intervals lofty mountain turrets of basalt, and rude crests of porphyry, acknowledged centres of the ancient volcanoes of this immense chain. Occasional dykes and subordinate crests are met with on the ascent, but the most magnificent views of mountain architecture are seen about the loftier portions of this range.

How is it now with granitic regions? When granite, gneiss, and the schists are associated, does not the grand central mass consist of granite or gneiss, and do not the schists occupy the more distant or outer portions of these regions? The exceptions to this prove the point we have established, that the schists may be either primitive or derivative rocks. But the general fact is too apparent not to have been noticed and described in all geological treatises, even the earliest. Argillite is commonly exterior to mica slate, and the talcose rocks and serpentine generally outside of the syenites, if they occur together. We should not however expect greater regularity than exists in acknowledged volcanic regions, and granites may be found inserted among all the schists, as dykes or mountains of trap and basalt are intruded among stratified deposits.

This subject is finely illustrated in Northern California. After passing twice from talcose rocks over syenite to granite, and in one instance back again to uncrystalline talcose or hornblende rocks, we made the same transition a third time. The features of the country were quite mountainous, and the mountains abrupt throughout the region of talcose and syenitic rocks; but the granite at the centres stood out in bold contrast with these serrated ridges, its lofty needle summits, white almost like snow from its albitic rock, peering above the green foliage of the forest about us, forming one of the grandest scenes I ever witnessed. The features of the region were too much like Tahiti not to be at once reminded of that island. From the granite, the route led over syenites and hypersthene rocks to talcose slate and a compact greenish rock resembling nephrite; from them to a stratified jasper of red and yellow colors covering large areas. The jasper rock is composed of layers two or three inches thick, which constantly coalesce and subdivide; it was obviously an aqueous deposit. It is associated very closely with talcose slate containing beds of serpentine, and not far off occurs the protogine to which I have alluded, and shown to be of derivative origin.

May we not safely set this down as a vast region of igneous action; the talcose rocks and slates and the jasper forming its outer border, and the granite the centre—analogous in some degree to the stratified circumference and compact basaltic centre of Tahiti? Does not the absence of crystallization in the outer rocks correspond precisely with this theory? Is not this jasper the final deposition of the silica into beds of ferruginous clay, where the waters had spread and cooled far from the centre of heat? and the talcose slates and serpentine associated with the jasper, do they not, by evincing the action of heated waters, bear us out in this supposition?

I would not be understood as implying that here was once a volcanic cone and a crater, for it is too well known that eruptions take place on a grand scale without forming cones; and indeed throughout the Pacific all the larger volcanic mountains are more like domes than cones, rising gradually at an angle of ten to fourteen degrees. But the inclination may have been still more gradual from the centre outward. The only point which I would sustain, is that the region in Northern California alluded to is a region of granitic eruptions, the granite peaks its centre, the jasper its outer borders. There may be many other centres in the same mountains, as volcanoes are sometimes crowded together, but in a hasty jaunt this could not be ascertained. Passing in only a single devious course through the region, it is impossible to estimate its extent. The jaspers were first met with about eighty miles from the granite.

The *facts* that have been presented lead us in conclusion to the following general views with reference to the earlier condition of our globe. I enter into no speculations with regard to the cometary nebula which has been supposed its condition when it first begun its revolutions in space: neither would I go back to the time when, according to some, it was a fluid mass resting beneath heavy vapors ready to settle upon its cooling surface—a supposition, by the way, no more hypothetical than that assuming the earlier rocks to be the remoulded material of another world—I come down to the era when the ocean existed. Igneous action was no doubt rife in those times, for however much we may wish to disbelieve it, there is evidence in almost every volcanic region, and especially such immense tracts as those of the East Indies and the Andes, that the present are comparatively quiet times. In the early age to which allusion is made, igneous action exceeded beyond doubt any thing of later date. These were times of extensive granitic eruption. Centres of igneous action were scattered over the earth or arranged in lines the sites of former fissures; for in almost all modern igneous regions a linear arrangement may be distinguished. From these centres or central regions, granite was poured out along with gneiss, syenite or some of the allied rocks; the ocean was agitated with repeated shocks, and heated by the opened fires; sands were shivered or worn from the ejected rocks and scattered far and wide around the place of eruption by the troubled sea; and after deposition, the permeating and superincumbent waters heated from the same or a subsequent eruption, finally recrystallized the deposits and studded them with new gems, or modified their composition through the magnesia, silica and other substances held in solution.

We cannot assert that there was ever a period after the ocean first covered the earth, in which no land appeared above its surface. Whether so or not, the lands, at a later period, had emerged, and in the shallower waters, but still under some pressure, porphyry and greenstone were ejected, taking the place of the granite or deep sea rock. Sedimentary deposits like those of the older fossiliferous rocks may have been in progress. Granite was in some parts still thrown out, and not till a very late period have these eruptions entirely ceased; indeed they may now be going on in the ocean's depths. But on the emerging lands, the granitic regions either ceased action entirely, or became porphyritic or basaltic. These rocks have continued to the present day, changing only by becoming more cellular where the eruptions were subaerial.

Thus we may believe that all the igneous rocks from granite to modern lava belong to one series, and were formed by one mode of action.

Partly in elucidation of this subject, and partly to suggest a doubt as to some accredited opinions with regard to the origin of certain mountain chains, I will conclude by presenting for consideration a few hints with regard to the great chain of mountains in western America.

The Andes and Rocky Mountains may be looked upon as originally a grand scene of granitic eruptions. It may have been an immense fissure, or much more probably a series of fissures ranging in general north and south, over which various granitic vents poured forth their granite floods. Granite peaks were thus formed, some of which still stand among the highest in northern America. The Wind River chain, according to the late surveys of Lieut. Fremont, is about thirteen thousand feet in elevation, and consists of this rock. As the land rose, granites were succeeded by eruptions of porphyry, trachyte, greenstone, &c. These continued the elevation of the submarine land, by adding new streams of molten rock and new beds of porphyry, sand and conglomerate; and together with sandstones and shales of granite origin, and limestones of different kinds, they continued building up the Andes while still submerged, or with the summits only above the waves. Sandstones, shales, salt deposits, beds of gypsum, limestones of Silurian and secondary ages, occur in the mountains, as on the plains of our continents; and they form in some parts, plains of immense extent, underlaid by horizontal strata. The depositions then took place in the same manner as on the low lands of other regions. The reason why a submarine mountain chain was formed, and not a flat continent, is the very simple one that the sources of all the material of the mountains lay nearly in one and the same line; for igneous eruptions threw out the material and piled up the mountain. There are two peculiarities in the structure of the mountain regions—the first is the great predominance of sandstones of porphyry origin, and which are not common in our plains; but this should be expected from the nature of the source where the material of the mountains was derived. The second is the broken character of the mountain heights. This is much less than is commonly believed, for a considerable portion of the range is covered with elevated plains. Yet there are gorges and valleys of great depth, and heights lofty and abrupt; yet nothing more than should be expected in a region of the most extensive igneous action in the world. There are variously displaced and tilted strata, attributable to local convulsions in the range.

In this manner we may suppose the Andes to have been mostly formed below the sea. Next, by gradual expansion below, or some other cause, an elevation commenced in the latter part of the tertiary era, if we may rely on the occurrence of tertiary rocks on the summit. Igneous eruption continued, but diminished with the elevation. The mountains slowly emerged, and continued raising their heads aloft till the continent at its foot had also appeared. During this elevation it was subjected to a tearing ocean, and thus its shattered sides were still farther gorged out. The elevation may not have been equal over the entire continent, and in all probability the eastern side participated but little comparatively in the motion, and was early dry land.

These views with regard to this chain of mountains are thrown out merely as general hints, and as thoughts to be hereafter tested by observation, rather than truths worthy of immediate confidence.

Art. XV.—*On the Temperature limiting the Distribution of Corals;* by James D. Dana, Geologist of the United States Exploring Expedition.

[Read before the Association of American Geologists and Naturalists, at Albany, April 29, 1843.]

I have before stated to the Association, that the temperature limiting the distribution of corals in the ocean is not far from 66° F. On ascertaining the influence of temperature on the growth of corals, I was at once enabled to explain the singular fact that no coral occurs at the Gallapagos although under the equator, while growing reefs have formed the Bermudas in latitude 33°, four or five degrees beyond the usual coral limits. In justice to myself I may state here, that this explanation, which was published some two years since by another, was originally derived from my manuscripts, which were laid open most confidingly for his perusal, while at the Sandwich Islands in 1840.* The anomalies which the Gallapagos and Bermudas seemed to present, were dwelt upon at some length in the manuscript, and attributed in the *latter* case to the influence of the warm waters of the Gulf Stream; in the *former* to the southern current up the South American coast, whose cold waters reduce the ocean temperature about the Gallapagos to 60° F. during some seasons, although twenty degrees to the west, the waters stand at 84° F. *Extratropical* currents, like that which flows by the Gallapagos, are found on the western coasts of both continents, both north and south of the equator, and *intratropical* currents are as distinctly traceable on the eastern coasts.† In consequence of these currents, the coral zone is contracted on the western coasts and expanded on the eastern; it is reduced to a width of sixteen degrees on the western coast of America, and of but twelve degrees on the east coast of America; while in mid-ocean it is at least fifty-six degrees wide, and about sixty-four degrees on the east coast of Asia and New Hol-

* The publication here alluded to we understand refers to an article by Mr. J. P. Couthouy, which appeared last year in the Boston Journal of Natural History.—Eds.

† The existence of these great oceanic currents was first pointed out to me by our distinguished meteorologist, Mr. Wm. C. Redfield, who kindly furnished me with charts of the same before the sailing of the Expedition.

land. The peculiar trend of the east coast of South America carries off to the northward much of the usual *south* intratropical current, and it is therefore less distinct in its effects, than the *northern* intratropical or Gulf Stream.

We have hence the remarkable fact, that the coral zone is fifty degrees wider on the eastern than on the western coasts of our continents. Such is the effect of the ocean currents in limiting the distribution of marine animals. These facts will be brought out more fully in the reports of the Exploring Expedition. The important bearing of these facts upon the distribution of fossil species is too apparent to require more than a passing remark. The many anomalies which have called out speculations as to our globe's passing through areas in space of unequal temperatures are explained without such an hypothesis. Instead of looking to space for a cause, we need not extend our vision beyond the coasts of our continents.

Art. XVI.—*On the Areas of Subsidence in the Pacific, as indicated by the Distribution of Coral Islands;* by James D. Dana, Geologist of the United States Exploring Expedition,—with a map.*

[Read before the Association of American Geologists and Naturalists, at Albany, April 29, 1843.]

The theory of Mr. Darwin with regard to the formation of atolls, or annular coral islands, has been fully confirmed by the investigations of the Exploring Expedition; but his regions of subsidence and elevation, and the conclusion that these changes are now in progress, appear to have been deduced without sufficient examination. Observations at a single point of time cannot determine whether such changes are in progress; they can only assure us with regard to the past. A series of examinations for years in succession is necessary to enable us to arrive at the grand deduction that the land in any part of our globe is now undergoing a gradual change of elevation. The views of Mr. Darwin respecting the rise of the South American coast, as well

* This map contains the track of the Exploring Squadron, and was intended to illustrate the article on the Exploring Expedition, published in our last number, but was unavoidably postponed.—Eds.

as that of the Pacific and East Indies, may well be received with some hesitation. According to my own observations, regions in which his theory would require a subsidence, have actually experienced an elevation at some recent period. I might instance several examples of this elevation in various parts of the Pacific. Suffice it to say here, that I found nothing to support the principle laid down by him, that islands with a barrier reef are subsiding, while those with only a fringing reef are rising; indeed facts most stubbornly deny it. Without entering upon the discussion of these facts, which, as they will appear in the Government publications, I am not at liberty to dwell upon here, I propose to point out what are the regions of subsidence which the coral islands in the Pacific indicate as having been in progress during their formation.

Before proceeding, I may be excused for adding here a few words in explanation of Mr. Darwin's theory with regard to the formation of coral islands. He rejects the unfounded hypothesis that coral islands are built upon the craters of extinct volcanoes, and proposes the following theory in its stead, which is supported by a minute as well as general survey of the facts.

The coral belt or atoll, he supposes to have been originally a barrier reef around a high island, like the reef around many islands in the Pacific. When the reef commenced, it could not have been extended to a lower depth than one hundred or one hundred and twenty feet, for this is the limit of the reef-forming corals. But if the island gradually subsided—so gradually that the corals could by their growth, keep themselves at the surface, the reef might finally attain any thickness, according to the extent of the subsidence. In this manner, subsidence might finally submerge the whole island, and leave nothing but the reef at the surface. Mr. Darwin points to instances in which only the mountain tops now remain above the ocean. Carry the process a little farther, and we have the coral belt surrounding its little sea—the usual condition of the coral island.

This theory, as is seen, supposes extensive subsidence. And so we remark must every theory: for without it, we could only have reefs one hundred and twenty feet in depth, instead of the great thickness they are believed to possess. It is my present object to fix the area of this subsidence, and suggest something with regard to the extent of it in different parts of the ocean.

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Washington Id
Sandwich Is
Hawaii
Columbia R.
Oregon
San Francisco
Azores
Madeira
Bermudas
Cape Verds
Galapagos
Callao
Lima
Rio Janeiro
Valparaiso
Rio Negro
Palmer's Land
Peacock
Sch. Flying Fish
Cook's farthest
Enderby's Land
Antarctic Land
Peacock enclosed
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On examining a map of the Pacific, between the Sandwich Islands and the Society group, we find a large area just north of the equator with scarcely an island. To the south, the islands increase in number, and off Tahiti, to the northward and eastward, they become so numerous, and are so crowded together, as to form a true archipelago. They are all, too, coral islands, throughout this interval. This then is a rather remarkable fact in the distribution of these islands. But let us look farther.

If we draw a line running nearly E. S. E. from New Ireland, near New Guinea, just by Rotumah, Wallis's Island, Samoa or the Navigators, the Society Islands, and thence bending southward a little, to the Gambier group, (see map,) we shall have all the islands to the north of it, with two or three exceptions, purely coral, while those to the south, are very generally, high basaltic islands. These basaltic islands are bordered by reefs, and these reefs are most extensive about the islands nearest this line. In the Feejees, the northeastern part of the group contains some coral rings, while the southwestern consists of large basaltic islands with barrier reefs.

Again, to the north of this boundary line, the islands *farthest* from it, are usually small, in many instances mere points of reef, a fraction of a mile in diameter, while some of the coral islands *near* the same line are thirty or forty miles in length.

Now a growing coral island or atoll, will gradually become smaller in diameter as subsidence goes on, and by the same process must finally be reduced to a mere spot of reef, or, if the subsidence is too rapid, that is, more rapid than the growth of the coral, the island will become wholly submerged and leave nothing at the surface.

On these principles, I base my conclusions. Along the equator, as explained, there is a large area containing few islands, and these small, while farther south, the coral islands are numerous and large: Is this not evidence, that the subsidence was either more rapid or carried on for a longer period in the former region than in the latter, where they are numerous and large?

Near the boundary line pointed out, stand some of these coral rings enclosing mountain tops, as islets,—as at the Gambier group. Does not this indicate that the subsidence was less here than among the islands *purely coral* to the north? and greater, than south of the line, where the reefs are more contracted and the high islands larger and more elevated?

Washington Island, (coral,) in lat. 5° N., is the last spot of land as we recede from our boundary line to the north-northeast. Beyond is a bare sea, to the Sandwich Islands. Is not this an area where the subsidence was too rapid for the corals to keep the islands at the surface?

It appears then that during this era, the Pacific from 30° N. to 30° S.—and perhaps beyond—was one vast region of subsidence: that subsidence took place most rapidly over the bare area between the Sandwich Islands and the equator, and less and less so as we go from this, to the south-southwest. At the boundary line pointed out, it was not sufficient to submerge many of the mountain summits, and south of this, the effect was still less.

This area covers at least five thousand miles in longitude and three thousand in latitude. The seas about the northwest coast of New Holland, show by their reefs, a contemporaneous subsidence, and they should probably be included, as well as some parts of the East Indies. Fifteen millions of square miles is not then an overestimate of the extent of the region that participated in this subsidence.

The region of greatest subsidence lies nearly in a west-northwest line, for we may trace it along by Washington Island far towards the arctic coast. The whole broad area of subsidence has nearly the same direction; for this is the course of the boundary line we have laid down as separating the high basaltic and the low coral islands. It is highly interesting to observe that the trend of the principal groups of islands in the Pacific, corresponds nearly with this course. The Low or coral Archipelago, the Society Islands, the Navigators, and the Sandwich Islands, lie in the same general direction, nearly west-northwest and east-southeast. It should be remarked that the Sandwich group, does not contain merely the seven or eight islands usually so called; eight or ten others stretch off the line to the north; some, small rocky islets, and others, coral, and the whole belong evidently to one series. I will not say that there is a connection between the trend of these groups and the area of subsidence; yet it looks much like it.

A further point may be worthy of consideration. The Sandwich group consists of basaltic islands of various ages. The island at the northwest extremity, Tauai, is evidently more ancient than the others, as its rocks, its gorges and broken mountains, indicate. By the same kind of evidence it is placed beyond doubt, that igneous eruptions on these islands continued to be more and more recent, as we go from the northwest to the southeast: at the present time the great active volcano, is at the southeast extremity of Hawaii, the southeast island. The fires have gradually become extinct from the northwestward, and now burn only on the southwest point of the group. At the Navigators, and I believe also at the Society group, the reverse was true; the northwest island was last extinct. Is there any connection between this, and the fact that low islands are numerous north-northwest of the Sandwich Islands and south-southeast of the Society? Does it indicate any thing with regard to the character of the subsidence in these regions?

The time of these changes we cannot definitely ascertain; neither when the subsidence ceased, for it appears to be no longer in progress. The latter part of the tertiary and the succeeding ages may have witnessed it. Although I am by no means confident of any connection, yet for those who would find a balance motion in the changes, I would suggest that the tertiary rocks of the Andes and North America, indicate great elevation since their deposition; and possibly during this great Pacific subsidence, America, the other scale of the balance, was in part undergoing as great or greater elevation.

But why if the western American coast was rising, do we find no corals on its tropical shores to indicate it? The cold extra-tropical currents of the ocean furnish us with a satisfactory reply.

Art. XVII.—*Abstract of the Proceedings of the Fourth Session of the Association of American Geologists and Naturalists.*

The fourth annual session of this Association was held, pursuant to the adjournment of last year, at the New York State Geological Museum in Albany, during the week succeeding the 25th of April, 1843. The next meeting will be at Washington City, on the 10th of May, 1844. The Chairman of the next meeting is Dr. John Locke, of Cincinnati; the Secretary, Dr. D. D. Owen, of New Harmony, Indiana.

Wednesday, April 26*th*, 10 A. M.—The Chairman of the meeting, *Prof. H. D. Rogers*, called the Association to order. The Secretary appointed at the last meeting, (Prof. O. P. Hubbard,) being unable to attend, on motion of Prof. J. W. Bailey, *Mr. B. Silliman, Jr.* was elected Secretary.

So much of the proceedings of last year were read as referred to the committees appointed to report on specified subjects at the present meeting.

No business being ready at the moment for the consideration of members, *Prof. E. Emmons*, by request, furnished the meeting with a general account of the principles of arrangement adopted in the great cabinet of geological specimens, collected during the geological survey of the state of New York, and in the midst of which the meeting was convened. He said the intention had been to make the arrangement as far as possible an expression of the natural order of succession observed in the various rock masses in the state; and as such it was both stratigraphical and sectional. No attempt had been made to combine with the stratigraphical a geographical arrangement.*

Mr. Emmons answered in reply to a query from the Chair, that he thought it was possible in a collection, to some extent to express both the geological and geographical distribution, while *Mr. Vanuxem* gave the opinion that any attempt to combine the two objects, would be productive only of confusion and difficulty. In the New York State Museum, the gallery had been set apart for a geographical arrangement.

Dr. Beck stated in reply to an enquiry from Dr. Houghton, that he had found it impossible to preserve a strictly chemical arrangement in the state mineralogical collection, although he had given up his attempts to this end very unwillingly. This led to a discussion between *Messrs. Beck, Houghton, Emerson, Silliman*, and others, on the general principles of mineralogical nomenclature and arrangement.

Dr. Owen then read a paper "on Geological Paintings and Illustrations.

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* By Art. XIII. of the Constitution, "All communications to the Association shall be presented in writing, and upon them discussions may take place which shall not be reported, but the facts presented in such discussions may be reduced to writing by the persons communicating them, and they may then be handed in at a subsequent session, when they may be entered on the records."—The Secretary has felt himself bound by this article to refrain from giving any fullness to the remarks of members who have not furnished abstracts of their observations, and the oral communications, as given in this abstract, are therefore necessarily much abridged, although contributing very greatly to the interest of the meetings.—B. S. Jr.

He called the attention of the meeting to a style of painting in distemper water colors, somewhat similar to scene painting, as particularly adapted for geological subjects. The charts, sections, and representations of fossils before the Association, were executed in this style; also the beautiful landscapes of Mr. Russel Smith, so that its capabilities could be judged of. This kind of painting was recommended on the score of cheapness, distinctness, the rapidity with which subjects may be executed, the ease with which corrections may be made, because it admits of execution on a large scale; because it looks equally well by candlelight and daylight, without even requiring any particular disposition or arrangement of lights; and, finally, because the paintings can be easily transported.

The materials employed were unbleached cotton, whiting, the commonest colors, and a little glue to fix them. The canvass was primed by whitewashing it with a mixture of water and whiting, to which about $\frac{1}{20}$ of dissolved glue was added. When the canvass was dry, a mere outline of the subject was sketched with a pencil, and the general tint and whole effect of light and shade brought out before any details were introduced. When the dead coloring was well executed, the finishing was easy. When the ground tint was dry, the details could be marked with pencil and put in with shadow tint. Then a few judicious touches with shade tint finished the design. If a clear and distinct effect was required, the color should be laid on a dry ground; if it was desired that the tints should blend together, they should be laid on damp ground. Any degree of contrast of light and shade could be effected in this style.

As a proof of the rapidity with which subjects might be executed in this way Dr. O. stated to the meeting, that he had in four months painted nearly eight hundred figures of organic remains, inclusive of lettering and stratification, and two large geological charts besides.

Dr. O. considered this distemper painting much easier than either oil painting or water colors on paper. The only objection to the style was its liability to injury by wet.

After the reading of Dr. Owen's paper, the subject of mineralogical classification was again introduced. *Mr. J. D. Dana* at the request of the Chair, stated that he had preferred the natural history arrangement, as being best calculated for instruction, and giving the most satisfactory view of the relationship of the several species and families. The arrangement adopted by him in his work on mineralogy, was based on the classification of Mohs, and was in truth mainly a chemical system, in which either the acid or the base was selected as the characterizing feature, according as one or the other most abounded. *Dr. Houghton*, *Mr. S. S. Haldeman*, *Dr. Beck*, and *Geo. B. Emerson, Esq.*, continued the discussion of the subject until the hour of adjournment.

The hours of session were fixed from 9 A. M. to 1½ P. M., and from 3 to 6 P. M.

Afternoon session, 3 P. M.—*Mr. J. D. Dana* read a paper "on the analogies between the modern igneous rocks and the so-called primary formations, and the metamorphic changes produced by heat in the associated sedimentary deposits." The points attempted to be established in this paper were—1st. That the schistose structure of gneiss and mica slate is no satisfactory evidence of a sedimentary origin, and is to be attributed solely to crystallization. 2d. That some granites with no trace of a schistose structure may still have a sedimentary origin. 3d. That the heat producing the changes that are termed metamorphic, was not applied from beneath by conduction from some internal source of heat; on the contrary it was through the heated waters of a surrounding ocean, which received their high temperature from the eruption itself. In other words, the metamorphic rocks so called are not *hypogene*, as explained by Mr. Lyell, but, to use corresponding phraseology, *epigene*, or analogous to other rock formations, deposited and solidified on the surface of the earth.

[As this paper by Mr. Dana is published entire in the present number of this Journal, (p. 104,) it is unnecessary to offer any abstract here.]

Mr. Dana's paper gave rise to much oral discussion, among several members, as to the possibility of heating a stratum of water at the bottom of the ocean, without at the same time giving rise to powerful upward currents which diffuse and dissipate the heat.

Prof. Espy said he had great difficulty in conceiving how the ocean could be heated to any extent in the way proposed by Mr. Dana, because of the disturbance of statical equilibrium in the ocean, giving rise to currents which would diffuse the heat.

Mr. W. C. Redfield said it was with reluctance that he entered upon this discussion, but in the course of it the assumption had been made, and seemingly admitted on all hands, that water at the bottom of the ocean, if raised in its temperature by the outspreading of lava or other cause, must immediately leave the bottom and rise to the surface. This was not necessarily so. He would illustrate his position by what often occurred in so rare and mobile a fluid as the atmosphere, and would cite a single example which he thought might be deemed sufficient. Humboldt in his travels in South America took note of the temperature at eighteen inches from a surface heated by the sun, and also at the level of six feet. Instead of finding a difference of temperature such as was due to the elevation or natural state of equilibrium, it was four hundred and fifty times greater, being 7° F. in four feet and a half of elevation, which could not have been the case did heated air necessarily rise immediately from the surface. We know, too, that in our American summers we have often a stratum of warm air on the surface, brought from a great distance by geographical transfer, so that the thermometer stands between 80° and 90° F., which continues even through the night, and day after day, at only some nine thousand feet beneath the snow line. Now if air heated above its ordinary temperature must immediately rise, how could this occur? And in so dense a fluid as water, confined under the enormous pressure of the ocean depths, how greatly lessened would be the chance of any speedy displacement of the heated stratum from the bottom of the deep sea?

Heated waters if spread in the bottom of a quiet sea could not permeate or rise through the incumbent colder waters, unless in a state of ebullition, or by the slow process of atomic displacement between the several planes of equal and differential temperatures, or by that insensible geographical transfer which probably occurs even at the lowest depths. Nor can the overlying colder waters permeate the stratum that lies beneath. Moreover, we know from the observations of voyagers, that in deep soundings made in some parts of the ocean, a warmer stratum has sometimes been found deeply imbedded beneath colder waters. This fact, which he had deemed conclusive, is of more certain value than any of our dynamical speculations.*

* Mr. Redfield annexes the following observations, made by Scoresby, and by the expedition commanded by Capt. Buchan in 1818, which showed an increase of temperature at increased depths, in certain portions of the arctic seas. It should be noted that the law of expansion in water, as its temperature descends below 40° Fahrenheit, does not apply to sea-water.

TABLE I.—*Scoresby's Observations.*

Temperature at surface.	BELOW THE SURFACE.		Latitude.
	Temperature.	Fathoms deep.	
31°	31°	13	79° N.
31	33.8	37	79
31	34.5	47	79
31	36	100	79
31	36	400	79
31	37	730	79
29.7	36.3	120	8
32	38	761	78

Mr. R. admitted that in numerous conditions which are familiar to our observation, heated air or water was constantly displaced by that of lower temperature and greater density. In these common cases the colder portions of the fluid, by favor of associated conditions, find access below the warmer portions, and force the latter to a higher level. But in the great aerial and oceanic masses it is often otherwise; and the truth was that mistakes of importance have sometimes been made in relation to this matter. He thought all would allow, that there was no more innate tendency in heated air or water to rise than in heated lava. An inferior stratum of heated water could not disobey the law of its own gravity, and might remain at the bottom of the ocean for a length of time that no one could determine.

Referring to another part of the paper of Mr. Dana—

Dr. C. T. Jackson suggested that the conversion of pulverulent coral into magnesian carbonate of lime, might have been effected by the action of magnesian springs, containing bicarbonate of magnesia. He would ask Mr. Dana if any such springs existed in or around the coral islands of the Pacific Ocean, or if any proofs of the former existence of such springs could be traced.

Dr. Jackson had witnessed with much interest Mr. Silliman's analysis of the corals, and of the magnesian limestone, formed from them, and it had occurred to him that a new theory of the formation of the magnesian limestones might arise from the facts observed by Mr. Dana. He was not entirely satisfied with M. Von Buch's theory of dolomi-

Table II.—*Observations of Buchan's Expedition.*

Temperature at surface.	Below the surface.		Latitude and date.
	Temperature.	Fathoms deep.	
33°	34°	15	From 79°.45 N. to 80°.27 N., in June and July.
34	34	30*	
34	34.5	35*	
33	34	60	
34	34.5	72	
32	36.7	73	July
31	35.6	83	"
32	36	94	"
32	35.3	95	"
31.5	36.5	103	"
32	35.6	108	"
30.3	36	120	"
30.5	36.5	142	"
32.5	36.5	173	"
32.5	36.3	185	"
31.5	37	237	"
32.5	35.5	270	"
32	35	331	"
33	43	700	May

* In harbor, west side of Spitzbergen. See Capt. Beechy's narrative of the expedition.

zation, by the action of igneous magnesian rocks. In some of the localities discovered by that distinguished geologist, thick beds of non-magnesian limestone existed between the dolomite and the igneous rocks, from which the magnesia was supposed to have been exuded. Von Buch supposed in such cases, that the pyroxenic porphyry penetrated the central mass at some point which was not visible, and thus conveyed magnesia to the superincumbent carbonate of lime. Dr. J. thought it more probable that the limestone, so situated, had become charged with carbonate of magnesia, by means of water charged with bicarbonate of magnesia; for the carbonate of magnesia would be deposited only at the surface, where its solution was freed from pressure. Heat, by expelling one equivalent of carbonic acid, would also cause a deposit of carbonate of magnesia from a solution of the bicarbonate.

In the instances cited by Mr. Dana, if no magnesian springs occur at the present time, we may reasonably suppose their former existence as one of the effects of volcanic action during the semi-extinct state of volcanoes. We may conceive of the disengagement of carbonic acid from various carbonates, acted upon by chlorohydric acid, or by sulphurous acid, both of which are abundantly exhaled from volcanic vents. If then carbonic acid gas was disengaged and discharged through comminuted volcanic magnesian rocks, such as tuffs, volcanic ashes and various pyroxenic rocks; those substances would be decomposed and their magnesia would be dissolved under pressure of the ocean, by carbonic acid, and would form bicarbonates, which would deposit the carbonate of magnesia the moment the solution was freed from pressure, or was acted upon by heat. Hence the various pulverulent carbonates of lime, corals, &c. might in this manner be charged with carbonate of magnesia. It is possible thus to account for the formation not only of the compact secondary magnesian limestones, but even for the formation of granular dolomite, but it is probable that the latter variety was rendered crystalline by the subsequent action of heat.

Mr. Dana said, in answer to Dr. Jackson's suggestion, that there existed no springs of hot water charged with carbonic acid or bicarbonate of magnesia, as Dr. Jackson had supposed, in any of the coral islands of the Pacific, and therefore such an explanation must rest entirely on hypothesis, and in reply to an enquiry from the Chair, he further stated that so far as Mr. Silliman had examined the corals brought home by him from the Pacific, they had proved to be pure carbonate of lime, but thus far only a few had been analyzed, not enough to be the basis of an opinion, as to the presence or absence of magnesia in them. He considered that the carbonate of lime was secreted by the powers of animal life from sea-water, and it was not impossible that magnesia might be secreted in the same manner.

Prof. E. Emmons, referring to the interest of the subject of metamorphism, advanced the opinion that the view of Mr. Dana was not sufficient to meet all the phenomena, and that cases existed where the changes could not be referred to his explanations. The influence of trap dykes had no doubt been overrated, and in his observations the alterations effected by them were confined to a very limited space, a foot or less, and not unfrequently the line of contact. He thought that there were metamorphic changes due to the influence of cold water transfused through and filling all the pores of rocks, particularly those changes which take place in lime-rocks. The siliceous nodules on the clay beds of Johnsberg, seemed to be in the position where they were formed, as the clay presented internal evidence of having never been moved, and this change from feldspar to clay, and the segregation of the silica, he deemed referable to the transfused water.

Mr. Dana said he recognized also the action of cold water as supposed by Prof. Emmons, and cited a bed of clay at the foot of a basaltic hill in New South Wales, containing nodules of siliceous matter, which he supposed proceeded from the decomposition of basalt.

Prof. H. D. Rogers found objection to the theory of Mr. Dana, on the ground that an internal fluid mass of molten lava, was more likely to convey heat to the superincumbent rocks, than an ocean of water heated to any considerable extent. Prof. Rogers said he would ask liberty to explain his views more fully at another hour.

The Association then adjourned till 9 o'clock, Thursday morning.

Thursday, April 27th, 9 A. M.—The Association met at the hour appointed, when the Chair presented a list of names from the standing committee, of gentlemen as candidates for admission to the Association, viz. Messrs. A. Osborn, of Herkimer, N. Y.; G. S. Weaver, of Cambridgeport, Vt.; Lyman Wilder, of Hoosick Falls, N. Y.; and Franklin Everett, of Canijoharie, N. Y. They were unanimously elected.

The Secretary then read a letter from Prof. O. P. Hubbard, the Secretary elect, to the Chair, stating that he was unable to be present at the meeting, from the pressure of other duties, and expressing his regret; also from Mr. John H. Redfield, secretary of the New York Lyceum of Natural History, containing a resolution of the Lyceum, inviting the Association to hold one of their regular annual sessions in that city at an early day.

It was resolved, "that the Association authorize the publication, in Silliman's Journal, of Mr. J. D. Dana's paper 'on the analogies between the modern igneous rocks and the so-called primary formations, and the metamorphic changes produced by heat in the associated sedimentary deposits.'"

Prof. Lewis C. Beck then read a paper "on certain phenomena of igneous action, chiefly as observed in the state of New York," of which the following is an abstract.

In this paper the author first adverted to the facts which are exhibited in various parts of New York in favor of the inference that certain primary rocks have been subjected to heat, subsequently to the crystallization of the imbedded minerals which they contain. Among the most striking examples of this kind, he noticed the locality in the town of Hammond, St. Lawrence County, where the crystals of apatite, feldspar and pyroxene in white limestone are often variously bent, and have their angles smooth and rounded as if by fusion, while crystals of zircon have been broken and their terminations moved from their original position. Similar appearances were referred to as occurring in the scapolite near Natural Bridge in Lewis County, and in the apatite and so called idocrase in Orange County; all of these minerals being found in the white limestone.

The author next noticed some peculiarities presented by the minerals occurring in gneiss and mica slate. In the former, whenever garnet is found the crystals are seldom perfect. Localities were enumerated in Westchester, Montgomery, Saratoga and Essex Counties, at which rounded or apparently fused garnets occur in the gneiss. On the other hand, when the same mineral is found in mica slate it almost invariably presents a perfect form and a fine finish. Such are the specimens from Dover, Dutchess County, &c.

From the facts adverted to, the author thinks we are warranted in the conclusion that whatever may have been the agency by which these minerals were originally segregated, the rocks in which they are found were subsequently subjected to a high temperature, sufficiently high at least to soften many of the minerals imbedded in them. The mica slate having been farther removed from the supposed source of heat, has its imbedded crystals more perfectly developed.

In noticing other evidences of igneous action, Dr. B. observed that there was one circumstance applicable to all the minerals found in the primary masses, with the exception of serpentine, viz. the total absence of water, at least in any thing like atomic proportions. On the other hand, this substance is a common ingredient in those minerals which are found in fissures of trap and greenstone, and in lavas which have been ejected from volcanoes. It was hence inferred that water was not evolved from a central nucleus during the earlier geological eras.

Several localities were referred to in New York in which the connexion between trap and serpentine, or the change of the former into the latter, is well exhibited. Facts were also stated in regard to the occurrence of the hydrous minerals both in trap rocks and in lavas. The general conclusion drawn from them was that the presence of water, known to be an almost constant condition of modern volcanic action, was no less so during the periods when the ejection of the trappean rocks took place.

The author also endeavored to show by a reference to facts connected with traps and lavas, that as we proceed to the interior of the earth there are arrangements of mineral forms quite different from those which characterize the lowest of the primary rocks which appear on the surface.

Dr. B. also submitted some remarks upon what has been called Antediluvian Climate, or the climate which is supposed to have prevailed during the fossiliferous era.

The author referred to several well known facts, to show that from the earliest periods of geological history down to the latest, the animals and plants afford the evidence that a higher temperature prevailed than is now observed, except in tropical regions. But he thought it had been hastily concluded that during these remote periods the refrigeration was *gradual.* The remains of animals found in the oldest of the transition prove that the arrangements of light and heat were the same or nearly the same as those which at present characterize tropical regions, and the same general conclusion was drawn from an examination of the remains found in the latest of the tertiary. There appears to be no gradation from more to less tropical forms in these immensely distant geological eras. A uniform or nearly uniform condition of things in regard to light, air, and heat, must have prevailed from one end to the other of this far-reaching series. Again, if it be admitted that the bowlder era was characterized by the prevalence of ice, at least in northern regions, the change from a tropical to a polar temperature must have been comparatively sudden.

The author upon reviewing all the facts, concluded that the theory of Poisson afforded a more consistent explanation than that which had been generally adopted.

Mr. J. D. Dana in reply to the reasoning of Dr. Beck remarked, that

Dr. B. argued that the zeolites might have been formed by the action of the volcanic steam on the rock: if this be possible it will by no means account for the large geodes of chalcedony in these rocks, which, consisting of layer deposited in layer, and often occurring in stalactites, was evidently formed from aqueous solutions. He also remarked that the numerous minerals of Vesuvius were not looked for in the recent eruptions, but in the older lavas of Somma, which had been exposed for some years at least to the action of moisture and other decomposing agents; and that as far as his observation went, lavas immediately after eruption do not contain hydrous minerals of any kind.

To Prof. Beck's remarks on the refrigeration of our globe, he replied that this theory of refrigeration must be admitted by those who believe in its once fluid state; but it cannot be asserted that this gradual diminution of temperature continued in progress till the recent period. Yet the diffusion of corals proves that the ocean was undergoing refrigeration in the tertiary period. The reef-forming corals do not grow where the winter temperature is below 66° F., and are in general confined between the latitudes 28° north and south of the equator. Yet we find coral rock on Porto Santo, near Madeira, where the water in winter often stands at 58°; and farther back in the tertiary period similar reef-forming corals occur in England, and in the oolitic period still farther north. Mr. D. alluded to a statement made by Mr. Couthouy at the meeting of the Association at Boston, that the limiting temperature of corals was 76° F., and took occasion to remark that Mr. Couthouy was indebted to himself (Mr. D.) for the views there advanced by him with regard to temperature limiting corals: and added that the temperature 76° F. was a mistake by Mr. Couthouy for 70°, the limit fixed upon by Mr. Dana when the views were communicated by him to Mr. Couthouy.

Prof. John Johnston of Middletown, Conn., observed that the crystals of beryl at Haddam, were singularly broken and distorted, in a manner similar to that mentioned by Dr. Beck as belonging to the crystals of apatite from Hammond, so well known to all mineralogists.

Dr. C. T. Jackson laid on the table specimens of metamorphic rocks bearing upon the question which had been started yesterday and continued to-day.

They were from Pequawket Mountain in New Hampshire. This mountain was upwards of four thousand feet high, consisting of a peculiar granite destitute of mica. It had burst through an argillaceous slate, which at the base of the mountain was broken up into fragments.

Half way up the mountain large masses of the slate, many upwards of 100 lbs. weight, were found imbedded in the granite; further up was a breccia of granite and slate, in which the latter is found varying in size from a diameter of several feet to less than one inch, mixed up with the granite in the greatest possible confusion; the slate standing in every possible position was aptly compared on the spot to the books of an extensive library scattered about. Passing over this breccia for the eighth of a mile, we find a finer breccia; at the summit only fine scales of the slate are found in the granite. We learn from this locality that the changes effected by the granite are very slight; the angles of the fragments are preserved, and there is no appearance of fusion. At the lower part of the mountain, changes in the structure are evident, but none in the form. The conclusion forces itself upon the mind, that here the granite was not intensely heated, or else that it is a very poor conductor of heat; that it was not liquid, but in the state of a thick paste. This leads to a consideration of the absence of vesicles, for the formation of which a pasty state is not favorable. The density of the paste may be estimated from the fact of magnetic iron in masses of several inches in diameter being found imbedded in it. If the granite had been in a liquid state, the iron would have sunk to the bottom. Dr. J. exhibited a hand specimen showing the drift scratches, which would be appreciated by those who knew the difficulty of obtaining small characteristic specimens. The mountain was covered with these scratches; they were from N. 10° W. to S. 10° E. The occurrence of metamorphic rocks is frequent in New Hampshire, Maine, and Vermont. An examination of the line of junction between the slate and granite shows that the eruption took place immediately after the deposition of the oldest argillaceous slate, and that it is much older than the granites of Switzerland.

Prof. E. Emmons then exhibited specimens, showing the effects of alteration by artificial heat, producing a columnar structure resembling basalt in a piece of the Potsdam sandrock, which had for many years been used as the hearth of an iron furnace; also a mass of sand altered by similar means. He instanced a trap dyke passing through the calciferous sandrock of Eaton, and converting the adjoining portions into a rock resembling white crystalline limestone. Prof. E. adverted to these as instances of change effected by dry heat. He then showed other specimens which he supposed to have been altered by the aid of heat and water conjointly; among these are specimens of calcareous spar, coated with chalcedony and other Rossie specimens. Prof. E. would divide the effects of heat under two heads; first, dry heat; and second, the conjoined effects of heat and water.

The President then requested *Mr. George B. Emerson* to take the chair, while he favored the meeting with some remarks "on hydrated minerals and antediluvian temperatures."

Prof. Rogers suggested whether the steam which so usually accompanies volcanic emissions, may not have furnished the water of the hydrous minerals found in the serpentine, referred to by Dr. Beck. We may easily conceive that this steam, by mingling with the lava matter in some localities and not in others, might cause the difference between the igneous injections involving hydrous minerals and those destitute of them. Organic remains afford evidence of the existence of water upon our globe at very remote dates, as early as the eruption of many of the ancient basalts and serpentines. A source of the steam existed, therefore, in periods of very ancient volcanic action.

Upon the subject of the ancient climate of the globe, Prof. R. avowed his dissent from the doctrine maintained by Prof. Beck, that the temperature of the ancient globe was uniform throughout the vast period of the secondary and tertiary races. He contended that we ought not to look for proofs of a very obvious refrigeration during any but a greatly prolonged period of geological time, since we must presume that the earth had already approximated to a statical condition of temperature at the time it became the abode of the earlier organic tribes. At the same time he appealed to the supposed habits of the ancient races, in support of the doctrine of a gentle and progressive cooling of the earth's surface. The hypothesis of Poisson, which explains the changes in the earth's general climate, by assuming the solar system to have passed successively into portions of space having different temperatures, being alluded to by Prof. Beck as offering a probable cause of the refrigeration in the past-tertiary period: Prof. R. stated that so sudden and transient a reduction of temperature must be considered as incompatible with the conditions of that theory. A translation of our system into a cooler region of stars we cannot suppose to arise but in a very gradual manner, nor would the globe part generally with its heat when so near its statical condition, but with an almost imperceptible slowness. He referred to the influence of the Gulf Stream on climate and its obvious dependence upon the physical geography of America, to show that local geological revolutions in the northern latitudes, causing changes in the distribution of land and water, would be sufficient to produce a temporary distribution further southward than usual of the arctic mollusca.

Mr. Gebhard also gave his views on the subject briefly.

On motion of *Mr. Emerson*, the Association accepted the invitation of the Mohawk and Hudson, and Troy and Schenectady

Rail Road Companies, to make an excursion on their roads on next Monday; and the matter was referred to the standing committee, to decide on the time and manner of the excursion.

Mr. James Hall then read a communication, "on wave lines and casts of mud furrows."

Mr. Hall presented specimens illustrating a paper read at the meeting of the Association in Boston, and exhibited some specimens of the Medina sandstone, presenting the markings which he called *wave lines*, from their perfect identity with the lines of sand deposited by the retiring waves upon a sea beach or upon the beaches of lakes.*

Every one must have observed that each advancing wave carries forward upon its crest a small quantity of sand, which at the moment of the cessation of the advancing motion, and at the commencement of the retreat, is deposited, marking in the most perfect manner the outline of the wave. (Mr. H. illustrated by lines on the black-board how these might be obliterated by a subsequent wave.)

These markings, often left on beaches for miles in extent on the ebbing of the tide or the dying away of the wind, might appear fanciful. He supposed that these minute tracings could hardly be preserved in the solid strata, but since other markings equally liable to obliteration were preserved, there was no reason why these could not be. They appear through successive layers of the sandstone, the layers varying from half an inch to two and three inches in thickness.

From the direction of these curves, the wind must have been from the N. W., or varying from that to N. N. W.

In connection with these markings, were the stranded shells of Lingula cuneata, which had been drifted ashore, and being an obstacle to the retreating waters, they presented all the appearances attendant on small pebbles in running streams, where the water scoops out a little hollow before it and on each side, while beyond is a little ridge of sand.

The markings to which the name of *mud furrows* has been applied, consist of little ridges upon the under surface of strata, varying in size from the finest possible lines or striæ, to that of ridges from an inch to six inches in diameter.

These always occur at the junction of a more sandy stratum with an argillaceous one below.

They have all the appearance of having been the filling of grooves or furrows made in the mud deposit below, after it had become partially indurated. No other cause could be assigned for their production.

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* Dr. Mantell in his Geology of the Southeast of England first called the attention of geologists to the occurrence of ripple marks on the sandstones of Tilgate Forest.

There is one situation at Goodwin's Falls on Cayuga Lake, where the lower side of an extensive stratum was completely covered with these casts of grooves. On Seneca Lake shore, twenty miles distant, and at precisely the same elevation, was a similar stratum, and probably the same one as seen on Cayuga Lake, and marked in like manner.

Mr. H. presented two specimens where these ridges were more than an inch in diameter. On the most elevated part of one of these, and for a foot or more in length, the surface is covered with small shells, while upon the surface on either side of the ridge, there were no shells. The inference was that the shells had drifted over the smoother bottom into the furrow and there remained till covered by the receding deposit to which they adhered. The direction of these ridges is always in right lines, and in a uniform direction. There are sometimes two or more systems of these ridges, similar to the groovings upon the surfaces of our present rocks.

Prof. Rogers inquired if these were not always at the junction of softer strata with more arenaceous deposits.

Mr. Hall answered that they were.

Prof. Rogers advanced the opinion that the coarse argillaceous matter carried over the surface by currents produced this grooving.

Mr. Hall replied that he had drawn the same inference regarding the smaller casts or lines, but had not satisfied himself of the origin of the larger ones.

Some general remarks were here made upon ancient denudation, during the deposition of the limestone of the Helderberg division, or about the period of the deposition of the Oriskany sandstone.

The Association then took a recess.

Afternoon session, 3 P. M.—The Chair proposed, from the standing committee, the names of the following gentlemen as members of the Association. Messrs. H. L. Kendrick, U. S. A., Francis E. Spinner, Herkimer, N. Y., Dr. Samuel Forry, U. S. A., Prof. Pearson and Prof. Foster of Union College, Schenectady, N. Y. These gentlemen were unanimously elected.

Prof. Bailey read a paper on the crystals formed in the tissues of dicotyledonous plants.

He stated, that in examining the ashes of many plants, great numbers of polygonal bodies were found, which subsequent observation showed to result from crystals. These crystals can easily be found *in situ* in the layers of the fibre of chestnut, locust, hickory, and many other trees. They also can be found in great quantities in even the hardest woods—as lignum vitæ, oak, mahogany, &c., and may be obtained isolated, by scraping the bark or wood into water, and picking out the woody particles. In the ashes of the leaves of many trees, every ramification of the vascular bundles was found marked out by rows of crystals. In very young leaves these crystals were only found along the midriff and a few of the principal veins. These crystals were shown to be referable to three principal forms :

1st. Form A, being modifications of a rhombic prism, oblique from an acute edge, and with the acute edges frequently replaced. This is the most abundant form among dicotyledons.

2d. Form B, to which were referred crystals with the lateral planes at right angles, as in hickory, iris, &c.; and,

3d. Form C, which is the same as the conglomerate ráphides of Quekett.

It was shown that the forms A, B and C, sometimes occur together in the same plants; that these crystalline forms all belong to the same system; that the identity of the corresponding plane angles rendered it probable that all these forms were derived from the same primary; and that the fact observed by Mr. Quekett of London, that forms A and B produced dissimilar effects on polarized light, might be due to the light being transmitted along different axes in the two kinds of crystals.

Tables, accompanied by drawings, showing the occurrence of these crystals in a great number of plants, were presented by Prof. B., who, in connection with these, remarked on the small amount of crystals in the Pine tribe, where they appear confined to the bark, and their apparent absence in some large groups of plants, as the Labiatæ, Compositæ, Graminæ, &c.

An account was given of the micro-chemical and other experiments, proving the composition of the crystals in all the plants contained in the tables to be oxalate of lime.

In remarking on the number of these crystals contained in plants, it was stated by Prof. B. that the number contained in a single square inch of the liber of many trees, as the willow, poplar, locust, &c., no thicker than a piece of writing paper, was at least a million, and that consequently the amount in the whole tree, including its bark, wood and leaves, must be enormous, and yet nearly all the trees of the forest were thus filled with crystals.

Remarks were then made by Prof. B. on the important questions concerning the causes and consequences of this vast production of crystallized oxalate of lime in the vegetable kingdom, and upon the development of heat and electricity which must attend its formation. He suggested as questions worthy of examination, whether oxalate of lime is a fertilizer, whether the fall of leaves, shedding of bark, &c. might not be nature's method of distributing this substance, as a fertilizing agent? whether it could be detected unchanged in soils? and what changes does it undergo during the decomposition of vegetable matter? He then stated that although oxalate of lime was the most common crystalline matter in plants, other substances also occur, and he showed drawings of cubical crystals in the cells of the potato, right square prisms in the cells of the outer layers of the onion, and flattened octahedrons, &c. in Rhus, all of which forms are incompatible with those of oxalate of lime. The examination of these forms and the acicular and other crystals of monocotyledonous plants, Prof. B. proposed to make the subject of a future communication to the Society.

Prof. B. then read an abstract of some observations on crystals, by M. Payen of Paris, which he had met with since the preparation of his memoir. His only knowledge of Payen's labors was derived from this brief notice in the London Microscopic Journal, which did not enable him to judge to what extent M. Payen might have anticipated his results. In cases, however, where the results obtained by M. Payen might be similar to those obtained by Prof. B., the latter could still claim originality, although not priority of discovery.

Dr. Jackson enquired of Prof. Bailey if he had observed similar crystalline bodies in Indian corn?

Prof. Bailey stated that he had not observed any definite forms in any of the grasses.

Dr. David Dale Owen then read a paper "on the Geology of the Western States."

The formations of the district described, belong chiefly to the eras of the bituminous coal, the carboniferous or mountain limestone, and the Silurian rocks of Murchison.

The order of superposition of the above formations, their dip and outcrop, were exhibited by two eighteen feet sections; one running from S. E. to N. W., from the Unaka Mountain in Tennessee, to the mouth of the Wisconsin river; the other from S. W. to N. E., from the Chickasaw bluff on the Mississippi to Pittsburg. The superficial area of each group of rocks was laid down on a large chart, colored to correspond with the sections. Over each formation, in their appropriate geographical and stratigraphical position, were figures of the organic remains on a magnified scale, so that they could be seen at a distance.

The most interesting points touched upon by Dr. Owen, were the description of the "Great Illinois Coal Field," equalling in area the entire island of Great Britain, and occupying the greater part of Illinois, about one third of Indiana, a northwestern strip of Kentucky, and extending a short distance into Iowa. A specimen of coal from this coal field was exhibited, which displayed in a beautifully distinct manner the woody fibre.

The absence of trap dykes and dislocations in the western coal measures, was adverted to, as a remarkable contrast to the coal fields of England, which are wonderfully disturbed by volcanic action and intrusive rocks. The position of the most productive salt springs was pointed out on the section near the base of the coal measures.

Rising from beneath the great Illinois coal field, and circumscribing it nearly in its whole extent, was a limestone, considered the equivalent of the mountain limestone of Europe, every where characterized by two very remarkable fossils—the Pentremite and Archimedes, and very important in practical economical geology, since *no workable seam of coal has ever been found beneath the rock containing these organic remains;* they are, therefore, trustworthy guides in determining the limits of the western coal measures.

Next in the order of succession followed a fine-grained sandstone and chert, interesting as being the repository of colossal beds of iron ore, not only in Tennessee, but in Kentucky and Indiana. It prevails in the region of country in these states known by the name of the Knobs. This formation has yielded some weak brines, but they have not been able to compete with those procured in the coal formation.

The lower part of this formation was supposed to be the representative of the Devonian system of England, and the Chemung group of New York.

The whole of the above described groups of rocks rested on a black bituminous shale, very like coal shale, but unaccompanied by any perfect seams of coal, and considered equivalent to the Marcellus shale of the New York geologists.

The above comprised one half of the paper; the reading of the remainder was postponed for a future day, and Dr. Owen concluded by drawing up a summary of the foregoing in the form of a series of queries calculated to draw forth the comparative observations of others in distant parts of the west.

Mr. James Hall then presented to the Association, a section intended to show the western relations of the New York strata, as developed in Ohio and other western states.

The Chair mentioned to the meeting, that G. B. Emerson, Esq. would favor the Association with a lecture on the importance of natural history as a branch of education, on Friday evening, at 7½ o'clock.

The Association then adjourned.

Friday, April 28th.—A letter from *Walter R. Johnson, Esq.* to the President, was read, in which he expressed his regret at being prevented from attending the present meeting, and enclosing his contribution towards the printing of the Transactions.

Mr. Nicollet then read a paper "on the cretaceous formation of the Missouri River."

Commencing at Council Bluffs and proceeding up the river, it was the design to give an intelligible view of the formations exposed on the river banks. But, previously, for the purpose of connecting the cretaceous formation with the geological formations at the east, he stated that the carboniferous formation could be observed from St. Louis up the river; that at Council Bluffs and Riviere des Moines on the Mississippi, the same carboniferous fossils were found. That south of the Missouri there was a continuation of the carboniferous and Silurian systems. In Iowa, the representative of the Silurian system contained a great number of fossils characteristic of the formation, while at the south, the mineral region of Missouri contained none or very few fossils, and we infer that it belongs to the same system, merely from the mineral characters. That owing to the topographical character of the country, no vertical sections were exhibited.

Mr. N. commenced his detailed observations, by giving a sketch of the topography of the banks of the river. The course of the river was continually changing—so much so that many of the bends described by Lewis and Clark could not now be recognized, and some laid down by himself on the map exhibited, in 1839, had already disappeared. He had, in fact, lately learned that the great bend opposite Council Bluffs, had been cut off. And hence it resulted that the travelling distances of his party, differed much from those of Lewis and Clark. Soon after leaving the Tchansansan, or Woody River, the hills recede from the banks, but after two days' journey the river again washes their base, and the carboniferous limestone again appears in place. Near the mouth of the Sioux River, the carboniferous rocks again appear, and this he considers their true limit, not having met with them or any of the older fossiliferous rocks beyond this locality. At this place the rocks in the bluff, consisting of argillaceous shale and carboniferous limestone, were seven or eight feet in thickness.

On reaching the Ayoway River, a great change takes place in the vegetation and in the geological formation of the country. At the third bluff above the mouth of the river, to which the name of Dixon's Bluff had been given, in honor of one of his most faithful and devoted guides, occur a group of rocks which we called Dixon's group. It consists of—

A. Argillaceous limestone, containing *Inoceramus barbarini* very much compressed; three feet of this rock appears above the water; how deep it extends below he had no means of ascertaining. Disseminated through it is iron pyrites in great abundance.

B. A calcareous marl, generally from thirty to forty feet thick, containing few fossils; an Orbicula and fish scale were found in it.

C. A ferruginous clay containing selenite in acicular crystals, and of great variety and beauty of form.

These rocks, which are always found thus associated, constitute the base of the cretaceous formation of the Upper Missouri; it extends from the Sioux to the Ayoway River, twelve miles, and rests immediately on the carboniferous limestone—all the intermediate strata appear to be wanting. Along this line, A disappears on account of the rise of the country, and between B and C is found a thin layer of fibrous carbonate of lime, containg *Ostrea congesta* of Conrad. The specimens of marl sent from this place have been found by Prof. Bailey to contain microscopic multilocular Foraminiferæ, the same as found in New Jersey. Here too, a white indurated clay alternates with C. A little beyond the mouth of the Tchansansan River (or the "continued Wood River") commences the Coteau de Missouri, along which, on the right, is the American desert; along here on both banks occurs this formation.

D. The last member of the cretaceous group, is a plastic clay, two hundred feet thick, divided into unequal beds by clay containing nodules of argillaceous iron; it also contains fragments of limestone, the different varieties of which were indicated for the benefit of future geologists. The plastic clay is full of new species of Ammonites, Inoceramus, Belemnites, Baculites, &c. and also remains of vertebrated animals which have been described by Dr. Harlan. Dr. Morton has described and figured some of these fossils in the Journal of the Academy of Natural Sciences of Philadelphia. Of the few species collected by Mr. N., four have been identified with those of the same formation on the Atlantic. This formation exhibits the geological features of the Upper Missouri for four hundred miles, retaining the same lithological and fossil character. From specimens and information he thinks this formation extends much farther to the west and northwest.

Nr. N. concluded this portion of his paper by giving an account of the topography of the country, and spoke in terms of high commendation of the accuracy with which Mr. Catlin had depicted the scenery in his interesting volumes. In the midst of the clay banks, and from the summit of the hills, dense smoke is frequenly observed to arise from crevices in the plastic clay; he called them pseudo-volcanoes. From this fact, and from the occurrence of the light spongy material brought down by the waters, and strewed along the shore, many have erroneously supposed that volcanoes existed on the Upper Missouri. This, however, is a mistake. The smoke and pseudo-pumice, he supposed to proceed from the same source, the ignition of the iron pyrites and lignite, which are found in great abundance in the plastic clay. These pseudo-volcanoes were not in action during the journey of Lewis and Clark, nor during that of Mr. N.; but every thing concurs to prove their existence; the unanimous declarations of the Indians and *voyageurs*—the blackened and sterile appearance of the more recent, and the name given to the district, *cote brule*, or "burnt coast." The Indians call these spots *mankah zita*, or "smoking earth"—thus recognizing their difference from volcanoes, which would be called burning mountains.

Mr. N. exhibited in illustration of his paper, his splendid map of the hydrographical basin of the Upper Mississippi, embracing an area of 10° of latitude and of longitude, prepared for government and engraved on its order. This map is not yet published; but will soon appear in connection with the report of Mr. Nicollet, accompanied by a map of the same territory, on a reduced scale, with the topography exhibited thereon, as directed by a recent order of the Senate. Some notion of the extent of the territory embraced may be formed from the statement, that it is nearly once and a half as large as that of France.

Mr. N. stated, for the benefit of future collectors, that the fossils and selenites, &c. could be obtained immediately after rains without the labor of digging, and that if favored with a brilliant sun they occasioned a beautiful appearance by their glittering lustre. Hence the name of "shining hills," so appropriately given by the Indians, and by them mentioned to the first white explorers. This name had been supposed [erroneously] to apply to the Rocky Mountains.

Prof. Rogers enquired of Mr. Nicollet, if there was any evidence of the existence of a saliferous formation south of the Yellowstone River.

Mr. Nicollet replied, that he had no definite knowledge on the subject, having never been there.

Dr. Houghton, referring to the rocks about the great Salt Lake, stated that these formations belonged to the strata of the lower portion of the coal series.

Prof. Rogers said, that in confirmation of the remarks of Mr. Nicollet, as to the pseudo-volcanoes of the Upper Missouri, he would state that he had been informed by fur-traders and others, in whose statements implicit reliance could be placed, that high up on the Missouri and Yellowstone, there were these hills of burning clay, and that after they were burnt out they sunk down, leaving permanent memorials of the fact. As among the species of fossils, (perhaps not more than twenty in number,) collected by Mr. N. four proved to be identical with those of the Atlantic cretaceous formation, and as it was probable that future researches would show a greater conformity of fossils, we had exhibited before us one vast formation, the extremes of which were apparently identical in date, and the deposit of one great sea. This became the more interesting from the difference which existed between it and that of Europe, as among two hundred species of fossils which had been found in cretaceous formations of the Atlantic, but one, and that even doubtful, has as yet been identified with an European species. The evidence appears strong, that the fauna of the formation on the two continents was dissimilar, and that there was a general identity of the fauna in America, as shown by a comparison, by Dr. Morton, of the cretaceous fossils of New Jersey with those of Upper Missouri. The Atlantic fossils corresponding with those of the Upper Missouri, as far as observed, are as follows: *Ammonites placenta*, (Dekay,) some very large, also found in New Jersey; *A. Conradi*, (Morton,) in Alabama; *Baculites ovatus*, (Say,) in New Jersey; *Belemnites mucronatus*, found in New Jersey, Alabama, and English chalk.

Mr. Redfield mentioned the occurrence of a cretaceous fossil, the *Exogyra costata*, (Say,) in the city of Brooklyn, opposite the city of New York. It was found about sixty five feet below the surface, in excavating a well in or through the drift on Brooklyn heights. This is believed to be the first authentic memorial of the cretaceous formation found in the state of New York. The specimen, a very fine one, is in the cabinet of Dr. John C. Gay.

The Chair stated, that the Association was without a treasurer, Dr. Locke having been detained from attending the present meeting. *Dr. Douglass Houghton*, of Detroit, was then elected to the office; and the accounts and funds of the Association were handed to Dr. H. by the Secretary.

The Secretary then presented to the Association a collection of corals and coral rocks from the West Indies, received last year from Mr. Peter A. Brown, of Philadelphia.

The Chairman remarked, that as it was understood that the Secretary was engaged in a series of chemical examinations on the corals and coral limestones of the Pacific, it might be interesting to make some comparison also with those of the West Indies, and therefore proposed that the specimens presented by Mr. Brown be given to Mr. Silliman for this purpose, which was passed.

Mr. James Hall read a communication "on the geographical distribution of fossils in the older rocks of the United States."

Mr. H. commenced with some general views as to the formation of sedimentary rocks. They required for their deposition materials to furnish the detritus sand or mud of which they were made up; bodies of water in which these materials could be suspended, and from which they might be deposited according to their density—sand first, and the more finely comminuted particles forming mud afterwards. As to the marine exuviæ they contained, these would depend, as to distribution, number and character, materially upon the depth of the ocean, the distance from the land or shore at which the matter enveloping them was deposited, and consequently where they had lived, and the nature of the bottom upon which they had their existence.

In connection with this he might be allowed to mention, that Mr. Dana had informed him that the forthcoming reports of the Exploring Expedition would contain numerous facts as to the distribution of shells and crustaceæ at the present day. He had met with no essay upon this subject, and the few scattered facts which were to be found in different authors rather stimulated than satisfied curiosity. The few facts upon the subject which had fallen under his observation, were now offered, in the hope of calling the attention of others to the subject, rather than with the expectation of furnishing any complete solution of the problem.

The rocks which in England are called Silurian, and which in this state we have termed the New York system, [under this name are comprehended the Cambrian, Silurian and Devonian systems, which are now considered as forming one system,] are known to be of great extent in this country. The researches of Murchison in Europe show their extent in that continent, and some of their fossil characteristics are known to occur abundantly in Siberia. The perfect development, wide range and comparatively undisturbed state of these rocks in the United States, afford excellent opportunities for studying the condition of the ancient ocean, from which they were deposited over wide areas. He exhibited a section extending from the eastern part of New York to the Mississippi, in which he had endeavored to point out the comparative developments of the different strata. Having travelled over this ground, he was acquainted with the lithological and fossil characters of the rocks. He first considered the changes in lithological character which these strata exhibit in proceeding westwardly, and their greater or less development.

The lower rocks exhibited by the section, and which were well developed in New York, he had met only at a few points westward. They occur at Frankfort, Ky., and according to Dr. Owen, on the Mississippi at Prairie du Chien, and at the mouth of the Wisconsin. Their development at Frankfort cannot well be ascertained. The same fossils which typify these rocks in New York are found in Kentucky and at the mouth of the Wisconsin. We thus have a uniform composition, nearly similar developments and like fossils, extending this great distance; and must, therefore, admit a uniform condition in the depth and bottom of this primeval ocean. Already do we know that this extent east and west was not merely a margin, but that the same rocks extend into Canada and stretch west beyond Lake Huron; and Profs. H. D. and W. B. Rogers have identified them in Pennsylvania and Virginia. (Specimens from some of the different localities were then exhibited.)

The next was the *Hudson River group*, made up of *shales*, shaly sandstones, and sandstones with little calcareous matter. This also is seen in Ohio, Indiana, Kentucky, and on the Mississippi above Dubuque; but with a change of character—having become more calcareous, so much so as to have received the name of Blue Limestone. Its thickness is apparently less than in New York. In New York its typical fossils are of the Conchifera, while with some exceptions the *Brachiopoda* are rare. At the west, the latter are the predominating fossils; while the fossils which are characteristic in New York, are the least prominent at the west. Corals and crinoidea are also far more abundant throughout the group to the west, and indicate a source of the calcareous matter. The crustacea also appear in greater numbers, and trilobites different from those of the Trenton. Of the *Oneida conglomerate*, the *Medina sandstone*, and the *Clinton group*, we have scarcely any definite traces at the southwest. The *Niagara group*, which is next in order, consists in New York of shale and limestone, both being highly fossiliferous, the former containing corals, crinoidea, shells and trilobites—the latter, chiefly corals. At the west the shale has disappeared with its fossils, and the limestone much increased in thickness, and, as in New York, abounds in corals. Here we have the calcareous matter increasing as we go west. This mass in the centre of New York, only a few feet thick, is two hundred and fifty feet at Niagara Falls, and in the western states little less than a thousand. The *Onondaga salt group*, which in New York is upwards of a thousand feet thick, has thinned out to the west so as to become almost insignificant. Succeeding these we have the Helderberg limestones, an extensive group abounding in fossils. They, however, except the two upper limestones, all disappear before we leave this state. And these two are well developed in Ohio, Indiana and Kentucky, and also on the Mississippi. The general character remains the same, with the exception of being lighter colored—the fossils are identical.

Then come the *Marcellus shales* and the *Hamilton group*, which form an important part of the series in New York. Its thickness is nearly a thousand feet, and contains more individual fossils than all the rocks below them. In the eastern part of this state these groups consist of slaty and sandy shales and impure sandstone—westwardly, the sand diminishes and the mud increases. In Ohio, &c. the lower member alone, a black shale, is visible, and it has thinned down to one hundred or even fifty feet, and has apparently lost all its fossils. Here we have a better instance of the gradual change and final disappearance of fossils than is elsewhere afforded. The lithological characters also change as we proceed west, and in accordance with the laws of mechanical deposits. If the origin of the deposit was at the east, we have first the sand—then mud intermingled with sand—then mud alone, and beyond, the clear blue ocean without turbidness.

We may then infer, that in this great ocean, greater depth and quiet were found at the west, and at the east a shallower sea and proximity to land; and we have here exhibited the influence of the conditions mentioned at the beginning of this paper. With slight exceptions all the intermediate deposits to the Old Red Sandstone may be considered as one group, made up of shales, with thin alternating sandy layers and flagstones. Going westwardly the shale increases and the thickness diminishes. In the centre of this state it has reached its maximum. And in these also the same change in the character of the fossils is evident as in that below. Large numbers of Fucoides continue in this group for a great extent. In Ohio, this group has diminished from two thousand in New York, to four or five hundred feet in thickness, and there is even a greater diminution of its fossils; and this gradual decrease of thickness and number of fossils continues, so that in Indiana it is almost non-fossiliferous. Here terminates the *New York system*, the rocks of which attain a greater development than perhaps in any part of the world. (Mr. Vanuxem thought the Old Red Sandstone should not be excluded from the system of New York rocks.)

We see, then, uniformity of lithological character in the calcareous formations is accompanied by uniformity of fossils, and *vice versâ;* and that a change in mechanical deposits at different distances from their sources is also attended with change in the fossils. We find too, in the first period, which includes the Hudson river group, a higher degree of vitality over the west portion; and in the second, that of the calcareous deposits an organization more fully developed here at the east. In the third division, still greater difference is perceptible in New York than west or south.

The sea here emphatically teemed with life, while at the west it was cold, dark and deep, with scarcely an inhabitant. And we may also learn that the same rocks may at one point be highly fossiliferous, and at another destitute of them, and may thus often be mistaken for different rocks, if sole reliance is placed upon fossil character. The next rock exhibits even greater changes in the organic contents and comparative conditions of the eastern and western extremes of this ocean. These consisting of sandstones and shales, red, green and gray, with a few shells, fish scales and fragments of bones, are equivalent to the Old Red Sandstone of Europe. In the eastern part of New York, it forms the Catskill mountains, and in Pennsylvania, it is about two thousand feet thick, but it can scarcely be identified beyond the Genesee River, nor do we know that it reappears at the west. Then follows a coarse conglomerate, which, after the disappearance of the Old Red, rests upon the *Chemung group*. In Indiana, however, the rocks of the Chemung are succeeded by a fine sandstone, which contains beds of oolitic limestone, with fossils entirely different from any at the east. Succeeding this limestone we have the conglomerate, which at the east rests upon the Chemung group.

Thus we have the great coal formation resting in one place upon the *Chemung group*, at another upon the Old Red Sandstone, and at another upon the limestone, which underlies the coal basin of the west. He concluded by remarking, that from what has been said it was evident that this immense ocean was bounded on the east by a continent which supplied all the mechanical deposits; that during some periods there was a cessation of these deposits, and calcareous deposits were produced. The influence of these deposits did not extend throughout the whole area, and beyond their reach flourished corals and many other beautiful forms in security, which thus prevented them from extending beyond their own domains.

[Mr. Hall's paper was read partly in the morning and partly in the afternoon, but it has been given above without division.]

Dr. Houghton, referring to the paper just read by Mr. Hall, said that the sandstone of Lake Superior, lying east from Kewuna Bay, dips at a moderate angle to the south, or a little east of south, and passes under a limerock which he considers to be the equivalent of the Trenton limerock of New York; while those conglomerates and sandrocks lying westerly from Kewuna Point, and flanking the trap on the north, dip to the north, mostly at a high angle. These last mentioned rocks are probably contemporaneous with the New Red, and were doubtless deposited during the long period that marked the upheaval of the trap, as trap was found, in a very coarse condition, to enter largely into the fragmentary material, composing, more particularly, the lower strata of these rocks.

Dr. H. proceeded to state, that the Pentremite limestone mentioned by Mr. Owen, in the part of his paper read yesterday, seemed gradually to thin out as it went north, and to exist, after we lose it in continuous beds, in strips and patches.

Prof. H. D. Rogers then said, that if he understood Mr. Hall, he considered the black bituminous slate of the west, as the equivalent of the Marcellus shale and Hamilton group of the New York system combined. A careful examination of that well characterized stratum had led him and his brother, Prof. W. B. Rogers, to regard it rather as the representative of the Marcellus shale alone. They had during the last summer and autumn traced both the Marcellus shale and the Hamilton through Pennsylvania and Virginia and East Tennessee, to their southwestern terminations, the former thinning out in Virginia, and the latter abruptly disappearing with the ending of the Clinch Mountain in an enormous fault in East Tennessee. The Marcellus shale, unaccompanied by any indications of the Hamilton group, was subsequently identified by its fossils during the same tour by Prof. H. D. Rogers, at Canary Fork, the Harpeth Hills near Nashville, and other localities in Middle Tennessee; also in Kentucky, southwest of Louisville, and at New Albany in Indiana, the fossils most frequently met with being the *Orbicula corrugata*, and a minute *Lingula.* Prof. R. thought that the Hamilton group—of which he could discover no trace by organic remains in the west, being in New York so remarkably replete in fossils—would in accordance with a general law of our strata continue some, at least, if not many of its species, as far westward as its sedimentary materials.

He next adverted to his having met with what he considered the *Dictuolites Beckii*, a Medina species, and *Fucoides biloba*, a Clinton form, in the so-called blue limestone formation of Cincinnati, and the *Strophomena rugosa* of the Clinton and higher groups of New York in the same blue limestone at Madison, Indiana.

In reply to Prof. Rogers, *Mr. Hall* said he did not consider the black bituminous shale of the west as the equivalent, but as the only representative of the Marcellus shales and the Hamilton group.

The President replied, that there was no equivalent of the Hamilton group at the west; the Marcellus shales were far more persistent, and were found where the other was entirely wanting. And such, too, was the case with regard to some other of the New York formations. There was another fact worthy of observation, that those species which we are apt to deem characteristic, are found to make strange aberrations, and may occupy dwellings, to which we perhaps think they have no title. Thus he had found in the blue limestone of Cincinnati the *Dictuolites Beckii* and *Fucoides biloba*, fossils which were typical of the Hudson River shales, associated with those of the Trenton and Clinton groups. Now he would ask, were these species created at an earlier period in the western ocean, and did they remain there during the convulsions which had elevated the New York rocks? Or did the earlier species of the Hudson River group continue on in the west, and thus become associated with the beings of another era? One or other of these suppositions must be correct, if the species have been correctly identified.

Mr. Hall in reply to what had fallen from the President in relation to the fossils which he supposed he had found out of place, remarked that there were indeed fossils in the Hudson River group resembling the *Strophomena rugosa*, but there was great doubt whether they were that fossil. He referred to the figure, in the Silurian Researches, of the *Leptæna tenuistriata*, which was corrugated in a manner similar to the *Strophomena corrugata.* Similar differences exist in respect to other fossils. Mr. Murchison has given a figure of *Orthis canalis*, which he considers distinct from the *O. elegantula* of Dalman. Von Buch, in speaking of this shell, remarks that it is found at the Iron Bridge at ———, in England, and that it differs from those from Sweden only in being smaller. Similar observations might be made in regard to other fossils, and they could only be declared distinct or identical by comparison in hand.

In relation to the *Fucoides biloba*, he had seen a similar fossil, as well as several other species of Fucoids in the Hudson River group, which were very similar if not identical with those of the Clinton group. But he had never seen any thing resembling the Dictuolites, except in the Medina sandstone. Both these forms might have been called into existence at the period of the Hudson River group, and the conditions favoring their existence have never recurred till the subsequent periods of the Medina sandstone and the Clinton group.

Both the Hudson River group and the Clinton group are at distinct points exceedingly different in composition, being at one point almost wholly argillaceous and arenaceous, while at another they were calcareous. Where these are similar in lithological character, the fossils are very similar likewise, and perhaps sometimes identical. A similar example occurs in the formations of the Niagara shale and the Delthyris shaly limestone, where several of the points are identical. In the latter there is a shell almost precisely similar to the *Orthis canalis* of Murchison, but larger and more resembling the *O. elegantula* of Dalman.

Dr. Emmons enquired whether he had understood Mr. Hall correctly, as saying that he considered the Caradoc sandstones as equivalent to the Hudson River group.

Mr. Hall replied in the affirmative, and remarked that they also evidently included the Clinton group.

Dr. D. D. Owen then took up the reading of his paper, "on the Geology of the Western States," where it was left at yesterday's session.

Beneath the black bituminous shale with which the previous remarks concluded, are thick beds of limestone, often magnesian, forming a mass varying from one hundred to five hundred feet in thickness, and occupying a vast superficial area, particularly in the north and northwest. This is the lead-bearing rock of Iowa and Wisconsin, and has yielded more lead than any other formation in the western states. The actual produce of the mines situated in the Mineral Point district of Wisconsin and Northern Illinois, was in 1842, 32,000,000 lbs. of lead.

It was remarked that in lithological character and mineral contents, the formation of this American lead region bore a strong resemblance to the "scar limestone" and great lead-bearing limestone of the North of England, a member of the carboniferous group, and that, were it not for the sure test furnished by a comparison of the organic remains, one would be strongly tempted to pronounce the formation in Iowa and Wisconsin identical with that of the lead region in Northern England; whereas an investigation of the specific character of the fossils proves the limestone of the northwest to be the equivalent of the Wenlock formation of Murchison in England, some of the Eifel limestones in Germany, and the Helderberg rocks of New York.

Besides the rich lead veins which traverse this formation in the northwest, it contains a valuable copper ore and vast quantities of carbonate of zinc.

The most remarkable feature in the palæontology of this great western limestone, is the number of imbedded fossil corals, amongst which the chain corals hold a conspicuous place, especially in the Iowa extension of this formation.

This great limestone formation rests on thin beds of a blue and grey limestone, alternating with marls, often a complete mass of agglutinated shells. It has yielded to the palæontologist more prolific subjects for contemplation and research than any other group of western rocks, especially in the families of Trilobites, Brachiopoda and Encrinites, and has enriched our cabinets with numerous specimens of the marine inhabitants of our globe at almost the earliest period to which animal remains have been traced. Many of them are identical with those found in the lower Silurian rocks of England.

This deposit is thickest near the centre of the Ohio valley, and alternates towards the northwest. Though it occupies the surface only over a comparatively limited area, yet there is every reason to believe that it is coëxtensive with the whole mass of superincumbent rocks.

The metallic veins which are so wide in the overlying magnesian limestone of Iowa and Wisconsin, thin away on reaching the more extensible layers of this underlying shell limestone and marl.

No inferior rocks are visible in the valley of the Ohio, but near the Wisconsin River are sections which show the relation of these lowest limestones of the Ohio valley with the inferior rocks. There the last described limestones are seen resting on a siliceous sandstone, beneath which we have again a magnesian limestone, so like the upper lead-bearing magnesian limestone as not to be distinguishable from it in hand specimens; and near low water of the Mississippi, at Prairie du Chien, another sandstone is visible beneath this lower magnesian limestone of the Wisconsin River. No well defined fossils have been found in these, the sandstones and lower magnesian limestones of the northwest, so that it becomes difficult to pronounce on their equivalencies. Judging from the lithological character, absence of fossils, mineral crystallizations, geological position, it seems probable that they correspond to the formation in the lead region of Missouri. It is highly probable, too, that the lower magnesian limestone of Wisconsin is cotemporaneous with the calciferous limerock of the New York geologist, as well as with the magnesian limestone that forms the Natural Bridge in Virginia.

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Fig. 1.

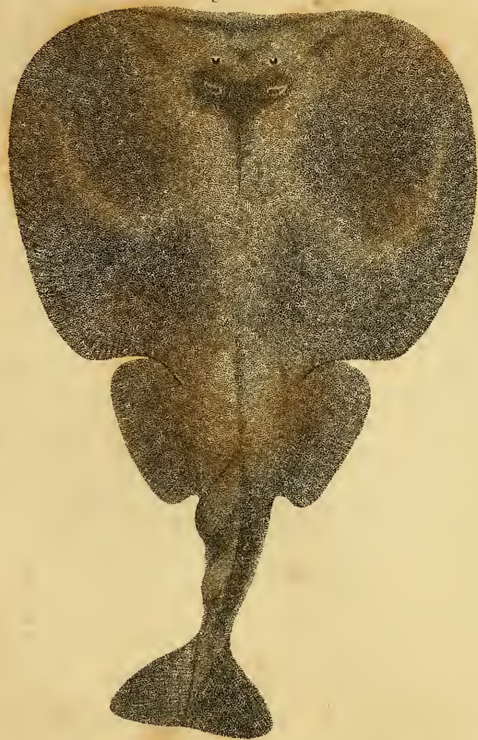
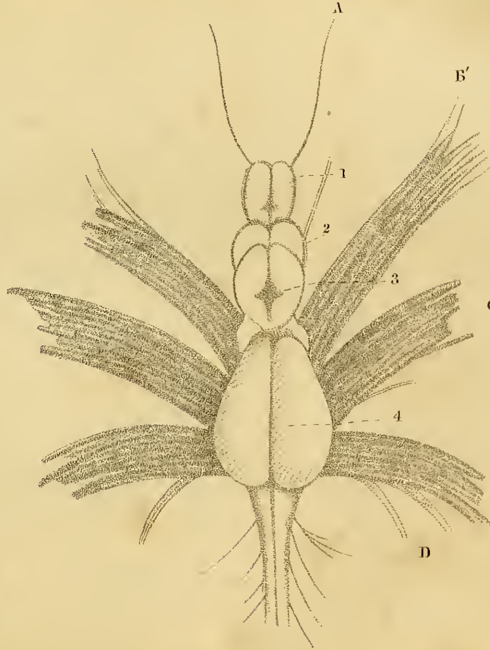


Fig. 2.



Torpedo occidentalis, *Storer.*

An important problem still remains to be solved with regard to the mineral lands of the west. We have seen that the metallic veins so productive in the thick beds of the upper magnesian limestone of Iowa and Wisconsin, dwindle away on reaching the underlying thin layers of shell limestone. Now the question for solution is: do these mineral veins, when they reach the underlying magnesian limestone, again expand and become productive?

Such are the geological formations of the beautiful valley of the Ohio, projected by nature on a scale of grandeur commensurate with the vast territory, the mighty vegetation, the majestic rivers, the gigantic forests, and the wide expanse of trackless prairie, that characterize this magnificent region of the west.

Dr. Owen concluded his remarks by a series of queries intended to draw the attention of other geologists to some points in western geology which still demand investigation.

The hour of 6 having arrived, the Association adjourned.

The Chair reminded the meeting that Mr. Emerson would favor the Association and the public with a lecture, on the importance of natural history as a branch of common education, at 7½ o'clock this evening.

[Our engagements to various correspondents forbid the continuation of these "Proceedings," and we are reluctantly compelled to postpone the remainder to our October No.—EDS. AM. JOUR.]

ART. XVIII.—*Description of a new species of Torpedo;* by D. HUMPHREYS STORER, M. D.—with a plate.

[Read before the American Academy of Arts and Sciences, April 25th, 1843.]

IN the January number of the American Journal of Science and Arts, I made a slight reference to a species of Torpedo which had been taken a few weeks previously upon the coast of Massachusetts. The description of a species captured on the coast of Ireland, published by William Thompson, Esq., Vice President of the Belfast Natural History Society, in the Annals of Natural History, answered so well to my specimen, that I was led to suppose it must be the *nobiliana*, Buonaparte. When however I carefully compared, with mine, the description and figure of the foreign species, contained in the second edition of Yarrell's British Fishes, I found no slight differences in the form of the disk of the body—in the size of the pectoral and caudal fins, and in the situation and form of the temporal orifices in the two specimens; and at once suspected the American fish must be an undescribed species. As Yarrell's figure was engraved from a dried specimen, and consequently might not perfectly represent the form of the fish, I wrote to Mr. Yarrell, stating to him my doubts of the identity of the two fishes, and presenting him with my figure. His opinion coincides perfectly with mine. I have therefore the pleasure to offer you a description of a Torpedo hitherto unknown to men of science; and as no other species of this genus is known to exist on the shores of our hemisphere, I shall call it *Torpedo occidentalis*.

Dr. Mitchill introduced the *Raia torpedo* into his "Fishes of New York," published in 1815, upon the authority of several fishermen with whom he had conversed, who had been electrified by a species of Ray, when they were detaching it from the hook with which it was taken. He had never seen a specimen, but had no doubt of its being the *common torpedo*, and consequently catalogued it as such. Since the appearance of Dr. Mitchill's paper, I cannot find any farther notice of the existence of the electrical Ray in our waters. In my Report on the Ichthyology of Massachusetts, published in 1839, I cited the testimony of several observers to prove that an electrical fish, known as the *cramp-fish*, was occasionally taken on the shore of Cape Cod, but had never been seen by a naturalist. During the month of November, 1842, a specimen of this long looked for species was captured at Wellfleet by Mr. Seth N. Covell, and I was so fortunate as to obtain it.

For the following valuable letter I am indebted to Capt. Nathaniel E. Atwood of Provincetown. This gentleman, for nearly a quarter of a century, has been a practical fisherman.

"In answer to your first question, my father came to live on the south side of this harbor, called Long Point, in 1819. Previous to that time I never saw a cramp-fish. It happened that year, and four or five years after, that cramp-fish were found uncommonly plenty. I should think at this place there were found from sixty to eighty per year. Since that time they have been very scarce, and for the last ten years previous to this, I think the whole number found would not exceed thirty; this year about a dozen have been found. They are found here in the months of September, October, and November, and at no other time of the year. The smallest I ever saw, I should think did not exceed twenty pounds weight, and was about as large as the head of a barrel; the largest I should think might weigh from one hundred and seventy to two hundred pounds; but as I have never weighed any of them, I cannot exactly tell their weight. The largest circumference is about twelve feet, or four feet diameter. You ask if I have ever received a shock from them? I can truly say that I have received a great many very powerful shocks, which have thrown me upon the ground as quick as if I had been knocked down with an axe. Although this shock is so powerful and severe, I have known individuals when taken from the water alive not to exhibit that power if they possessed it. You ask how they are captured? The largest number of their own accord run ashore upon our sandy beach. I have known two to be taken with the hook in our bay by persons fishing for other fish; and others, being discovered in the day time near the shore, are harpooned and dragged on shore.

"You also ask if I have known any one to receive a shock without having taken the fish up with the hand? I have received many shocks by taking hold of the pole of the harpoon, when I was at the distance of eight or ten feet from the fish, but the shocks are not so severe. I have also felt its effect when holding the rope attached to the harpoon, but in this and in cutting the liver from the fish when it is nearly dead, there is generally nothing more than a numbness felt in the fingers, and they seem to incline to straighten, so that I have known it difficult to grasp the handle of the knife while cutting the fish." "It does not run on shore on the north or town side of our harbor." "No part of the cramp-fish is used except the *liver;* this contains very good lamp-oil, equal to purified sperm-oil. I have never known it used for any other purpose of late; but formerly it was used for *cramp*, by bathing the parts afflicted, and it has been taken inwardly for cramp in the stomach, but of its effects when thus given I know nothing. The smallest of the fish I have seen, produced about one pint of oil, and the largest produced three gallons; the common size fish produce from one to two gallons."

The entire length of my specimen, which is a female, is four feet and two inches, and its greatest breadth is three feet: the greatest length of the pectoral fins is two feet, and their greatest breadth is fifteen inches. The first dorsal fin, which is three inches and a quarter long and five inches high, is situated at the posterior portion of the pectorals, one half of its base being posterior to those fins. The second dorsal is two inches long, and two inches and three quarters high; it is two and a half inches back of the first dorsal, and three inches anterior to the commencement of the upper lobe of the caudal fin. The ventral fins are ten inches long, and five and a half inches wide. The anus is large, and is situated beneath the middle of the ventrals. The caudal fin is nearly triangular; its lower portion is the larger: the depth of this fin at its posteror extremity when expanded is eleven inches; its posterior margin is straight. The globe of the eye, which is circular, is an inch and a quarter in diameter: the cornea is oval; its longest diameter is one half of an inch, and is directed obliquely outwards; its shortest diameter is three eighths of an inch. The spiracles are oval, and smooth at their edge; they are one and a quarter inch in their largest diameter, and one inch in their shortest diameter, and are directed outwards and a little forwards. On the anterior and inner surface of the spiracles, just within the orifice, is a plaited membrane, the folds of which resemble somewhat the nasal septa; the longest of these folds are next to the median line, and they gradually diminish in length as they recede from it. The mouth when closed, measures six inches across from the angles, and when opened to its widest extent, it measures from the middle of the upper to the middle of the lower jaw five inches. The teeth are numerous, small and sharp—broad at their bases, and pointed at their extremities like spines. When the fish is placed upon its under side, and the anterior extremity of the disk is turned backwards, the nostrils are observed about three inches beneath its edge: they are covered above by a membranous prolongation, formed by a fold of the skin which arises from their exterior angle and is continued to the median line; the free edge of this fold is five eighths of an inch wide at its greatest width. A second fold commences at their outer upper angle, and passes downwards and inwards to the middle of the lower edge of the aperture. A third fold commences near the middle of the second, and is directed outwards and a little downwards. The nasal cavity is divided by a horizontal plate into two portions, and at right angles to this proceed numerous small septa going to the upper and lower margin of the nostrils. The color of the whole upper surface of this species, is a dark brown with a few almost black dots distributed over it: the body beneath is white.

My friend Dr. Wyman dissected the electrical organs, and has furnished me with the following notes.

The electrical organs of the Torpedos have already been well described, especially by Mr. Hunter and Mr. John Davy; and in the present species there exists nothing which does not sufficiently correspond with the descriptions of these anatomists. The organs in which the electricity is developed, are situated in the space comprised between the anterior edge of the pectoral fin and the cranium, the outline of which is sufficiently obvious in the plate. They are of a kidney shape, the concave edge being directed towards the bronchiæ, and measure fifteen inches in length and eight in breadth. They consist of multitudes of triangular, quadrangular and hexagonal columns, extending from the upper to the under surface of the body, and each column is subdivided into numerous cavities or cells by transverse septa, of which Mr. Hunter counted more than one hundred to the inch, and each cell is filled with a gelatinous fluid. The most remarkable peculiarity, however, is the disposition of the nerves by which the electrical organs are supplied, and which have undergone a development of which there is probably no parallel in the class of fishes. The fifth and eighth pairs of nerves are the electrical nerves. The fifth pair of nerves, B, is distributed to the anterior part of the head, and the anterior portion of the electrical apparatus; and the eighth, C and C′, known as the vagus or branchio-gastric nerve, has its usual distribution to the organs of respiration, and the œsophagus and stomach, and in these directions its branches are of the usual size; but the additional branches which go to the batteries, as also is the case with those of the fifth, have acquired a volume many times that of the spinal marrow itself, and are to be regarded as an index of the great activity of the organs to which they belong. One other peculiarity equally remarkable remains to be noticed, viz. the ganglia from which the posterior nerves, the eighth pairs, originate. By referring to the plate the following parts will be seen: 1. cerebral hemispheres; 2. optic lobes; 3. cerebellum. These constitute the brain properly speaking, and have the same relative size as in the Raiadæ generally; but behind is a ganglionary mass (4) which exceeds the brain itself in bulk, and from which the electrical nerves, as will be seen in the plate, are derived; this has been denominated the branchio-gastric ganglion in fishes, and as well as the nerves which have been already described, will serve to indicate the immense activity of the electrical apparatus.

EXPLANATION OF PLATE III.

Fig. 1. Torpedo occidentalis.

Fig. 2. Brain. 1. Cerebral hemispheres. 2. Optic lobes. 3. Cerebellum. 4. Branchio-gastric ganglion. A. Olfactory nerve. B. Fifth pair; B′, branch to the anterior part of the head. C, C′. Branchio-gastric or electrical nerves; D, branch to œsophagus and stomach; E, spinal marrow.

Art. XIX.—*Description of some New Species of Plants;* by S. B. Buckley, A. M.

[Being unable to find room for Mr. Buckley's detailed account of a botanical tour through the mountains of Alabama, Georgia, Tennessee, and Carolina, we have, in accordance with his request, merely extracted the description of new species for present publication. We also append, in a note, the diagnostic character of a new genus of Santalaceæ, established by Dr. Torrey, upon materials chiefly furnished by Mr. Buckley, to whom it is dedicated; a full account and figure of which will hereafter be given in this Journal.*—Eds.]

Streptopus maculatus (*n. sp.*): stem and nerves of the lower surface of the leaves minutely pubescent; leaves sessile, ovate-lanceolate, acuminate; pedicels generally in pairs at the summit of the branches, not distorted; sepals subspatulate, acuminate, yellowish-white with numerous purple spots, rather longer than the filaments; anthers oblong; style longer than the stamens; stigma short.

* Buckleya, *Torr.*—Flores dioici. Perigonium calycinum, profunde quadripartitum; laciniis demum deciduis. *Masc.* Stamina 4, perigonii laciniis opposita. *Fœm.* Perigon. tubo cum ovario connato; limbo quadripartito. Discus epigynus carnosus, breviter quadrilobus. Stamina nulla. Ovarium inferum, unilocularis, uniovulatum; stylus unicus brevis; stigma quadrilobum. Drupa oblonga, compressa, putamine crustaceo, sulcato. Semen endocarpio adhærens. Embryo in axi albuminis copiosi carnosi reclusus, gracilis.—Arbuscula Tennesseensis. Folia alterna, disticha, integerrima, scabro-pubescentia. Flores terminales, parvi, virides; masculi umbellulati: fœm. solitarii.

B. distichophylla = Borya distichophylla, *Nutt. gen. N. Am. pl.* 2, *p.* 232.

Cumberland Mountains, Tennessee: flowers in April. Plant 1–2 feet high, with the habit of Streptopus lanuginosus, but differs from that species in its larger flowers and spotted sepals.

Smilax grandifolia (*n. sp.*): leaves cordate ovate, abruptly acuminate, smooth, shortly petioled, 5–7 nerved; flowers numerous, reddish brown, on peduncles 1–2 inches long, anthers yellowish white; stem terete, and upper branches often unarmed; prickles slender, 3–4 lines long, and very acute.

Hab. Alabama and Mountains of North Carolina. July.

Stem climbing, 8–10 feet long, often the whole plant nearly smooth or with a few scattered prickles. Leaves large, on petioles 2–4 lines long. Differs from S. rotundifolia in its larger leaves, longer peduncles, more numerous and differently colored flowers, and more slender prickles.

Phacelia Purshii (*n. sp.*): assurgent, pilose; upper leaves sessile, pinnatifid, lower petiolate and subpinnate; lobes lanceolate; acute segments of the corolla fimbriate.—Whole plant, especially the leaves and calyx, very hispid. Flowers blue, in a simple terminal raceme; pedicels elongated; stamens exsert; anthers oblong elliptic; style two-cleft, longer than the stamens. Differs from P. fimbriata of Michx. in its blue and more numerous flowers, erect habit, and also in being larger and much more rigid and pilose. Phacelia fimbriata, *Pursh*, *Flora*, *Vol. I*, *p.* 140.

Hab. Western and Southern States. Mr. John Carey, of New York, showed me the error into which Pursh had fallen in supposing this plant to be P. fimbriata, of Michaux.

Phacelia fimbriata (*Michx.*): procumbent, assurgent; upper leaves sessile, pinnatifid, lower petioled, subpinnatifid; lobes of the upper leaves sublanceolate, acuminate; segments of the lower leaves ovate, subobtuse; raceme solitary, short; corolla white, subrotate, lobes of the margin ciliate; flowers subdistant on elongated pedicels.

Hab. High mountains, North Carolina. *Michaux*, *Flora*, *Vol. I*, *p.* 134.

Whole plant slightly pilose or nearly smooth, 6–8 inches high. Easily distinguished from the preceding by its procumbent habit, white subrotate corolla, and fewer flowers. It is also much smoother and less rigid.

Phacelia pusilla (*n. sp.*): leaves sessile, pinnatifid; segments obovate, abruptly acuminate; racemes simple; pedicels short or elongated; divisions of the corolla round, entire; flowers pale blue or white, nearly as large as in the two preceding species; stamens exsert; sepals linear-oblong, acute, $\frac{2}{3}$ the length of the corolla.

Hab. Prairies of Alabama. Flowers in April.

Whole plant pubescent, subglaucous, branches numerous, united near the root, assurgent, capsule ovate, villous.

Phacelia brevistylis (*n. sp.*): leaves petioled, pinnatifid; segments slightly incised, lobes subcuneiform; segments of the corolla blue, round, entire; anthers subincluded; styles shorter or equalling in length the corolla; capsule orbicular, hairy, in length nearly equal to the short style; racemes terminal; pedicels elongated.

Hab. Limestone rocks, Hamburg, Wilcox County, Alabama. Flowers in April.

Stem branching, and with the petioles slightly pubescent; leaves smooth, with a few scattered hairs on the margins and both surfaces. Distinguished from P. bipinnatifida by its smaller flowers, subincluded or included style and filaments, and the larger and less incised lobes of its leaves.

Andromeda (Leucothea) montana (*n. sp.*): leaves perennial, subcoriaceous, ovate-lanceolate, entire and minutely serrate, margins ciliate; flowers in large terminal or axillary panicles; pedicels 3-bracted, the two upper bracts opposite the lower at the base; bracts subulate, stem of the panicle pubescent.—Shrub 5–6 feet high, rigid, leaves nearly two inches long and one broad, on petioles about half an inch long. The upper portions of the stem have scattered mucronate glands, appressed and pointing upward.

Hab. High mountains of Virginia and North Carolina.

Andromeda (Zenobia) recurva (*n. sp.*): leaves deciduous, ovate, acuminate, serrate, glabrous; corolla cylindrical, 5-toothed; anthers biaristate, included; calyx of 5 sepals, 2-bracted.—Shrub 3–4 feet high; stem smooth and much branched. Flowers in long, naked and somewhat recurved racemes; pedicels short. Leaves about $2\frac{1}{2}$ inches long and $1\frac{1}{2}$ broad; petioles 2–5 lines long, midrib and veins of the leaves slightly pubescent. This species has an affinity to Andromeda racemosa, but differs in its biaristate anthers, recurved racemes, larger leaves, and it is also a smaller shrub.

Hab. Mountains near Paint Rock, Tennessee, and the Warm Springs, North Carolina.—Flowers in April.

Angelica Curtisii (*n. sp.*): leaves large, bipinnately divided, segments subcordate or lanceolate; laciniæ submucronate; stem glabrous, terete, striate, involucre and involucels none.—Stem large, about 3 feet high, petioles large, long, and sheathed at the base; segments of the leaves 3–5, leaflets large and deeply laciniate, umbels crowded, fruit large, oblong, elliptical, commissure with 2 vittæ, lateral wings as broad as the seed.

Hab. High mountains of North Carolina, especially the Bald Mountain in Yancey County, where it was discovered in flower by the Rev. M. A. Curtis.

Arum polymorphum (*n. sp.*): stemless; leaves ternate, ovate, acuminate, outer leaflets rhomboid-ovate, auricled or deeply divided, approaching a pentaphyllous form; spadix clavate, longer than the subcylindric tube; fertile florets crowded around the base; spathe peduncled; tube subcylindric, broadest at the top; lamina ovate, acuminate, longer than the tube; stem 1–1½ feet high, form of leaves very variable, but generally the outer ones are more or less divided near the base; fruit, scarlet berries, few and crowded at the base of the spadix.—I have a subpentaphyllous form, collected on the banks of the French Broad, in which the spadix is more attenuate towards the apex. It is possible that this form may be the A. quinatum of Nuttall.

Carex Caroliniana (*n. sp.*): styles 3; pistillate spikes 2–3, long exsertly pedunculate; scales of the fertile florets ovate, acute, as long or longer than the perigynium; perigynium triquetrous, subacuminate, achenium ovate, elliptical, 3-angled, angles subacute; scales of the staminate spike ovate, subobtuse, reddish-brown; culm compressed, striate, subfiliform, fertile florets few and small, 3–6, generally 3, on long filiform peduncles; radical leaves numerous, 4–6 lines wide, as long or longer than the culms, culms numerous.

Grows in tufts, Table Mountain, South Carolina, April to May.

Carex miser (*n. sp.*): styles 3; staminate spike solitary; pistillate spikes 2–3, lower one shortly peduncled, erect; bracts smooth, ovate, subacuminate, with membranaceous margins; perigynium lanceolate, acuminate, 3-sided, twice the length of the bracts; achenium ovate-lanceolate, 3-angled, shorter than the styles; scales of the staminate spike oblong, subobtuse, margins membranaceous; stem slender, erect, triquetrous, radical leaves numerous, subsetaceous, shorter than the culm, which has 2 or 3 setaceous leaves near the summit longer or equalling in length the spikes; spikes small, aggregated near the summit of the culm, and nearly covered with a reddish brown color.

Hab. Summit of Roan Mountain, North Carolina.

Carex styloflexa (*n. sp.*): stigmas 3; staminate spike solitary; pistillate spikes 2–3, lower exsertly and long-peduncled; peduncles filiform; pistillate scales linear, acute, nearly as long as the fruit, with broad membranaceous margins; perigynium rhomboid-ovate, subtriquetrous, inflated, and slightly curved at the apex; achenium obovate, 3-angled, angles prominent, subacute; stem smooth, slender, 3-angled; leaves linear-lanceolate, smooth, shorter than the stem; peduncle of the staminate spike varies in length from a few lines to 5 or 6 inches; scales of the staminate spike membranaceous, lanceolate, acute.

Hab. Mountains, Macon County, North Carolina.

Diervilla sessilifolia (*n. sp.*): leaves sessile or subamplexicaule, oblong-ovate, lanceolate, acuminate, glabrous; capsule cylindric-oblong, acuminate, crowned with the subulate-setaceous teeth of the calyx, beak short.—Diervilla trifida, β. *Torr. & Gray's Flora, Vol.* 2, *p.* 11.

Hab. Mountains of North Carolina, June to July.

Stem 2–4 feet high, branched, leaves 2-4 inches long, obscurely serrate, flowers crowded near the summit or at the summit of the branches on peduncles from the axils of the leaves; peduncles 3–6 flowered; flowers sessile or pedicellate. Differs from D. trifida in its sessile leaves, shorter beak, and larger cylindrical capsule.

Hypericum graveolens (*n. sp.*): stem simple or slightly branched, terete, smooth; leaves oblong-ovate, clasping, punctate on the lower surface; flowers in terminal or axillary cymes; sepals linear-lanceolate; petals narrow, oblong-lanceolate; stamens numerous, filaments nearly the length of the styles and petals; styles 3, nearly twice as long as the carpel; stem 2–3 feet high; flowers large, numerous, in a somewhat trichotomous cyme; leaves usually about two inches long and little more than an inch broad.—Whole plant when touched emits a strong and unpleasant odor.

Hab. High mountains, North Carolina. Flowers, July to Aug.

Scutellaria arguta (*n. sp.*): leaves cordate, ovate, dentate, long petioled; stem and petioles pubescent; calyx short, teeth of the calyx obtuse; flowers small, few, in axillary or terminal racemes; leaves smooth, with a few minute hairs on both surfaces and the margins; stem subprocumbent, 8–12 inches long; leaves 1½–2 inches long, 1–1½ inches wide, petioles 2–2½ inches long, teeth of the leaves large, subobtuse, upper surface of the leaves deep green, under surface pale green.

Hab. Black Mountain, North Carolina, near the head of the Swaninoa River; generally grows on large rocks. Flowers, July to August.

Vaccinium hirsutum (*n. sp.*): leaves deciduous, ovate, entire, slightly mucronate, nearly sessile; corolla oblong, and nearly closed at the apex, with five short teeth; anthers awnless, included; filaments and style hairy; berry globose, many-seeded; whole plant, including the flowers and fruit, thickly coated with small hairs.—Plant about a foot high, much branched; flowers in small terminal or axillary racemes; pedicels one or two-bracted. The hairy flowers and fruit of this species will easily serve to distinguish it.

Hab. Mountains, Cherokee County, North Carolina.

Zizia pinnatifida (*n. sp.*): leaves tripinnately divided; segments ovate-lanceolate, cuspidate; stem smooth, striate, branching towards the summit, with one or two long petioled leaves near the base; petioles of the lower leaves about 12 inches long, and those of the upper an inch, or nearly sessile; umbels few, axillary and terminal, 10–12 rayed; involucels naked or with one or two small leaflets; fruit elliptical with prominent ribs, dark brown when mature; flowers yellow.

Hab. Banks of the French Broad River near the Warm Springs, and near Sugar Town Falls, Macon County, North Carolina.

Thalictrum debile (*n. sp.*): stem low, procumbent or assurgent, much branched, glabrous, diœcious or polygamous; flowers few, on axillary or terminal peduncles; leaves on long petioles, ternately or biternately decompound; leaflets small, petioled, broad or rounded, crenately and obtusely lobed; carpels oblong, strongly ribbed and slightly stipitate, about the length of the slender style; filaments filiform, anthers linear, elongated, acute.—Plant 6–8 inches high, sending out numerous branches near the root. Petioles 1–2 inches long; those of the leaflets 3 lines to an inch in length. Differs from T. dioicum in being procumbent, much smaller, and in having petioles to the leaflets, and fewer flowers.

Hab. Rich woodlands near Allenton, Wilcox Co., Alabama, March and April. The stem and leaves decay and disappear about the first of May.

Iris Duerinckii (*n. sp.*): bearded, leaves subfalcate, ensiform; scape 1–4 flowered; petals obovate-spathulate, deflexed; filaments inserted into the tube of the corolla; anthers linear oblong.—Stem 6–12 inches high, generally longer than the leaves. Filaments exsert from the tube nearly one third the length of the corolla; tube of the perigonium elongated, slender, exsert. A variety is stemless, one-flowered, with the leaves much longer than the flower. Described from specimens received from Prof. Duerinck, who collected them near St. Louis, Missouri. It is probable that this species is the Iris Missouriensis, of Martins, which name belongs to a species previously described by Nuttall. See Martins' Delectus seminum Horti Botanici Louvaniensis, 1840.

Justicia lætevirens (*n. sp.*): leaves lanceolate, ovate, acuminate; flowers axillary and terminal, white, in cylindrical compact spikes.—Stem erect, simple or branched, nearly 12 inches high, slightly glaucous. Leaves large, 2–3 inches long, 1–2 inches wide, glabrous on the lower surface, slightly hairy above, gradually tapering into short petioles. Flowers numerous in a compact bracted spike; bracts ovate ciliate; tube of the perigonium exsert, caducous; corolla 3–4 toothed; filaments 2, slender, inserted and included within the tube of the corolla; capsule at the base surrounded with numerous filiform bracts (calyx?)

Hab. Near rivers in shady woods, Wilcox Co., Alabama; flowers during the summer.

Malva LeContii (*n. sp.*): leaves subsagittate, entire, obtuse, dentate; teeth large, obtuse; lower surface of the leaves very pubescent; midrib and veins prominent; upper surface scabrous; sepals ovate, acute; involucre 5–6 leaved, as long as the calyx; carpels wrinkled.—Stem shrubby, 4–5 feet high, pubescent, much branched, flowers large, pale red; leaves numerous, rather small, on petioles 6 lines to an inch long; under surface greenish white, and covered with a dense, soft, woolly pubescence. A well defined species; described from specimens received from Maj. LeConte, who collected them in the southern part of Georgia.

Pteris Alabamensis (*n. sp.*): frond pinnate; leaflets alternate, linear-lanceolate; pinnulæ alternate, oblong-lanceolate, terminating abruptly at the base, sessile, generally auricled on the upper basal margin; stipe and rachis smooth, black.—Frond 4–6 inches long, 2–3 broad, with an oblong-lanceolate outline; easily distinguished from other species growing in the United States, by its auricled pinnulæ.

Grows in tufts on limestone rocks, that form the banks of the Tennessee River, at the foot of the Muscle Shoals, Alabama.

Phlox glutinosa (*n. sp.*) viscid-pubescent; leaves oblong-lanceolate, mucronate; divisions of the calyx long, setaceous; tube of the corolla twice the length of the calyx; flowers bright red or scarlet.—Stem simple, erect, about 12 inches high; whole plant covered with a glutinous pubescence. Differs from P. aristata in its simple, erect stem; bright red or scarlet flowers, and its leaves are also broader as well as mucronate. Phlox aristata has many assurgent stems from the same root; this species rarely if ever more than one.

Hab. Pine woods, Black's Bend, Wilcox Co., Alabama: May.

Art. XX.—*Ornithichnites of the Connecticut River Sandstones and the Dinornis of New Zealand.*

It is with great pleasure, not unmingled perhaps with some pride, that we present to our readers the following correspondence between Dr. James Deane, the original observer of the Ornithichnites, (so well and boldly described by Prof. Hitchcock,) and Dr. Mantell of England, to whom we had the pleasure last summer of transmitting a very full and beautiful series of these tracks collected by Dr. Deane, and accompanied by the letter which follows.

The greatest scepticism has existed in England in relation to the truth of Prof. Hitchcock's and Dr. Deane's inferences from these singular impressions, on the part of those enjoying the greatest and best deserved reputation in palæontology and comparative anatomy. This is not surprising when we reflect on the important bearing which the facts if admitted would have on our preconceived notions of the fauna of this comparatively ancient geological era. But we believe the evidence on which these inferences rest has never been fully presented to any mind competent to judge of the facts, without resulting in the most thorough conviction of their correctness.

The letter from Dr. Owen will be read with peculiar interest, as containing in a most desirable form, the first information which it has been in our power to present of that most interesting discovery—the existence of the immense *Dinornis Novæ Zelandiæ*, so valuable in bearing out and confirming the views of Prof. Hitchcock in relation to the authors of the fossil impressions in the Connecticut sandstones.

Letter of Dr. Deane to Dr. Mantell.

Dear Sir,—With this letter you will receive through Prof. Silliman of Yale College, a box of fossil footmarks derived from the New Red Sandstone of the Connecticut, a considerable river intersecting this State. These beautiful fossils, indicating a high grade of animal existence in a period of the earth so immensely remote, may well be regarded among the wonders of paleontological science. Prior to the year 1834 the traces of extinct birds so low down in the geological series, were altogether unknown, and even now that the accumulated evidence of the fact is so overwhelming, the assumption that they are such, is received with grave circumspection. That the footsteps of Connecticut River are, however, the authentic traces of extinct *birds*, is confirmed by the undeviating comparisons they bear to living nature. In the year just mentioned my attention was attracted to these splendid relics, so boldly displaying the essential characters of foot-prints of living birds, that I could not hesitate concerning their origin, although no effort of the mind could comprehend the period of their antiquity. The impressions were perfectly defined, succeeding each other in the determinate order of living birds, and being aware that footsteps of animals upon rocks were unknown, or at least controverted occurrences, I communicated the discovery to Prof. Silliman of Yale College, and to Prof. Hitchcock of Amherst College, then geologist to the state of Massachusetts. Both gentlemen admitted the plausibility of my statements, yet remained incredulous as to inferences, ascribing the origin of these remains to accidental causes, and it was only after accurate models were transmitted to them, that the real truth was obvious. Prof. H. then gave the specimens an inspection which resulted in the unqualified conviction that these foot-prints were genuine vestiges of birds. He subsequently explored the entire valley of the Connecticut River with extraordinary success, the details of which he has given to the scientific world in several treatises of great ability.

During the past year I have received letters from Prof. Silliman, enquiring if I could furnish such examples of these fossils, as might appear to establish the fact that they were unquestionable footsteps of *birds*, but it is only at this time in my power to render a satisfactory reply, and that reply is most emphatically expressed by the beautiful specimens I now have the pleasure of sending you. You cannot fail to observe, notwithstanding the enormous contrast in the size of these footmarks, the striking resemblance they bear to each other, and for distinctness and beauty of impression, and fidelity to living nature, the *O. tuberosus* is most remarkable of all, indeed its perfection supplies a model for the comparative anatomist. Although the distinct varieties of these imprints hitherto discovered, established several intrinsic characters common to all, such as succession of feet, numbers and arrangements of toes, form and insertion of claws, &c., it was not until the discovery of this variety that an example occurred so faultless as to illustrate the construction of the *joints* with precise accuracy, although Prof. Hitchcock had long suspected the truth that its perfection reveals. In exploring the bed of the river at low water in 1841, I was gratified with the discovery of several new species of these imprints, exquisitely perfect. If they had been made in wax and turned into solid stone, they could not have been more so. The material upon which the animal trod possessed a degree of tenacity not only to retain distinctly the lobate form of the respective joints, but even in some rare instances, the corrugations of the integuments. In them every feature of the impression is without blemish, the ridges marking the boundaries of the joints and also of the toes being clear, sharp and correctly defined, and the impress vividly distinct. These features are all displayed by examples now before you. The surface of the rock is compact and smooth, and I wish to call your attention to the fact that the part depressed by the weight of the bird is so condensed as to appear to be enamelled, and is extremely hard. Each tuberous joint presents the appearance of being swayed by a distinct blow rather than being moulded by a fleshy joint. In some examples there is no concavity in the depression of the individual joints, on the contrary the surface is flattened and may properly be illustrated by the impress of a hammer upon a mass of lead. [Nos. VI, XV to XVIII, inclusive.] The beauty and perfection of the impressions seem due to the condition of the plastic material at the time of receiving them, and the finest examples are those in which the impressions are most superficial, being evidently made when the surface of the stratum had been subjected to a partial process of desiccation, just at that point sufficiently hard to take as well as receive the minutest lines. This peculiarity is very apparent in your collection; in some of the specimens the surface is thoroughly enamelled by this drying process, and it is an indispensable condition to the complete preservation of these remarkable fossils. The contour of the foot in this species is surpassingly elegant.* In the nomenclature of Prof. Hitchcock it is denominated *Ornithichnites tuberosus*, but it is proper to remark that it is not identical with that so named in his final report to the legislature of Massachusetts. The tuberous expansion of the joints being truly developed in this variety, it has received the appellation to which it is eminently entitled.

This superb variety may be taken as the type of these impressions, and in its essential elements it is faithfully represented by the foot-prints of most species of extinct birds. The identity is displayed in the order of the joints, as every example in the collection will show, the inner toe having two, the middle three, and the outer four lobed swellings. This distinguishing feature, as far as it goes, is conclusive evidence of this identity, but the analogy extends to the *claws*, which are so distinctly stamped that their peculiar form and insertion can be closely traced. In most of the specimens this appendage is beautifully illustrated and so delicately executed as to suggest the idea that its plantar surface was membranous. The analogy is still further main-

* Most of our readers have seen the accurate and expressive representatives of these forms in the drawings published by Prof. Hitchcock in this Journal, and his final State Report, and also those copied from them by Dr. Buckland in his Bridgewater Treatise; otherwish we should illustrate the present paper by several drawings.—EDS.

tained by the divergence of the toes, the progression and alternation of the feet, and the existence in each variety of steps of an identity or individuality as unequivocal as is ever displayed in animated nature. The examples xv, xvi, xvii, xviii, are in point. If you will place them in the order of numbering two and a half feet asunder, which was the length of the stride, so that the connecting line shall fall a little inside of the centre of each impression, they will occupy their original positions, and it will be observed that the long toe is alternately upon either side of this line. Indeed we may sometimes follow the route of the bird several rods, step after step, and the sight is sufficient to fill the beholder with astonishment; he is irresistibly carried backward through the long series of years, until lost in the unsatisfactory computation of the era in which these impressions were made.

Their existence upon the face of the rock, without any exception, is in strict conformity to the laws of nature. The footsteps, invariably those of a biped, occur upon the *upper* surface of the stratum, while the cast or counter-impression is upon the lower. Not a single deviation has ever occurred. The reverse impression is beautifully illustrated by the specimen No: xii. It is moulded by a subsequent deposition in the matrix, formed by the foot of the bird, and it is an indispensable condition, which universally exists. The foot is usually tridactylous, although there is the same deviation from this condition, as is found in living birds. There are usually three, sometimes four, but rarely five toes. The length of step is in correspondence with the dimensions of the foot. When it is one inch in length, the intervening distance of the step is from three to five inches, but when the foot is fourteen inches and upward, the stride is from four to six feet! When I talk of a foot fourteen inches in length, it may appear incredible that the earth ever produced such a colossal bird, but if you refer particularly to No. xix, you will find it of full measure, and that without a projecting heel! I am particular to state, that its enormous magnitude consists entirely of the three toes, the heel not reaching to the ground. It is altogether a remarkable impression by reason of its size, and perfect delineation. With the exception of the lateral claws, it is accurate as any specimen I have ever seen, and as it is a shallow impression, it is best seen by placing it at a distance of ten or twelve feet, in the reflection of a strong light. In all these remains the distinctive marks of organization are traced with great fidelity. There are no conflicting phenomena to disturb the inevitable conclusions of the judgment, but a mutual relation is ever perceptible, that carries the internal evidence that these marvellous impressions are the genuine vestiges of birds, and that no quadruped animal with which we are acquainted, living or extinct, would have made them.

The feet of birds being so prominent an organ, to separate them into generic divisions, we cannot omit to inquire if the extinct genera have their representatives in the living. The inference is that by leaving their traces on the muddy bottoms and margins of the ancient waters, the extinct birds were waders, and to some of the species the classification of living birds supplies a similitude. The *O. tuberosus* is very accurately represented so far as form is concerned, by the pinnated feet of the *Fulica custata* and other genera of the order Grallæ, the toes being bordered by membranes that give the same form and expression to the foot, and completes the presumption that these remarkable imprints are authentic traces of birds.

Assuming them to be such, the enquiry naturally suggests itself why we do not discover the co-existence of these fossil bones; but there are plausible explanations why the skeletons of birds should not be found in strata deposited by the agency of water. It is not the element in which birds live, and as nature in her appointments has adapted all animals to the media in which they exist, we may see in this law a philosophical reason why their bones do not exist. The osseous system of birds consists merely of thin cylindrical shells of great strength and buoyancy, and it therefore results that should death accidentally happen upon water, the carcass would not be quietly deposited at the bottom, but be drifted about by currents until destroyed by decomposition or violence. This seems to be a legitimate conclusion, although it by no means follows that such bodies would not accidentally become entangled at the bottom, or be suddenly enveloped by a stratum of that plastic material subsequently to be converted into solid rock. It is not therefore impossible but the skeleton may be discovered, a single bone of which will be sufficient to determine the order of animals to which it belongs. And if comparative science be so invariable in its application, that from a fragment a complete fabric may be restored, does it not follow with great force, that from exact impressions of each bone of the foot of an animal, with all its appendages, we may thereby reconstruct its corporeal frame? Comparative anatomical science owes its absolute certainty to the uniform operation of nature's laws; they are constant, and this consistency is the key that deciphers the mysterious dialect engraven upon the enduring rocks. We learn from this imperishable record that the footmarks of Connecticut River are none other than real vestiges of birds striding over the earth, in a period of its existence so remote, that the imagination is overwhelmed with the conjecture of its duration. It is impossible to stand upon the ancient rock and see the faithful characters engraven there, without yielding to the irresistible truths they reveal. It is there that, rejecting hypothesis, we bow to the supremacy of truth, because we feel its all-constraining power.

Not the least interesting of the fossil remains of this ancient rock are the impressions produced by the fall of rain-drops, some fine examples of which I add. They exist under the actual conditions regulating footsteps, and not unfrequently both are found upon the same surface. The track No. 1, is an instance, although not very striking. My attention was forcibly arrested when gathering these minerals, by observing an analogous appearance upon the mud of the river's bank, within a few feet of the rock whence the specimens were taken. They were similar, with the exception that the recent impressions only required the action that operated upon the ancient to convert them into beautiful fossils. Tracks of living birds are extremely numerous upon the alluvial mud of Connecticut River, and when indurated by the action of the summer's sun, their entire removal is frequently easy.

I have thus thought proper, dear Sir, to accompany these memorials of extinct existence, with such remarks as may perhaps serve to explain the manner in which they exist. But after all, I leave them chiefly to tell their own story, for they reveal in silent though eloquent language, the events that occurred in a period of time so far back in the infancy of our earth. I deem that they could scarcely fall into better hands, for I am not unacquainted with the surprising discoveries in fossil geology, developed by your agency. I have never studied them without gratification, for the bright light thus thrown upon the mysteries of creation.

I am, dear sir, most respectfully, your obedient servant,

James Deane.

Greenfield, Mass., Sept. 20, 1842.

Reply of Dr. G. A. Mantell to Dr. Deane.

Crescent Lodge, Clapham Common, England, Feb. 13, 1843.

My dear Sir—I have deferred replying to your highly interesting communication, and acknowledging your kindness, until an opportunity occurred of submitting the specimens of *ornithoidichnites* to the examination of the Geological Society of London. At the last meeting I placed the specimens before the Society, and read the letter with which you had favored me, and afterwards gave a *viva voce* description of the fossils, illustrating my remarks by drawings showing the position and relative distances of the foot-prints when *in situ*. A brief notice of foot-prints by Mr. Redfield, was read on the same evening, and Mr. Lyell, who communicated it, gave a graphic account of the appearance of the impressions of feet seen by him in various localities of the United States, in company with Prof. Hitchcock. Mr. Owen (of the College of Surgeons) was not present, but the President, Mr. Murchison, read a short note from that gentleman, expressing his doubts as to foot-prints alone being sufficient evidence to prove whether the animals which made them were birds or reptiles. Mr. Murchison was also sceptical as to these markings having undoubted claims to be considered as true foot-prints of birds; but Mr. Lyell stated his conviction that they were genuine *ornithichnites*. The enormous magnitude of the largest imprints, served to present the greatest objection to some of the Fellows; but this difficulty is removed by the recent discovery in the modern alluvial strata of New Zealand, of some bones of a *struthioid* bird with trifid feet, equal in size to the most colossal of the fossil foot-prints hitherto observed in your country. This New Zealand bird is stated by Mr. Owen, (who has described the bones hitherto received in England in the Zoological Transactions,) to belong to a new genus allied to the ostrich and emu. There is a tradition among the natives that some individuals of this giant of the feathered tribes existed not more than one hundred or one hundred and fifty years ago. It seems therefore to have been annihilated by human agency, like the dodo. The Apteryx of New Zealand will in all probability share the same fate ere another century or two shall have passed away.

At the anniversary meeting of the Geological Society, Mr. Murchison, the President, alluded to the subject of *Ornithichnoidites*, and after paying a just tribute of respect to you as the first observer, and to Prof. Hitchcock as the successful investigator of this important branch of palæontology, confessed that the gigantic bones from New Zealand, evincing as they did most unequivocally the existence, even in our own times, of birds as large as any required by the American footmarks, had removed his scepticism, and that he had no hesitation in declaring his belief that the Ornithichnites have been produced by the imprints of the feet of birds which had walked over the rock when in a soft and impressible state; an opinion in which I entirely concur. Sooner or later the skeletons of these unknown birds will be discovered in the strata.

It cannot fail, sir, to be gratifying to you to know, that your brief but lucid description, illustrated by the highly interesting suite of specimens, has placed this important subject before the geologists of England in a most clear and satisfactory point of view, and that the thanks of the Society were warmly and unanimously expressed for so valuable a communication. With great respect, I am, dear sir, your obliged and faithful servant,

GIDEON ALGERNON MANTELL.

Letter of Prof. Owen to Prof. Silliman on the Ornithichnites and Dinornis.

Royal College of Surgeons, London, March 16th, 1843.

My dear Sir—I beg to acknowledge the favor of your esteemed letter of the 27th of February, and am unwilling to delay my answer, although I am not able to answer all the points to which it relates. I have not yet, for example, seen the entire collection of foot-prints in the possession of our common friend, Dr. Mantell, but on the few which he has obligingly submitted to me, (two very clear ones last Saturday night at the soirée of the President of the Royal Society,) I may venture, after much mature consideration, to speak. You may be aware that M. De Blainville contends that the ground—viz. a single bone or articular facet of a bone—on which Cuvier deemed it possible to reconstruct the entire animal, is inadequate to that end. In this opinion I do not coincide. I have had too frequent evidence of the potency of the law of correlation of structures in an animal organism to doubt the strength of Cuvier's proposition. But if a single bone has been deemed insufficient to give the entire animal, with more reason may we doubt the efficacy of a foot-print.

We must bear in mind the conflicting opinions to which the Chirotherium impressions have given rise: next, in regard to the Ornithichnites, it is important to remember that there were reptiles at the age of the New Red Sandstone, the *Rhynchosaurus*, e. g. (see Trans. of the Cambridge Phil. Soc., Vol. VII, Part III, p. 355,) which presented a singularly close approximation to birds in the form and structure of their edentulous skull; and might not a corresponding modification of the feet complete the resemblance of these ancient reptiles to the fabled cockatrice? A biped reptile would not be more anomalous than a jerboa or kangaroo.

In the foregoing remarks, I wish to be understood as merely indicating the grounds which justify caution in assuming the existence of a highly organized, warm-blooded, quick-breathing, perhaps volant, feathered biped, from foot-prints merely. I have, however, recently acquired very important additional evidence of the former existence in the north island of New Zealand, of a gigantic bird, having the same low grade of organization, as regards the respiratory system, which I have demonstrated in the *Apteryx* of the same island. (Zool. Trans. Vol. II.) It is to this circumstance, perhaps, that Dr. Daubeny alludes in his letter to you. My evidence is not however foot-prints, but the bones themselves. If you will refer to the Transactions of the Zoological Society, Vol. III, Part I, p. 29, you will see the first indication of the gigantic Struthious bird of New Zealand, which indicates Cuvier's principle, as showing what may be made out of a single fragment of bone. Three years after that fragment was interpreted, a box containing femora, tibiæ, a metatarsal bone, and portions of pelvis, vertebra, &c. was transmitted to Dr. Buckland from New Zealand, who generously placed them at my disposal. They were described at the meeting of the Zoological Society, January 24, 1843, and established the fact that at no very remote period, say a couple of centuries ago, there existed in New Zealand a tridactyle Struthious bird, one third larger than the African ostrich, resembling the apteryx in the proportions of the tibia to the metatarsus, and in the absence of air in the former, and therefore, most probably in the rudimental state of the wings. Now the metatarsal bone of this bird, which I have called *Dinornis Novæ Zelandiæ*, is fully large enough to have sustained three toes equivalent to produce impressions of the size of those of the *Ornithichnites giganteus* of Prof. Hitchcock. This I had the pleasure to demonstrate to Mr. Boott of Boston, during his late visit to London. It seems most reasonable, therefore, to conclude that the *Ornithichnites* are the impressions of the feet of birds, which had the same low grade of organization as the *Apteryx* and the *Dinornis* of New Zealand, and these latter may be regarded as the last remnants of an apterous race of birds, which seems to have flourished at the epoch of the New Red Sandstones of Connecticut and Massachusetts.

Believe me, very faithfully yours,

RICHARD OWEN.

In concluding this subject, we are sure our readers will agree with us that we cannot do better than to quote the following paragraph from the last address of Mr. Murchison before the Geological Society of London, Feb. 17, 1843. Mr. M. says:

"To American geologists we are indebted for our acquaintance with this new class of phenomena. The existence of the fossil bones of birds of ordinary size had, it is true, been ascertained by Dr. Mantell in the Wealden strata, but great was our astonishment, and I may add our incredulity, when Prof. Hitchcock first announced that in rocks of considerable antiquity, (the exact age of which is still uncertain,) there existed innumerable impressions in successive layers, which must have been formed by birds, some of them of gigantic size, and to which he boldly assigned the name of 'Ornithichnites.' Various opinions were entertained, and much scepticism prevailed concerning these impressions; but it is due to Dr. Buckland to state, that he never doubted that the views of Prof. Hitchcock were founded on true natural analogies, and he accordingly published this opinion with illustrative plates in his Bridgewater Treatise. The recent visit of Mr. Lyell to North America, and a memoir he has read, as well as a communication from Dr. James Deane of Massachusetts,* have necessarily brought this highly interesting subject again before us: whilst a very remarkable discovery in natural history, has at all events almost entirely dispelled scepticism regarding the true *bird-like* character of even the largest of the footsteps, however difficult it may be to imagine the presence of such highly organized creatures at a very early period. The observations of Mr. Lyell completely support the views of Prof.

* The same which accompanies this article.—EDS.

Hitchcock as to the littoral nature of the footstep deposit in Connecticut, and that the prints in question were left by birds on the mud and sand of former estuaries, the bottoms of which were gradually submerged, and by the increase of fresh matter were permanently preserved." * * * *

We are forced to omit an interesting page on the Dinornis of Prof. Owen, for want of room. Mr. M. continues: "Now to apply this discovery to our Ornithichnites, one of the greatest difficulties which many of us had to overcome, was the gigantic size of the largest American footsteps, which measure fifteen inches in length ; and it is a most curious fact that upon placing the fossil cast alongside of the metatarsal bone and tibia of the largest individual of Dinornis, Prof. Owen is of opinion, that if the feet of this great tridactyle bird be found, they will, from the usual proportions maintained in such animals, be fully as large as those of the American Ornithichnite. From this moment, then, I am prepared to admit the value of the reasoning of Dr. Hitchcock, and of the original discoverer, Dr. James Deane, who, it appears by the clear and modest paper lately brought before us by Dr. Mantell, was the first person who called the Professor's attention to the phenomenon, expressing then his own belief, from what he saw in existing nature, that the footmarks were made by birds. Let us now hope, therefore, that the last vestiges of doubt may be removed by the discovery of the bones of some fossil Dinornis ; and in the mean time let us honor the great moral courage exhibited by Prof. Hitchcock, in throwing down his opinions before an incredulous public."

Art. XXI.—*On the Great Comet of* 1843; by Mr. S. C. Walker and Prof. E. O. Kendall, of Philadelphia.*

High School Observatory, Philadelphia, May, 1843.

To the Secretaries of the American Philosophical Society, &c.

Gentlemen—We avail ourselves of the centennial meeting of the members of this Society to lay before them generally, the reasons which induce us to believe that the recent visitor is a comet

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* The first letter was communicated to the American Philosophical Society, at their centennial anniversary, May, 1843; and published in the United States Gazette, May 29, 1843: since revised, and furnished by the authors, for this Journal, at the request of the Editors. The second letter is now for the first time published.

of short period of only $21\frac{7}{8}$ years; and that it is identical with those of February, 1668, and of December, 1689. An early suggestion of its identity with that of 1668, was made, we believe, by Prof. Peirce, in a lecture delivered at Boston, on the 23d of March last. Shortly before that date, viz. March 20, it appears to have been noticed by Mr. Cooper of Nice, in a letter to Schumacher, published in the London Times. The question of their identity has been discussed by Prof. Schumacher and Mr. Petersen of Altona. The latter applies Galle's elements to the perihelion passage in 1668, and Prof. Schumacher expresses an opinion in favor of their identity. The subject has been more fully discussed by Mr. Henderson, the Astronomer Royal of Scotland; who, in a letter to Schumacher of April 11th, states that "there appears great probability in favor of the supposition that the late comet, and the one which appeared in 1668 are the same." Mr. Henderson then gives the elements of the comet of 1668, and a comparison of the ephemeris computed from them with the places of the nucleus of the comet as found by Mr. H. on a map in his possession containing a trace of its path among the stars, from March 9th to March 21st, 1668, as seen at Goa. The agreement is quite sufficient to warrant a conclusion of their identity. The first suggestion of the identity of the comets of 1689 and 1843, was made by ourselves in a letter to the editor of the Philadelphia Gazette, April 6th, in which after giving our own elements of this comet, and Pingré's elements of that of 1689, we mentioned "these elements agree quite well with Prof. Peirce's and ours, except the inclination. The observations used by Pingré are pronounced to be good by Olbers, and he expresses confidence in the elements of Pingré. Still the imperfections of instruments and catalogues of stars in 1689, may have caused such imperfections of the observations as to lead Pingré to an inclination of 69°, instead of 39° or 36° as found at present. When we consider that the inclination found by Prof. Peirce and ourselves is derived from an orbital motion of less than 2°, it is manifest that the position of the plane of the orbit, or in other words the inclination, must be quite uncertain. The same difficulty must have occurred in 1689, under still more unfavorable circumstances. It is quite likely therefore that a modification of the elements of this comet not greater than those of Halley's comet in its successive periods, would represent the observations used by Pingré, as well as his own elements, or at least within such limits as those to which the errors were liable."

In a communication in the Inquirer of the 11th April we still repeated our suggestion of the sameness of these comets. Finally in the Boston Courier of April 25th, Prof. Peirce published his elements of the comet of 1689 and found an inclination smaller even than that of 1843, with other elements agreeing very well with those of the recent comet. This removed all doubt in our minds of the identity of these comets, and on the arrival of the London Times of April 14th, containing Schumacher's opinion confirmatory of Prof. Peirce's of the sameness of the comets of 1668 and 1843, we compared the periods to see if the comet of 1843 could not be both that of 1668 and 1689, and we found that a period of 21$\frac{7}{8}$ years would answer for all three. We announced this conclusion in a letter, dated May 8th, in the United States Gazette of May 11th, with an attempt to account for its not being seen except about the eighth period of its revolutions, when it returns to the perihelion at the same season of the year. We also stated that our parabolic elements, which gave an orbit passing through our first and last normal places of March 20th and April 9th, gave the place on the middle date of March 30th too much advanced. We also stated that such was the case of all the good parabolas obtained for its orbit in Europe or America, and mentioned our coincidence in opinion with Encke, that the parabola was not the true orbit, and added that probably it would be found to be an ellipse of 21$\frac{7}{8}$ years. We also stated that an attempt further to correct the parabola for the middle observation, would lead to a paradox such as Encke had encountered in his attempt to complete an orbit on the presumption that the curve is a parabola. We immediately, with the kind assistance of Mr. John Downes, commenced the computation of an orbit on Gauss's general method, without presuming upon any conic section; but hoping to find an ellipse, and found a double paradox, a comet moving in an hyperbola, and that hyperbola having its perihelion point within the body of the sun. We immediately announced this result in the United States Gazette of the 19th April, and invited an expression of opinion from astronomers, as to the legitimate interpretation of this result. It was manifest, that if the centre of gravity of the comet and tail was moving away in a non-periodical curve, our favorite opinion of the identity of these three comets, and short period of $21\frac{7}{8}$ years, would be untenable. Although we considered the hyperbolic orbit as well as the small perihelion distance to be both paradoxical, we were willing to submit them as genuine deductions from our observations and computations, and leave them to be received as paradoxes, or explained away as the sequel should show. In so doing, we postponed for the time urging our favorite theory of the short period of $21\frac{7}{8}$ years. It is true that we had suggested the probable cause of the acceleration of the comet's place for the middle observation as computed from a parabolic ephemeris, to be owing to the shape of the comet, in the United States Gazette of the 6th of April, after pointing out the acceleration of the comet's place for the middle observations, viz.* "The slight difference between the two curves (our parabola and the true path of the comet) is lost amidst the errors of observation, and the uncertainty whether the central portion or the densest part of the nebulosity corresponds with the actual centre of gravity." We were aware that Encke had resorted to this hypothesis, to explain the paradox of the acceleration of his comet, previous to his more fortunate suggestion of the resisting medium. In regard to the recent comet, our attention was early called to this source of error by our esteemed correspondent Mr. E. C. Herrick, of New Haven, who, in a letter addressed to S. C. Walker, on the 29th of March, remarks, "The concentration of light in the nucleus (as seen in the 10 feet Clark Telescope of 5 inches aperture) seemed to me on two occasions to be considerably nearer the anterior than the posterior part. Once we thought we could detect three dim starlike points, but it was almost impossible to decide with certainty. *Where the tail is so immense, is there not some hazard in assuming the centre of the nucleus to be the centre of gravity of the whole body?*" We are particular about the dates of these suggestions respecting the centre of gravity of the comet and tail, in as much as it is found to be a matter of much importance in the sequel. Having fairly on the 19th and 20th laid our two paradoxes, viz. the hyperbolic orbit, and the perihelion distance less than the sun's semidiameter, before the public, with some suggestions as to the inferences that would follow from a strict interpretation of this result of calculation and observations, viz. that of the necessity

* See Mr. R. W. Haskins's paper on the "Resisting Medium" in this Journal, Vol. XXXIII, p. 19.

of a rebound, or of the comet's flowing round the sun, we waited for the opinions of our friends, and for further information from the European observatories. We have since received both, and hasten to lay them before you. First, the arrival of the Caledonia brought out the announcements from most of the European observatories in Prof. Schumacher's excellent Astronomical Notices of April 22. From these it appears that the comet's nucleus was first seen in Europe, at Nice on the 14th, and first observed at Rome on the 17th of March. This was five days later than it was observed at several places in the United States, viz. on the 9th and 11th, not to mention Mr. Clarke's measures of the distance of the nucleus from the sun on the 28th of February. The latest observation quoted by Schumacher, is that of Encke at Berlin, March 31st. Perhaps it was seen later. We followed it at the High School Observatory till the 10th of April. The conclusion of Encke, Steinheil, Nicolai, Schumacher, Argelander and others, that the parabola is not the true conic section for this comet, confirmed the announcement we had made on the 11th April. Encke who alone of all the astronomers yet heard from, had discussed the question of the particular conic section, had found an hyperbola resembling ours, with the perihelion point just falling outside of the sun. Thus one of our paradoxes, that of the hyperbolic orbit of the observed centre of the nebulosity, was confirmed by the only astronomer in Europe, who as far as heard from had gone over the same ground with ourselves.

For the other paradox, viz. a perihelion point within the body of the sun, we find the most ample confirmation. This element is thus stated by the European astronomers:

Plantamour, Geneva, - - -	0.0045
Arago, Paris, - - - -	0.0054
Galle, Berlin, - - - -	0.0118
Argelander, Bonn, - - -	0.0072
Nicolai, Manheim, - - -	0.0037
Encke, Berlin, - - - -	0.0047 or less.
Do. do. - - - -	0.0036
Do. do. - - - -	0.0052 hyperbola.
Mean, - - - - - -	0.0057
Do. omitting Galle, - - -	0.0049
Our last result, - - - -	0.0041 hyperbola.
Sun's semidiameter, - - -	0.0047

Thus it appears that Plantamour, Nicolai, and Encke on two occasions, had encountered the same paradox as ourselves, viz. that of a perihelion point within the sun. It is also remarkable that none of the orbits except Encke's hyperbola suffice to represent the observed path of the centre of the nebulosity among the stars.

Hence it appears from the concurrence of authorities on these subjects, that good observations of the path of the centre of the nebulosity, carefully reduced, lead to a hyperbolic orbit, and an approach of centres of the sun and comet as near as their physical qualities will permit.

In this stage of the enquiry the principal difficulty consists in reconciling these two paradoxes with our favorite opinion of the identity of the three comets of 1668, 1689, and this year, with a short period of 21⅞ years. Now it is fortunate that in the case of our hyperbola the same natural and plausible explanation that does away with the one paradox does away with the other. The true key to the solution of the difficulty is, we are persuaded, the suggestion first made to us by Mr. Herrick, March 28th, and first suggested to the public, by ourselves, in the United States Gazette of April 6th, viz. the "uncertainty whether the central or densest portion of the nebulosity corresponds with the actual centre of gravity." We now proceed to state the opinions of our esteemed friends and correspondents on this point. Dr. Anderson of New York, writes under date of May 19th and 22d, stating unhesitatingly that the analogies in favor of the identity of the comets of 1668 and 1689, should lead us to reject the hyperbolic orbit as being unnatural in itself, and wholly irreconcilable with these analogies. And that we should rather regard this hyperbolic orbit, and too close perihelion distance, as the consequence of some error in the data, or in the methods, or in the computations. That there is nothing in the effect of contact of the bodies, or resistance of the comet by the atmosphere of the sun, which could change the character of the conic section, from one of a less velocity to one of a greater. From Professor Alexander, of Princeton College, we have received a letter dated May 20th, in which he proposes an explanation of the difficulty at once simple and natural, and fulfilling all that was required by Dr. Anderson. It is based on the supposed occurrence of the very error against which we were cautioned by Mr. Herrick, March 28th, and which we alluded to in our published letter of April 6th, namely, the error arising from measuring the place of the densest point of the nucleus instead of the common centre of gravity of the nucleus and tail. We give below his letter in full. We have also had placed in our hands by Professor A. D. Bache, a letter from Professor Bartlett, of West Point, dated May 23d. We give below that part of his letter which treats on this subject, remarking that we have no doubt that the coincidence in opinions of Mr. Herrick, Professor Alexander, and Professor Bartlett, has taken place without either one having any knowledge that the same idea had occurred to the other two. We would also remark that the criticism of Professor Bartlett, on Arago's parabolic elements and on our own, is just, and confirms our statements that no parabolic ephemeris will perfectly represent consecutive observations of this comet. We know of only two sets of elements that will give a good ephemeris; the one is Encke's and the other is ours. Both are hyperbolic and paradoxical. We give them below. The explanation of Professors Alexander and Bartlett, we have no doubt, is the true one. It is plain and natural, and *a priori* extremely probable. It will also satisfy the criticism of Professor Anderson, in as much as it points out the particular source of the error of the data, which Dr. Anderson supposed must exist somewhere. The explanation is doubly satisfactory for ourselves, since it leaves the way clear for the establishment of the short period, and the identity of the three comets of 1668, 1689, and 1843, and leaves us still a hope of seeing this remarkable visitor in 1865. Moreover it does away with both paradoxes, and shows at the same time, that the European astronomers, as well as ourselves, who were led into them, arrived at them in the legitimate and only possible mode of observation and computation.

Professor Bessel, of Königsberg, the greatest living astronomer, and since Olbers's death, the most experienced and sagacious observer of comets, remarks in a letter to Prof. Schumacher, dated March 28th:—"This comet seems to have expended the greater part of its nucleus in building up its splendid tail."

We are happy to add the testimony of our friend Mr. Nicollet in favor of the strength of these analogies, and of the probable return of this comet in 1865, as an inference not to be in the slightest degree shaken by the fact that a nice discussion of the observations of the apparent centre of the nebulosity has led to the two paradoxes already quoted. We hail the favorable opinion of this distinguished traveller, who received the Lalande medal for the discovery and the elements of the comet of the year 1821. We are happy further to add the testimony in favor of the plausibility of the period of 21⅞ years, communicated to us in writing or verbally by our valued friends, Alexander, of Princeton; Mitchell, of Nantucket; Gilliss, of Washington; Herrick, of New Haven; Loomis, of Western Reserve; and, nearer home, of Professors Patterson and Bache.

Princeton, Saturday, May 20, 1843.

My dear Sir—Your esteemed favor was received last evening. I have entire confidence in the scrupulous care with which your observations have been conducted, and do not doubt that the computations founded upon them have been well guarded; yet that the comet should have actually struck the sun or his envelope, and then *rebounded*, seems to me to be so violent a supposition, as to be inadmissible, except upon compulsory evidence, or in the absence of any other rational explanation. Admitting the facts, however, to be as above stated, how are we to avoid the conclusion? I will venture to suggest what I am at present disposed to regard as a plausible solution of the difficulty.

The centre of gravity of the comet of 1843 was at an unusual distance from that which seemed to be the actual nucleus: this led to an erroneous estimate of the comet's position. As, moreover, the comet, when first observed, was nearly in its perigee, it is altogether possible that the error arising from the cause here suggested, was at the same time at its maximum, and that it continually decreased until the comet disappeared. The effect upon the relative position of the apparent and true orbits would consequently be such as is roughly represented above—the true or dotted orbit deviating more and more from the apparent, as we retrace it in the direction opposite to the comet's motion, and thus escaping the sun at the perihelion.

I am obliged to pause, as the hour has arrived for closing the mail. I hope to see you at Philadelphia in a very few days, and may perhaps write you again before that time, in answer to the question you more particularly propose.

In extreme haste, yours truly,

Stephen Alexander.

Sears C. Walker, Esq., Philadelphia, Pa.

West Point, May 23d, 1843.

My dear Bache—The more immediate purpose of this letter, is to suggest to yourself and the Society, what has appeared to me a possible explanation of the very great discrepancies between the observations and both the ephemerides computed from M. Arago's and Mr. Walker's elements.

I suppose that the *apparent* orbit of the comet is different from the *true;* or that the path of the nucleus is not the same as that described by the centre of gravity of the entire mass. To illustrate my meaning, suppose the comet to approach the sun in a parabolic or very elongated elliptical orbit, which will be, by the principles of physical astronomy, the path of the centre of gravity. As the comet approaches the perihelion, let it be greatly but gradually elongated in the direction of a line joining the nucleus and the sun, the tail being thrown off in a direction from this latter body, and suppose this to result from the repulsive action of the cometary particles upon each other, in consequence of the heating influence of the sun, in the same manner as the elastic force of vapor is increased by an elevation of temperature. The action being limited to the particles upon each other, the centre of gravity will be undisturbed, and continue to describe its regular orbit from which each extremity of the elongation will recede on the line of the radius vector, though in unequal degrees, till it reaches a maximum, resulting from an equilibrium between the elastic force of the cometary medium and the weight of its elementary particles, or the force by which they are drawn towards the centre of the mass.

The expansive action here supposed, would, in the nature of things, be gradual; and hence, before the nucleus, or the *thing observed*, could be totally resolved into a vapor like the tail, and thus disappear, the reverse action would begin, in consequence of the rapid retrocession of the comet from the sun. The disturbed motion of the nucleus being for a part of the time *from* the true orbit, or that of the centre of gravity, *towards* the sun, the observations, if made at this time, would give a constantly increasing eccentricity, or diminishing perihelion distance; and thus the perihelion itself might be brought apparently within the surface of the sun, while not a particle of the comet's matter would touch that body. The observations, if made while the mass of the comet is contracting towards its centre of gravity, would give an increasing perihelion distance till this point is again brought within the nucleus in the depths of space. Very truly yours,

WM. H. C. BARTLETT.

A. D. BACHE, LL. D., Philad.

We subjoin Encke's hyperbolic elements, and also our own. The latter have been recomputed, after correcting a slight oversight in our calculations, kindly pointed out by Prof. Anderson. Encke's ephemeris agrees beautifully with his observations. Our hyperbolic elements give an ephemeris corresponding with our normal places within one second of space:

	Encke.	*W., K. and D.*
Perihelion passage,	Feb. 27d.49778 m. t. Berlin.	27d.58939 m. t. Green.
Longitude of perihelion,	279° 2′ 29.9 m. eq. March 0	280° 44′ 3.7 m. eq. March 30
Long. asc. node, . .	4 15 24.9	15 57 3.2
Inclination, . . .	35 12 38.2	34 19 52.0
Eccentricity, . . .	1.00021825	1.00090495
Gaussian angle,* . .	1° 11′ 49″.0	2° 26′ 12″.1
Perihelion distance, . .	0.00521966	0.00410367
Daily motion retrograde,	13″.175559	159″.58936

It will appear on comparing these elements, that they agree very well, excepting the eccentricity and its secant, the Gaussian angle. This is always the most uncertain element in such investigations. We might *a priori* believe that our result has more weight, from being derived from twenty two days' motion of the comet, whereas Encke's was derived from only eight days' motion. Moreover, our places were normal, or average places, and his derived from observations of a single night. A third argument in favor of our elements is, that they were derived directly from the normal places, without any hypothesis respecting the conic section. Whereas Encke's were obtained by variations from a parabolic curve acknowledged to be erroneous. That there can be no error in the process of computation by Mr. Downes and ourselves, is shown by the fact that the elements reproduce by computation our normal places, after applying the following small corrections, viz.

* This angle is the arc whose secant is the same as the eccentricity.

March 20.5	R. A. −0″.6	Dec. +0″.7
" 30.5	R. A. −0 .0	Dec. −1 .0
April 9.5	R. A. −0 .6	Dec. +0 .3

These normal places were obtained from a comparison of all our observations with the best ephemeris we could obtain, which was computed from our elements at our request, by Mr. John Downes, the editor of the United States Almanac, and obtained from the average corrections concurring together near the 20th and 30th of March, and 9th of April, for Greenwich mean midnight. These are far more correct than the result of any single measure. We give them for the use of astronomers freed from refraction, parallax, and aberration.

March 20^d.5,	R. A. 46° 4′ 38″.4	Dec. S. 9° 9′ 45″.5
March 30^d.5,	59 51 1 .2	6 36 32 .5
April 9^d.5,	68 56 41 .6	4 45 35 .7

Let us now consider the period belonging to the mean motion. It is obvious that if we adopt the explanation of Messrs. Alexander and Bartlett, the mean motion and consequent period of the centre of the *nebulosity* observed, and of the real centre of *gravity*, must be the same. This is a necessary condition, since they both arrive at the perihelion point at the same instant of time. Now the earth's sidereal motion in a mean solar day is 3548″.18761. The mean motion of the apparent centre of the nebulosity by our elements is 159″.58936. This gives a period for the apparent centre of the nebulosity, and consequently for the actual centre of gravity, of 22.2339 years.

This is the keystone of the arch; it is the last argument that was wanting to complete the conclusion. Analogy had already raised a violent presumption of a period of 21$\frac{7}{8}$ years. The same, or nearly the same period, has here been derived by a process entirely independent of these analogies, and entirely free from any hypothesis respecting the period or nature of the conic section that forms the orbit. The coincidence is wonderful, and shows not only the strong probability of the period, and of the identity of the three comets, but also the extreme precision of the normal places, derived from our measures with the filar-micrometer; for an error in any of these places of 10″ would have led to a greater discrepancy.

We have presented the argument *a posteriori*, from the nature of the orbit observed in March and April last. We have found a period that in seven revolutions reaches back nearly to 1689, and in eight revolutions to 1668. We now present the argument *a priori* derived from analogy.

Number.	Comet.	Lon. of Perihel.	Lon. of Node.	Inclination.	Perihelion distance.	Perihelion passage.
1	Comet of 1668	279°.6	359°.8	35°.9	0.0048	Feb. 28.8*d*
2	Comet of 1689	273 .5	346 .5	30 .4	0.0103	Dec. 2.13
3	Comet of 1843	280 .5	348 .5	39 .3	0.0087	Feb. 27.54
4	"	272 .3	356 .5	36 .6	0.0147	27.20
5	"	262 .7	357 .7	36 .7	0.0541	27.55
6	"	277 .0	361 .3	35 .7	0.0082	27.45
7	"	274 .8	359 .1	35 .9	0.0109	27.09
8	"	274 .5	357 .6	36 .4	0.0113	27.46
9	"	279 .2	359 .9	36 .0	0.0045	27.46
10	"	281 .4	365 .9	35 .0	0.0030	27.37
11	"	275 .5	359 .0	36 .1	0.0104	27.54
12	"	277 .5	361 .0	35 .7	0.0071	27.47
13	"	280 .5	364 .6	35 .2	0.0037	27.39
14	"	278 .8	362 .2	35 .5	0.0054	27.41

No. 1. By Henderson, Astronomer Royal of Scotland.
2. By Prof. Peirce, from Pingré's places.
3. By Prof. Peirce, from his and Mr. Bond's places.
4. By Messrs. Nooney and Hadley, from Walker and Kendall's places.
5. By Prof. Anderson, from Prof. Bartlett's places.
6. By Prof. Anderson, from Walker and Kendall's places.
7. By Prof. Alexander, from his own places.
8. By Mr. Galle of Berlin, do.
9. By Mr. Plantamour of Geneva, do.
10. By Prof. Encke, do.
11. By Walker, Kendall and Downes, do.
12. By Argelander, do.
13. By Nicolai, do.
14. By Laugier and Mauvais, do.

Either argument is quite conclusive, and their coincidence establishes almost to a demonstration the period of the comet of $21\frac{7}{8}$ years, and its identity with some of the many others, quoted by Pingré in his Cometography, as having occurred in the three series of cycles of 175 years (8 revolutions) which precede the respective dates of its recent appearance in 1843.2, its expected appearance in 1864.9 or 1865.0, and in 1886.9. It also completely confirms the observation made by Messrs. Herrick and Bradley, of the eccentricity of the densest portion of the nebulosity in that nebulosity. It confirms the remark, we published in the United States Gazette of April 6th. It confirms the coincident opinions of Profs. Alexander and Bartlett. It explains away the seeming paradoxes of the hyperbolic motion of the apparent centre of the nebulosity, and of the tendency of this fictitious curve to a perihelion point within the sun's surface, while the true ellipse of $21\frac{7}{8}$ years' period has a perihelion distance greater than the sun's radius, leaving the comet free to depart and return, as it must do about the 1st of January, 1865, to be seen under more favorable circumstances than at this visit.

We conclude by expressing our great satisfaction at the explanation of Profs. Alexander and Bartlett, which, with the computations of the new orbit, by Henderson, for the comet of 1668, and by Prof. Peirce for 1689, have removed the only known obstacle to the admission of the period of $21\frac{7}{8}$ years and the elliptic orbit suggested by ourselves on the 8th inst.; accordingly we offer it to the members of the Society on this their centennial celebration, as the established period of this remarkable comet.

Encke in 1819, from 21 days' observation of his comet, found by the application of Gauss's method, a mean daily motion of 989″.3, whereas the true motion was 1076″.9. In 1826 Santini found from 30 days' observations of Gambart's comet, a mean daily motion of 700″.4, whereas the true motion was 528″.0. Our mean motions from the elements and the true period are respectively 159″.6 and 162″.2. The conjectures of Encke and Santini turned out to be true. Our coincidence is even closer; but requires an additional hypothesis, that of Messrs. Herrick, Alexander, and Bartlett, which in some degree weakens the inference.

If we admit this hypothesis, and suppose that the perihelion distance was possible, that is, for instance, greater than 0.0047, then we shall find the elliptic elements of the comet's orbit the same as the hyperbolic, omitting the Gaussian angle, and making the eccentricity greater than 0.9994.

The actual elliptic elements may be found on this hypothesis by assuming the above value of 0.9994 for the elliptic eccentricity, and then giving to the difference between the elliptic and hyperbolic radii vectores the form of a constant quantity multiplied by the reciprocal of the square of the elliptic radius vector. This constant should then be determined from the series of observations by the method of least squares. The elliptic elements should of course be used in computing perturbations.

We have the honor to be your obedient servants,

SEARS C. WALKER,
E. OTIS KENDALL.

To Messrs. John K. Kane, Alexander Dallas Bache, LL. D., Robley Dunglison, M. D., Joshua Francis Fisher.

High School Observatory, Philadelphia, June 16th, 1843.

TO THE SECRETARIES OF THE AMERICAN PHILOSOPHICAL SOCIETY.

Gentlemen—Since writing the letter which was read at the centennial meeting of the Society, we have compared our normal places of the comet on the 20th and 30th of March with the European observations. We have not been able to find any later than the 31st of March, and must still rely on our own measures for the comet's place on the 9th of April. In order to test the normal places for March 20th and 30th, we subjoin the differences therefrom of the European observations referred to the date of Greenwich mean midnight, after rejecting two in all, whose discrepancies from the mean result exceeded fifty seconds of space.

Observation compared with			Correction of normal place, $\Delta\alpha$.	Correction of normal place, $\Delta\delta$.	Date of normal place.
Paris,	March	19.5	+ 7″.2	+ 2″.2	March 20.5
Geneva,	"	19.5	+ 7 .0	+ 2 .1	
Rome,	"	19.5	+31 .0	+ 5 .0	
Rome,	"	20.5	— 7 .3	+35 .1	
Berlin,	"	20.5	+ 5 .4	—23 .9	
Munich,	"	20.5	+ 3 .4	—21 .4	
Manheim,	"	21.5	—13 .9	—26 .1	
Geneva,	"	21.5	— 2 .6	—28 .3	
Bonn,	"	21.5	+ 0 .5	—28 .1	
Berlin,	"	21.5	— 7 .1	—20 .9	
Munich,	"	21.5	—23 .5	+ 0 .5	
Vienna,	"	21.5		+31 .0	
Mean correction,			—00″.0	— 6″.1	
Manheim,	March	29.5	+ 0″.9	—13.″9	March 30.5
Bonn,	"	29.5	— 3 .3	—16 .1	
Manheim,	"	30.5	+20 .6	+15 .5	
Berlin,	"	30.5	+ 2 .1	—19 .5	
Berlin,	"	31.5	+33 .3	— 0 .8	
Mean correction,			+10″.7	— 7″.0	

If we allow to the High School observations the same weight as that of one European observatory, then the normal places of the point of observation of the comet's nebulosity will stand thus, being freed from parallax and aberration.

Greenwich mean time.	Normal place, R. A.	Correc. R. A.	Corrected normal place, R.A.	Normal place, Dec.	Correc. Dec.	Corrected normal place, Dec.
Mar. 20d.5	46° 4′ 38″.4	+0″.0	46° 4′ 38″.4	-9° 9′ 45″.5	-5″.4	-9 9′ 50″.9
Mar. 30 .5	59 51 1 .2	+9 .5	59 51 10 .7	-6 36 32 .5	-5 .2	-6 36 37 .7
April 9 .5	68 56 41 .6	+0 .0	68 56 41 .6	-4 45 35 .7	-0 .0	-4 45 35 .7

Then the corrections of the ephemeris, computed from our hyperbolic elements, will be

March 20.5	$\Delta\alpha = -0''.6$;	$\Delta\delta = -4''.5$
March 30.5	" $+9''.5$;	" $-6''.2$
April 9.5	" $-0''.6$;	" $+0''.3$

These values are so small that a change in the elements of the orbit of the point of the nebulosity observed, which should reduce them to zero, would be too small to indicate any change in the conclusions already drawn by us from our first normal places. Unless, then, further observations shall be obtained from the southern hemisphere previous to the perihelion passage, we see no way of avoiding the conclusion that the point of the nebulosity observed was moving in an hyperbola, with a mean daily motion of about 160″, which in a curve having a periodical character, would give a duration of a revolution of about 22 years, with elements, as far as we know, identical with those of the comets of 1668.2 and 1689.9.

We subjoin from Pingré's Cometography a list of comets that have appeared at dates when this comet, if it be the same as those of 1668.2, and 1689.9, must have been in a situation to be seen from some part of the earth. It must be recollected that this comet can never have come to its perihelion in the months of November, December, January and February, without being a conspicuous object in the morning or evening twilight, before or after the passage of the perihelion. In all instances it must have been best seen in the southern hemisphere. We have given nearly all the coincidences in dates. Those which have no (*), nor (?) annexed are coincidences in date. Those marked with an (*) have, besides the coincidence in date, some circumstance, whether of physical appearance or apparent path in the heavens, analogous with the comet of 1843. Those marked with an (?) are probably mere coincidences in date without being the same individuals.

Date.	Periods of eight revolutions preceding the recent appearance.	Single revolutions and mean period.
B. C. 432	13×175.00	104×21.876
A. D. 268 (?)	9×175.02	72×21.878
442 (?)	8×175.15	64×21.894
617	7×175.17	56×21.896
968	5×175.04	40×21.880
1143 (*)	4×175.05	32×21.881
1317	3×175.40	24×21.925
1493	2×175.10	16×21.888
1668.2 (*)	1×175.00	8×21.875
1843.2		

Date.	Periods of eight revolutions preceding expected return in 1865.04.	Single revolutions and mean period.
B. C. 60 (*)	11×175.00	88×21.875
A. D. 290 (*)	9×175.00	72×21.875
639 (?)	7×175.14	56×21.892
815	6×174.99	48×21.874
990 ?	5×174.99	40×21.874
1165	4×175.23	32×21.904
1340 *	3×174.98	24×21.873
1516 ?	2×174.47	16×21.809
1689.95 (*)	1×175.00	8×21.875
1865.04		

Date.	Periods of eight revolutions preceding expected return in 1886.91.	Single revolutions and mean period.
B. C. 213	12×174.99	96×21.874
A. D. 488	8×174.85	64×21.856
837	6×174.97	48×21.871
1012 (*)	5×174.96	40×21.870
1362 (*)	3×174.94	24×21.867
1537 (*)	2×174.91	16×21.864
1886.91		

We have stated the opinion of several able astronomers, that the densest portion of the nebulosity of the recent comet (necessarily selected as the proper point for micrometric measures) was eccentric towards the sun from the real head or centre of gravity of the comet, tail and nebulous envelope. In fact the comet never presented any appearance of a distinct kernel or head, but only a vague and ill-defined nebulosity or cloud, gradually condensed towards the centre, or, according to Messrs. Herrick and Bradley, towards a point nearer the sun than the centre of the disc (if we may so call it) of the nebulosity. In Prof. Bartlett's letter, mention is made only of the elastic force of the vaporous matter surrounding the comet and composing its envelope or tail. On this hypothesis we have suggested the simplest and most natural method of completing the elliptic elements, viz. that of making the excess of the supposed elliptic over the actual hyperbolic radius vector of the point observed equal to a constant co-efficient of the reciprocal of the square of the radius vector, and determining by means of the constancy of this value, the actual eccentricity corresponding to a period of $21\frac{7}{8}$ years, and a perihelion point of the centre of gravity or head of the comet actually outside of the sun though nearly in contact with it. In fact this multiplier is not necessarily constant, nor necessarily a co-efficient of the reciprocal of the square of the radius vector; still this hypothesis is the most simple and plausible that can be made, and is perhaps quite as complex as the nature of the question permits us to make.

As the subject of the physical organization of the head, tail, and nebulosity or envelope of comets, has been discussed by Sir William Herschel, Olbers, Brandes and Bessel, with their characteristic genius and acumen, we deem it proper to consider the bearing of their opinions and researches on the present question.

Sir William Herschel* states that the kernel or head of the great comet of 1811, of about 1″ in diameter, could only be seen with high powers in his most powerful telescopes, and that with ordinary instruments he saw only the nebulosity or envelope; but that when the head or kernel was seen, it was seen within the envelope eccentric from the sun, or in other words the densest portion of the envelope or nebulosity was eccentric towards the sun. This is precisely the phenomenon observed by Messrs. Herrick and Bradley with reference to the disc of the nebulosity, though the kernel or head could not be seen. This also agrees with Bessel's remark, that this comet seems to have thrown out nearly all its head in forming the nebulosity and tail.

We come next to Olbers's theory† of the formation of the envelope and tail of comets. This was promulgated in 1812, shortly after the appearance of the great comet of 1811. We do not recollect to have any where met with a translation of it. It is perhaps the only theory ever proposed that explains all the phe-

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* Monatliche Correspondenz, Vol. XXVIII, p. 459.
† Ibid., Vol. XXV, p. 3.

nomena observed respecting that comet. Olbers supposes that any particle composing the surface of the comet, or approaching from the frozen regions of space within a certain distance of the sun, is affected with a new repulsive force resembling that which drives off substances from an excited prime conductor. These particles thus polarized he supposes to be thrown off from the head of the comet with a force proportioned to the mass of this head or nearly solid portion of the comet, and inversely as the square of the distance from the centre of this head. The same particle in acquiring polarity with reference to the comet, acquires also polarity with reference to the mass of the sun, and is repelled by that mass, instead of being attracted by it with a force also varying inversely as the square of the distance from the sun. The origin of this polarity may be ascribed to the action of the sun's light or heat, or both. This particle thus endued with one repulsive force acting in the direction of the prolongation of the radius vector from the sun, and inversely as its square, and with another repulsive force acting in the direction of the prolongation of its radius vector from the centre of the comet and inversely as its square, and with its original tangential velocity, at the time of parting with its actual cohesion with the comet, moves away in space in such a manner as not to return. Now the geocentric position in the heavens with respect to the head of the comet, of any such particle for any given elapsed time after it is thrown off from the comet's surface, may be readily computed, from the known tangential direction and velocity for any assumed values of the two repulsive forces of the comet and sun, for a unit of distance—say the earth's mean distance from the sun. Olbers remarks that the heliocentric orbit of such a particle must be an hyperbola, moreover that the points in space where the two repulsive forces of the sun and comet make equilibrium, for any original direction of repulsion from the surface of the head, must have the portions of expelled matter more condensed than any portion of space between such points and the comet's head, thus form apparently a hollow envelope or nebulosity, in the shape of an hyperboloid having the head of the comet in its internal focus, and its apex towards the sun, the continuation of this hollow hyperboloid from the sun beyond the parameter, (so to speak,) forms the tail of the comet. The shape of the hyperboloid, and consequently of the tail, depends upon the ratio of the repulsive forces of the sun and comet. If the one is determined by measure, the other can be computed from it. Olbers made measures of the shape of the visible section of this hyperboloid.

Brandes* gave this theory a thorough discussion, and finds analytically that the opinion of Olbers is true, that the envelope if so caused must be an hyperboloid, and then from the observed dimensions of a section of this hyperboloid as seen from the earth computes the ratio of the two repelling forces for that comet.

The theory of the formation of comets' tails seems to have made but little advances from 1812 till the return of Halley's comet in 1835, when the astronomers of Europe, with a full knowledge of Olbers's theory, and with powerful instruments, observed the tail and nebulosity of that comet, with reference to this theory.† Struve remarked that the densest point of the nebulosity was eccentric in that nebulosity, conformably to the observation of Messrs. Herrick and Bradley for the recent comet. But the most indefatigable observer was Bessel,‡ with the Königsberg heliometer. He detected a pendulous or vibratory motion of that portion of the nebulous matter, which was expelled from the hemisphere of the comet next the sun, resembling that of a magnet round the magnetic pole. He finds that with the addition of this pendulous motion of this streaming matter, or in other words of the apex of the hyperboloidal envelope of the comet in the plane of the orbit, and for fifty degrees or more on each side of the antipode of the comet's tangential direction, all the observed phenomena of the tail of Halley's comet may be explained. Bessel then proceeds to compute the repulsive force of the sun and comet on these expelled particles from the observed shape of the tail. He also computes for any point of the tail how long the expelled particle had taken since the date of its expulsion to arrive at the point observed.

All these phenomena Bessel explains upon the supposition that there is no molecular attraction or repulsion between the particles thus expelled; but that their heliocentric motion is due to the tangential direction in which they are expelled from the comet, and to the two forces of repulsion of the sun and comet, and the

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* Monatliche Correspondenz, Vol. xxvi, p. 533.

† Schumacher's Astronomische Nachrichten, Vol. xiii, p. 303.

‡ Ibid., Vol. xiii, p. 177. Also Schumacher's Jahrbuch for 1837, p. 142. Con. des Tems, 1839.

orbital direction and velocity of the particle at the time of expulsion. The reason assigned by Bessel for not supposing a molecular connection between the particles of the nebulosity of the comet, is the fact that the rays from a star seen by himself in the Königsberg heliometer, through this nebulosity, were not refracted by the medium of the nebulosity, but preserved, when seen through this medium, the same relative position with reference to any other fixed star as when seen before entering the medium. Bessel argues that this could not have been a vapor, gas, or air, through which the star was seen, or it would have been refracted.

Having stated the principal points of the theories of Olbers and Bessel regarding the formation of comets' tails, it remains to consider their bearing on the present question. It is obvious that the difference between the radius vector of the head and of any particle (one in the point aimed at for instance) of the nebulosity, would not be a simple multiplier of the reciprocal of the square of the radius vector; but would on the contrary include a term depending upon the reciprocal of the square of the distance of this particle from the centre of the head. It may be shown, however, from the length and narrowness of the tail of the recent comet, that this latter term is nearly insensible, since the comet's expulsive force compared with the sun's, must in this instance have been very small, for when acting in the normal to the radius vector, it was able to impress but a small normal velocity on the particles thrown off.

We conclude then that whatever theory of the formation of comets' tails we adopt, if we suppose the particles of the nebulosity and tail to be material, and the densest portion of the former to be extended from the centre of the comet's head towards the sun, the simplest method of deriving the true elements of the orbit of the head, from the computed elements of the orbit of the point observed, is that which we mentioned in our letter of the 25th of May last.

We subjoin the elements of the comet computed from our first normal places by Prof. Anderson of Columbia College, New York, viz.

Perihelion passage, . . .	Feb. $27^d.579857$ m. t. Green.	
Longitude of perihelion, .	$279^\circ 40' 35''.50$	m. eq. Mch. 30.
Long. of ascending node, .	$15^\circ 0' 56''.45$	
Inclination, ...	$34^\circ 21' 6''$	
Eccentricity, ...	1.0008560	
Gaussian angle, ...	$2^\circ 23' 0''.76$	
Perihelion distance, . . .	0.00415697	
Mean daily motion retrograde,	$146''.50299$.	

We have the honor to be your obedient servants,

Sears C. Walker,
E. Otis Kendall.

To Messrs. John K. Kane, Alexander Dallas Bache, LL. D., Robley Dunglison, M. D., Joshua Francis Fisher.

Art. XXII.—*Remarks on Mr. Owen's Letter to the Editors on Dr. Harlan's New Fossil Mammalia.*

[to the editors of the american journal of science.]

New Orleans, May 5, 1843.

Gentlemen—In the last number of your Journal, (Vol. xliv, No. 2, p. 341, April, 1843,) I was gratified with the perusal of an interesting letter by Prof. Owen of the Royal College of Surgeons, London. The observations and opinions of Dr. O. on fossil osteology, are entitled to the highest respect; placed at the head of the richest osteological collection in the world, and endowed with a genius which peculiarly qualifies him for the successful prosecution of his favorite department of science, he has perhaps accomplished more for its advancement than any other single living laborer in this attractive field of research. His criticisms on my "notice of new fossil mammalia," are conceived in the proper spirit, having no other object than the advancement of science, and as such are duly acknowledged.

I write under a full conviction of the difficulties attending my present isolated position: there does not exist a scientific library within a thousand miles, and the natural and physical sciences have consequently few votaries.

The observations of Dr. Owen and myself on certain fossil mammals, have resulted in some discrepancies of opinion, which I conceive require some explanation on my part.

Prof. Owen pronounces me in error in supposing that his genus "*Mylodon*," is founded on my genus *Megalonyx laqueatus;* the latter he admits is a true *Megalonyx*, and by no means to be included in his *Mylodon*. My original description of *M. laqueatus*, was published in the Philadelphia Journal of the Academy of Natural Sciences in 1831. Subsequently, perhaps about the close of the same year, I published in the "American Monthly Journal of Geology," "a description of the jaws, teeth, clavicle, &c. of the *Megalonyx laqueatus*"—fossils then in the collection of Mr. Graves of New York.

Now, the "error" above noticed, rests entirely on a difference in opinion between Dr. O. and myself respecting the true nature of these New York specimens. On a careful inspection at the time, I considered them as belonging certainly to the genus *Megalonyx*, and as closely allied to if not identical with the *M. laqueatus*. But when the entire skeletons of individual species cannot be examined, it is not easy to pronounce with certainty in all cases, on the identity of species. Dr. O. relies in every instance on the structure, arrangement, &c. of the teeth, in his designation of species, without conceding to other portions of the skeleton a relative importance. To me it appears that the organs of mastication, viewed alone, are more liable to lead to error in forming distinctive characters than are the organs of locomotion; thus, in form, structure, and arrangement of the enamel, the superior molars of the horse differ more from the inferior molars of the same individual than do the molars of the *Megalonyx* of the New York specimens from the *M. laqueatus* of my original memoir.

On a careful examination and comparison of the tibiæ in both specimens of *Megalonyx*, I could perceive no specific difference, much less discrepancies authorizing the adoption of a new genus for it, as Dr. O. has done under the name of *Mylodon*. The form and structure of the tibia in my new genus "*Orycterotherium*" of Missouri is totally distinct from either. Professor Owen has founded his observations of the characters of the New York species on the drawings of the cast of the jaws and teeth, together with the figures accompanying my memoir. In the present state of our inquiries, I think that we are not yet prepared to pronounce with certainty that "the name *Orycterotherium Missouriense*, must sink into a synonym of *Mylodon Harlani*."

Professor Owen proceeds to state, "The *Megalonyx laqueatus* of Dr. Harlan is a true *Megalonyx*," [but which was not "*mainly founded on the cast of a tooth.*"] "Nor can I conceive any reasonable ground for its specific distinction from the *Megalonyx Jeffersonii.*" Perhaps not, if we confine our observations to teeth. Of the latter animal we have as yet only the bones of the fore-arm and hand, and a broken tooth, to rest our opinion on; I think, however, that in a comparison of the radius alone of the two specimens we might detect "reasonable grounds for specific distinction." I have no means in reach of comparing the distal extremity of the tibia of the *Orycterotherium Missouriense*, with the same part in the Brazilian species which forms the type of Dr. Owen's "*Mylodon.*"

Dr. O. remarks at p. 345 of the letter above quoted: "The tibia of the Missouri *Mylodon* corresponds with that of the *Megatherium*, in the deep ovoid depression at the anterior and internal part of the lower articular end." He also supposes that a similar structure will be found to characterize the tibia of the "*Scelidotherium*," and further on continues: "The Megatheroid family thus appears to have been as strikingly distinguished by this structure of the ankle-joint as the sloths are by the pivoted articulation of the astragalus with the fibula." It is somewhat surprising that two experienced anatomists should differ so widely on a structure submitted to ocular demonstration; in the tibia of the *Megalonyx laqueatus* originally described by me, as also in the specimen in possession of Mr. Graves, there is not the least approach to such a conformation. Nor will the tibia of the *Megatherium* show any such comparison with the same part in the *Megalonyx;* they all differ widely in this respect from the *Orycterotherium.*

Further on, p. 345, Dr. O. states: "In my report on the '*Missourium*,' printed in the proceedings of the Geological Society, Dr. Harlan will find that I have duly acknowledged the originator of the opinion that the '*Tetracaulodon*' was nothing but the *young* of the gigantic *Mastodon.* To Mr. Wm. Cooper of New York, the honor of this insight belongs." This paragraph involves two errors; the "*Tetracaulodon*" was not founded on characters peculiar to a *young* animal; nor was it first announced by Mr. Cooper that the "*Tetracaulodon*" is only a variety of the *Mastodon.* That the four tusks exist in the skull of the adult *Mastodon* is abundantly testified by specimens now in numerous cabinets, including that of Mr. Koch's, if we rightly remember, and by reference to dates it would be easy to show, that I immediately announced its identity with the *Mastodon*, in a paper read before the American Philosophical Society in 1830, shortly after the appearance of Dr. Godman's memoir. At first we had only noticed this additional tusk in the young animal, but in the following year, when on a visit to the University of Virginia, at Charlotteville, we were shown adult jaws with the same peculiarity, and published the fact in the "American Monthly Journal of Geology." I beg to be understood, Messrs. Editors, to make these comments on Dr. Owen's remarks in good faith and kind feelings, and with the utmost deference to the opinions of my valued friend, Prof. Owen; but we are none of us immaculate. In all my investigations I have striven for truth and not for victory. I am, therefore, not impatient of criticism; on the contrary, when conducted in the manner of Dr. O., I court it, as subservient to truth and to the best interests of science. We are proud to bear testimony to the very general accuracy of his published facts; in matters of mere opinion, however, an honest difference must be anticipated. We are very willing to concede, that in most cases it will prove in the end that "it is not Homer nods, but we that sleep."

I sincerely hope that Prof. Owen will frequently favor the readers of your important Journal with his valuable observations.

I have the honor to be, gentlemen, your much obliged friend,

R. Harlan.

Art. XXIII.—*Bibliographical Notices.*

1. *Histoire Naturelle des Poissons d'Eau Douce de l'Europe Centrale;* par L. Agassiz. *Embryologie des Salmones;* par C. Vogt. Neuchatel, 1842. Planches.—M. Agassiz, in order to render his excellent monograph on the fresh-water fishes of Central Europe as complete as possible, has determined to enter into the details of anatomical structure and development of the different natural families, which will come under his observation. To this end he has associated with him M. Vogt, the author of the present livraison, comprising "l'Embryologie des Salmones." This is founded on the original observations of

the author, commencing with the earliest period at which the ovum comes within the reach of the microscope, and following it through its various stages of development, terminates with the escape of the embryo from its membranous envelopes.

The arrival of the spawning season of the Coregonus palæa, the species experimented upon by M. Vogt, is announced by these fishes associating in pairs, and jumping frequently above the surface of the water, the female protruding ova at the same time that the male ejects the seminal fluid; by these simultaneous movements, impregnation is effected, but large numbers of ova escape the fecundating influence. In order to procure a sufficient number of ova for microscopic observation, he has found it most convenient to have recourse to artificial impregnation, which is more successful, as regards the numbers impregnated, than when effected in the natural manner. The artificial mode consists simply in squeezing ova from the female, and the seminal fluid from the male, and the two are brought in contact by being placed in a vessel containing water. The development takes place perfectly well in the house, provided water is used taken from the locality in which the fish usually deposits its ova. This last is by no means an unimportant step, since his experience goes to show that ova which are ordinarily deposited in lakes, are destroyed or blighted, if water from rivers is made use of, and *vice versa.* The water should also be frequently agitated and changed, and all sudden variations of temperature avoided; the most favorable degree of warmth, being from 4° to 8° R. or from 40° to 50° F. Congelation of the water in which they are contained, retards their progress, but does not destroy life.

He finds the ova liable to diseases, and like those of some species of Limax, are often attacked by vegetable growths, consisting of slender jointed filaments, often expanded at their free extremities, giving the egg a milky appearance, and finally destroying its vitality. He has seen the same growths attacking young fishes, and causing death in the course of eight or ten days. If the same specimen is to be brought frequently under the microscope, care should be taken to keep it immersed in water, and not to keep under the instrument beyond a few minutes at one time. If the outer or shell membrane is to be removed, it is most easily accomplished under water; if the vitelline membrane is to be opened, this should be done in the air, for the vitellus is rendered completely opaque, by being brought in contact with water. We mention these different steps, for the benefit of those who wish to engage in similar investigations.

If the ovary of the C. palæa is examined in the spawning season, ova of different degrees of development are to be seen, but all consisting of the same essential parts. According to M. Vogt, the following is the order of events in the formation of an ovum ; a simple cellule is first produced in the substance of the ovary, which has been denominated "la vesicule germinative," germinating vesicle, or vesicle of Purkinje ; this, after it has acquired a certain size, forms exterior to and enclosing itself, a second which is the "vitelline membrane," and at the same time there is formed in the interior of the germinating vesicle a number of granules, which constitute what is called the "tache" or "taches germinative," germinating spot or spots—they being for the most part grouped together, so as to form a single granular mass. These different parts once formed, increase in size, but in different proportions. When first formed, the vitelline membrane is only large enough to enclose the germinating vesicle ; but when the vitellus is perfectly formed, it is many times larger. The entire ovum having acquired the diameter of $\frac{5}{8}$ line escapes from the parietes of the ovary into the cavity of the abdomen, having previously acquired an enveloping membrane, which corresponds to the shell membrane of the eggs of birds.

The mature ovum, which is susceptible of impregnation, is nearly transparent, having a slightly yellowish tinge, resulting from the presence of globules of oil in the vitellus, which, in consequence of their lightness, occupy its most elevated portions, constituting what is denominated the oil disk. The vitellus contains no cellules whatever, and has a very strong resemblance to albumen. Immediately on being immersed in water, the latter penetrates the shell membrane, and accumulates between it and the membrane of the vitellus, occupying the place of albumen in the eggs of birds and reptiles, which has no existence in ova of the C. palæa. The vitellus floats and turns freely in the watery fluid.

As to the effects of the seminal fluid, the author says but little, although beyond a doubt indispensable to the development of the genus. The difficulty of appreciating its immediate effect, may be estimated from the fact that impregnated and unimpregnated ova manifest, during the first day, precisely the same changes ; after this period, however, the difference is striking ; the ova going on through the successive steps of the development of organs, and the other becoming opaque, soon passes into a state of putrefaction.

M. Vogt has given detailed descriptions and figures of the evolution of the different organs, and the changes which they undergo during embryonic life ; not commencing, however, with the first appearance of individual organs merely, but going back to the formation of the cellules, out of which the organs themselves are generated. The nervous system, skin and muscles, intestinal canal, with its appendages, and the sanguiferous and respiratory systems are described in turn, as they each pass through their successive transformations. The formation of cellules and their transformations into tissues and organs, form one of the most prominent topics treated of in this work, and it will be obvious to those who have paid attention to the subject, that the author's views with regard to these changes, vary somewhat from those of Schwan, Valertin, and others, which at the present day are the most generally accepted. It is well known that the researches of Mirbel and Schleiden, have proved that all the organs and parts of plants are originally composed of simple cellules; and Schwan, in making similar researches with regard to animals, has been led to the conclusion, "that in their primitive state, all the tissues are composed of cellules, and that the elements of all the organs, whatever their form, are also generated from them." The researches of Vogt confirm these results, but as regards the formation of the cellules themselves, he entertains different views. Schwan maintains that the *nuclei* and *nucleoli* are primary, and the germinating vesicle, secondary formations; whereas, according to Vogt, the vesicle is in nearly all cases formed first, and the nuclei and nucleoli subsequently. In the cellules from which the epidermis, black pigment and intestinal canal are formed, no nucleoli make their appearance, until a comparatively advanced period. The nuclei are much more generally found than the preceding, a very few cellules being destitute of them; but these he thinks are as clearly secondary formations, since they cannot be recognized until some days have elapsed. With regard to the truth of the author's views, as respects this and some other subjects treated of, it will be impossible to form a correct estimate, without going over the same ground and repeating his experiments. Setting aside, however, the controverted points, we think it will be readily admitted that his observations form an exceedingly valuable addition to the science of embryology, and the labor and industry with which these observations have been conducted, justify us in forming a high estimate of the results of his future researches. Agreeable to the plan adopted by M. Agassiz, this livraison will be followed by another, by the same naturalist, on the *anatomical structure* of the Salmonidæ.

2. *Hooker's Icones Plantarum; or Figures, with brief descriptive characters and remarks, of new or rare Plants, selected from the author's Herbarium.* New Series, Vol. I, 1842; and Vol. II, part 1, January, 1843. (8vo., plates 401–550.) London: H. Baillière.—We duly informed our readers, (Vol. XLIII, p. 189,) that the new series of this low-priced and very valuable work was to be continued regularly as a quarterly publication. Three numbers of the continuation have now reached us; which are principally devoted to the illustration of the rar-

ities of Gardner's collections in Brazil, Schomburgk's in Guiana, Skinner's in Guatemala, Linden's in Mexico, Mr. Wright's, &c. in the Falkland Islands, and Cunningham's, &c. in Australia. One of the most remarkable of Dr. Gardner's plants is his *Utricularia nelumbiifolia*, (t. 505;) which sends up from a creeping stem numerous scapes more than two feet high, bearing a raceme of very large violet-colored flowers, and round, centrally peltate leaves 3 or 4 inches in diameter, which resemble those of the Nelumbium! Among those from Mr. Skinner's collection in Guatemala, we were surprised to meet with a Smilacina, (*S. flexuosa*, Hook.) The antarctic plants are especially interesting. Among them we have a figure (t. 492) of the *Bolax glebaria* of Commerson, one of those dwarf and singularly tufted Umbelliferous plants so characteristic of the vegetation of the southern extremity of this continent. The rounded and excessively dense tufts of this species, which in their young state D'Urville compares to mole-hills covered with green turf, at length, according to Mr. Wright, resemble small haystacks! Their appearance, we imagine, is not unlike that of the larger masses of *Diapensia Lapponica* upon the Alpine summits of the White Mountains, only that they are on a much greater scale. The *Dalibarda geoides* of Persoon and DeCandolle turns out a genuine Rubus. We find only two North American plants, viz. *Carex filifolia*, *Nutt.*, (under which a wrong name is inadvertently cited in place of that of the more able author of the Monograph of North American Cyperaceæ,) and *Oakesia Conradii* of Tuckerman. Sir Wm. Hooker has not the fruit of this interesting plant, and he states that the flowers in his specimens do not so well accord with Dr. Klotzsch's description as could be wished. He figures the abortive pistil which is frequently found in the staminate flowers; but as he does not notice some curious particulars observed by the writer of this article, it may here be briefly mentioned,* 1. The plant is *polygamo-diœcious;* or, at least, some of the flowers are not unfrequently perfect. 2. These perfect flowers are sometimes provided with three stamens, similar to those of the sterile flowers; but more commonly they present a single antheriferous stamen, sometimes accompanied by two short sterile filaments, and sometimes destitute of these rudiments: occasionally a fertile flower is furnished with three short rudimentary filaments. 3. In the perfect flowers, especially when only one stamen is antheriferous, the anther is commonly found to be *one-celled;* the other cell being entirely suppressed, or else reduced to a mere vestige, as was seen in a single instance. 4. The lobes of the style are variable in number, and are of-

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* Since this article was in type, these characters have also been pointed out to the writer by Mr. Oakes, who furnished the specimens he examined.

ten two-toothed or cleft. 5. The floral envelopes, or rather scales, are not distinguishable into two well-defined series, (calyx and corolla,) and their number is very variable; the innermost series (*corolla* of Klotzsch and Tuckerman) not unfrequently consisting of three scales, and the others of 5 to 10 successively imbricated scales. A. Gr.

3. *Enumeratio methodica Caricum quarundam: species recensuit et secundum habitum pro viribus disponere tentavit* Edvardus Tuckerman, Jun. LL.B. *etc. etc.* Schenectadiæ: 1843. (pp. 21, 8vo.)—This pamphlet we believe is not formally published, but was printed for private distribution among the author's botanical friends. It is the result of an attempt—in most respects very successful—to effect a natural distribution of the species of the vast and difficult genus Carex. Mr. Tuckerman adopts the following primary sections.

1. Psyllophoreæ, (Loisel.) Spica unica simplicissima androgyna, s. dioica. Stigmata 2–3.

2. Vigneæ, (Koch.) Spiculæ plures sessiles androgynæ, in spicam continuam, s. interruptam, s. paniculatam dispositæ. Stigmata 2.

3. Vigneastra, (mihi.) Spicis compositis ramosis ramisque semper androgynis, apice masculis 2–3-stigmaticis. *Wahl.*

4. Leptantheræ, (mihi.) Vigneas inter et Carices. Stigmata 2, rarissimè 3.

5. Legitimæ, (Koch.) Spicis simplicibus sexu distinctis, rariùs pseudandrogynis. Spica terminalis ferè semper mascula, nunc plures. *Wahl.*, *Koch.*

These sections are mostly divided into subsections, and the latter into minor groups, amounting to fifty-one in number, which (except those of the first section) are not furnished with diagnoses,—and really it would prove no easy task to characterize them,—but are distinguished by the names of their leading species. Many interesting critical remarks are interspersed among, and follow the systematic portion; and four new species are indicated, viz. *C. alopecoidea*, (= C. cephalophora, var. maxima, *Dew.*,) *C. neglecta*, (aff. C. trispermæ and C. Deweyanæ,) *C. Monile*, (= C. bullata *β.?* *Torr.* and *Gr.*) and *C. Torreyi*, (= C. pallescens?) The proposed arrangement of the *C. straminea* group is perhaps the best that can be done with it; except that *C. arida* will renew its claim to specific distinction. We cannot distinguish *C. Meadii*, *Dew.* from *C. panicea.* In conclusion, we must be allowed to express our strong dislike of the attempt to change, in one or two cases, long established specific names, because they conflict with the Linnean canons and other excellent rules. "*C. scirpina*" may be a better name than *C. scirpoidea*, *Michx*, (although nomina *Barbaro-Latina* are as expressly forbidden by the canon as *Barbaro-Græca*,) and

"*C. vulpinæformis*" is greatly preferable to *C. vulpinoidea, Michx.;* but to discard the received names, as here proposed, on the strength of the axiom § 223 *Phil. Bot.*, besides the danger of the precedent, is really to be "*plus saint que le Pape;*" for Linnæus himself ever adopted all the nomina Barbaro-Latina there cited as examples, viz. *Tamarindus* and *Morinda*, as well as one of the three N. Græco-Latina, viz. *Sapindus.* The practice of the great botanist affords the safest construction of the rule. The genus Carex, with all its difficulties, has long been an especial favorite with the botanists of this country; who will hail with pleasure so zealous a laborer in this yet unexhausted field as Mr. Tuckerman. A. Gr.

4. *Flora Brasiliensis, sive Enumeratio Plantarum in Brasilia hactenus detectarum quas cura Musei Cæs. Reg. Palat. Vindobonensis suis aliorumque Botanicorum studiis descriptas, et methodo naturali digestas sub auspiciis Ferdinandi I, Austriæ Imperatoris et Ludovici I, Bavariæ Regis ediderunt* Stephanus Endlicher et Carol. Frid. Phil. De Martius. (Vienna and Leipsic, royal fol.) Fasc. I, 1840. Fasc. II, Jan. 1841. Fasc. III.–V, Apr. 1842.—This work, although principally based upon the rich collections of Martius, &c. at Munich, and of Pohl at Vienna, is intended to embrace all the known Brazilian plants; which at the commencement of the undertaking were estimated at 15,000 species. It will extend, it is thought, to a dozen large volumes, and will probably require as many years for its completion; although the enterprising authors have secured the aid of many celebrated botanists, such as *Nees von Esenbeck*, *Bentham*, *Lindley*, *Schlechtendal*, *Unger*, *Zuccarini*, *Spring*, *Rœper*, *Pœppig*, *Meisner*, *Kunze*, *Grisebach*, *Hornschuch*, *Decaisne*, &c. for the elaboration of particular orders. The first fasciculus comprises the *Musci*, which are elaborated by Prof. Hornschuch, and the *Lycopodiaceæ* by Prof. Spring; and is illustrated by eight plates. The second contains the *Anonaceæ*, by Martius, (with 14 plates,) who adds an interesting historical account of the species cultivated for their edible fruit. In the third, fourth and fifth fasciculi, the learned Nees von Esenbeck has given a monograph of the *Cyperaceæ* of Brazil, illustrated by thirty plates from admirable drawings by Putterlich of Vienna. All the plates of the systematic part of the work, we should remark, are *engraved* upon stone in the same excellent style as those of *Martius, Nov. Gen. et Spec. Brasil.; Siebold & Zuccarini, Flora Japonica*, etc. Several new genera are established, one of the most interesting of which is *Hoppia*, nearly allied to Carex. The genera of the tribe *Sclerieæ* are greatly, not to say unduly multiplied; and, in a note, even our *Scleria triglomerata* is separated as the type of a new genus, *Trachylomia;* which name however

the anterior *Trachyloma* of Bridel renders inadmissible. In the *Rhynchosporeæ*, we are pleased to find that Nees has adopted the genus *Psilocarya* of Torrey; to which he has added eight species. The *Ptilochæta*, *N. ab E.*, nearly approaches *Eriochæte*, (sub Rhynchospora,) *Gray*, *in Torr. mon. Cyp.* A good figure and full analysis is given of *Scirpus* (*Isolepis*) *subsquarrosus*, Muhl., under the name of *Hemicarpha subsquarrosa*, from which a second species from St. Louis, *H. Drummondii*, *N. ab E.*, (in a note,) does not appear to differ; and the genus is at length referred to the *Hypolytreæ*.

The fifth fasciculus of this Flora also contains the *Smilaceæ* and *Dioscoreæ*, by Prof. Grisebach, with six plates. Each fasciculus comprises a portion of a very interesting and graphic introductory chapter by Prof. Martius, upon Brazilian vegetation generally, with illustrations of some of its more remarkable features and peculiarities in different regions. This is illustrated by a series of spirited *Tabulæ physiognomicæ*, in tinted lithography, eighteen of which are already published. The whole Flora will form a series of monographs, prepared by some of the ablest botanists in Europe, (each with its letter-press and plates independently numbered,) which, if we mistake not, may be separately purchased. The subscription price is very moderate, viz. from thirty to thirty-three florins for each volume of 40–50 leaves and as many (uncolored) plates. A. Gr.

5. *Binomial Theorem and Logarithms;* for the use of the Midshipmen at the Naval School, Philadelphia. Perkins & Purves, 1843.—This is the title of a work recently published, from the pen of Professor Chauvenet, of the United States Naval School, Philadelphia. The author modestly remarks in his preface, that his original design was to use the work in manuscript, but he found it necessary to enlarge it so much as to render its use in that form impracticable. He says, " in preparing it for the press, the original design has been still farther extended, and the work now assumes the form of a distinct, if not a complete, treatise upon the binomial theorem and logarithms."

We have read the work somewhat carefully; although it has been prepared especially for the use of the midshipmen, we consider it worthy of general circulation. Such a work has long been a desideratum among elementary mathematical treatises. The subjects upon which it treats, although intimately connected, and in some measure dependent upon each other, have hitherto been treated of disconnectedly, and could only be studied properly by reference to different works. Here we have a complete and thorough treatise upon the subjects on which the author writes. He has introduced into it every thing *necessary* to make it so; at the same time he has omitted every thing not absolutely essential to that purpose.

Its chief excellence consists in the clearness and precision of the steps by which he advances. He combines clearness with sufficient length, so that his steps are abundantly evident, at the same time that there is difficulty enough to render them interesting to the more intelligent student.

He does not pretend to originality, but his style and method are peculiar to himself, and his demonstrations are either altogether new, or happy modifications of those of other writers. With regard to the binomial theorem he says in his preface, " a rigid demonstration of it, at once simple and elementary, has been much sought for by mathematicians. The one here given depends upon a principle which is the foundation of the differential calculus, and is in fact little else than a *translation*, of the very simple demonstration afforded by that science, into the elementary language of algebra." We have examined the demonstration with some care. The principle is similar to that used by Bourdon in his later editions, (we have not seen his earlier,) and by other algebraists. Prof. Davies, in his translation of Bourdon, gives Euler's method, which though ingenious seems by no means so clear or so elegant, and certainly more abstruse and less direct. Euler first deduces the binomial formula in the case when the exponent is a positive integer, and then proves that the same formula exposes the expansion of binomials affected with negative and fractional exponents.

Professor Chauvenet has made the demonstration at once elegant and direct, by first proving the fundamental principle somewhat in the form of a lemma. This principle is found in the peculiar nature of the quotient of $\frac{x^m - y^m}{x - y}$, whatever be the nature of the exponent m. He first beautifully shows in a few lines that this quotient is always exact, and the series limited, when m is positive and integral; and then in an equally striking manner demonstrates that when $x = y$, although the quotient then becomes $\frac{0}{0}$, (the expression for an infinitesimal quantity,) that still it is for all values, if the exponent is equal to mx^{m-1}, the well known form of the differential co-efficient of x^m. When these principles are established, the demonstration becomes at once direct and clear, and as elementary as the student can desire. He has interspersed through the work numerous and appropriate examples; into this chapter in particular he has introduced a beautiful collection, many of them original, others from French works, which illustrate fully the application of the binomial theorem.

The chapter on the " nature and use of logarithms" is extremely happy, well calculated to interest the student and to place the subject before him in an entirely new light. We would call the attention of teachers of mathematics particularly to it. The mode of deriving the logarithmic formula, is that of Euler, but so modified and improved as scarcely to be recognized. The method of finding "the number corresponding to a given logarithm," is different from any we have before seen. The method usually employed is by "the reversion of series," as it is called, a method which is tedious and liable to the great objection, that it does not reveal the *law of the resulting series*, by which any succeeding term may be inferred or deduced from the preceding terms, however numerous. That law has been invented and applied by the author.

In short, we consider the work to be a valuable one, and one from which almost any mathematician may derive advantage.

6. *Transactions of the Association of American Geologists and Naturalists*, 1840–1842. Boston: Gould, Kendall & Lincoln. Royal 8vo, pp. 544, with 21 plates.—This volume embraces the reports of the doings of the three first years of the Association of American Geologists and Naturalists, at their meetings held at Philadelphia in 1840 and '41, and in Boston in 1842. Several of the papers it contains, and all the *proceedings* of the sessions, have already been before our readers in the pages of this Journal; but the great bulk of the volume appears now for the first time, and embraces all the papers read before the Association at its three first meetings. We shall not attempt any notice of its contents, but can assure our readers that the volume is every way creditable to American science, and must be considered as an essential companion to all who would keep up with the rapid progress of American geology and the cognate sciences; while it gives to the body from which it emanates a character which at once places it among the permanent agents of scientific progress.

MISCELLANIES.

DOMESTIC AND FOREIGN.

1. *Notice of certain siliceous tubes (Fulgurites) formed in the earth;* in a letter from Charles E. West, to the Editors, dated Rutgers Female Institute, New York, March 21, 1843.—A remarkable natural phenomenon was observed a few years since in the town of Rome, state of New York. I was particular at the time, to gather what information I could respecting it, which is now submitted to the readers of your valuable Journal.

A lambent flame was seen playing at night upon the surface of a sand bank, some seventy or eighty feet high, which forms the east bank of the ancient channel of what is called Fish Creek. This excited the curiosity of the neighborhood and led to an examination of the spot. After removing some twelve or eighteen inches of the soil, they discovered an irregular tube of very coarse glass, which had evidently been made from the sand of the bank. The sides of the tube were compressed, and very irregular. Its longest diameter was about half an inch. Its interior was highly glazed, while its exterior was rough, being covered with particles of sand. When they had exposed about fifteen feet of the tube, they found it necessary to sink a shaft of logs to prevent the caving in of the bank. They continued to dig thirty feet deeper, when it was discovered that the tube, which had maintained an almost vertical position, made a sudden inclination and passed deeper into the bank. The fear of inhumation now compelled them to relinquish all further effort in tracing its course. They, however, dug five feet more in a vertical line and came to water; making in all rising of fifty feet from the surface. The tube was single for some distance from the top, where it made two bifurcations. Some eighteen inches below the surface were found thin strata of indurated sand, which were easily broken by the shovel; they were highly inclined, and their surface was undulating. Some of them were separated from each other one or two inches, others three or four inches. These interstices were filled with sand, which by digging, had shaken out in some instances and left the strata like the leaves of an open book; they were glazed, but not so highly as was the interior of the tube.

From this narration of facts, two questions naturally suggest themselves. 1st. In what manner was this tube formed? and 2d. What was the source of the light?

Without attempting to offer satisfactory replies to these questions, I would remark in relation to the first, that sand tubes of a few feet in length have been frequently described, but none of them, so far as I know, equals in interest the one referred to. None had its great length. To account for their formation, several theories have been proposed. One, that carbonate of lime held in solution had been gradually deposited around vegetable stalks, which finally wasted away, leaving these peculiar tubes. Another, that they are the work of insects. The third and most popular theory is, that they are produced by *lightning*.* It has been suggested, that whenever the electric fluid in its passage into the earth meets with the essential ingredients of glass, it fuses them into these singular tubes, provided the current be of sufficient intensity. It appears to me that neither

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* Hence the name Fulgurites, by which they are usually known.

of these causes is adequate to produce a tube *fifty* feet in length, such as we have described. That the first two had any agency in the matter, we cannot admit for a moment; for the tube gives evidence of igneous action, and consists of silex instead of lime. With respect to the third, let us inquire, if from the diffusive tendency of electricity to divide itself into a thousand ramifications on coming in contact with moist bodies, it is probable that the fluid would pass for fifty feet or more in a continuous line through moist sand? It strikes us as highly improbable. Again, if lightning is the cause, why did it not produce a *solid mass*, instead of a tube? And yet, if we set aside these objections, the tube appears as though it were formed in this way. The smooth and highly glazed surface of the interior, admitting atmospheric electricity to be the agent, might be accounted for from the fact of its being nearer the central action of the fluid, and also from the fact that there would be no particles of unmelted sand within the tube to mar its surface, while the exterior in its liquid and afterward pasty state, coming in contact with particles of sand, would be pierced by them and made rough. It would be natural to suppose that a tube produced in this manner would collapse, presenting a flattened appearance.

With regard to the second question, it is now impossible to tell what the gas was which produced the light, because it has disappeared since the destruction of the tube. It may have been phosphuretted hydrogen, derived from the decomposition of animal bones deposited ages ago beneath that sand bank, or it may have been pure hydrogen, resulting from the changes which native protosulphuret of iron undergoes when exposed to moisture; for it is well known in the spontaneous decomposition of water by this mineral when thus exposed, that it absorbs the oxygen of the water, forming a protosulphate of iron, and eliminates heat sufficient to inflame the hydrogen; or it may have been sulphuretted hydrogen derived from the decomposition of iron pyrites, the bisulphuret of iron, which is often associated with organic remains, which would also afford phosphuretted hydrogen, thus yielding a mixture of these gases, one of which burns spontaneously at ordinary temperatures.

The writer has made these gratuitous comments, not with the intention of satisfactorily accounting for these phenomena, but for the sake of awakening enquiry among your readers upon this interesting subject.

2. *Supplementary notice of the Ceraurus crosotus;* in a letter from Prof. John Locke, M. D., to the Editors, dated Cincinnati, Feb. 24, 1843.—Below are some figures of parts of the crustacean which I have denominated the *Ceraurus crosotus*, described and figured in a

previous letter. (See this Journal, Vol. xliv, p. 346.) It is very rare that we meet with this fossil entire; my own specimen, which is somewhat mutilated in its smaller appendages, is the only one known to me. To the practical geologist it will be a matter of interest to be informed what fragments are of most frequent occurrence. The subjoined figures represent such as are most abundant in our rocks.

Fig. 2.

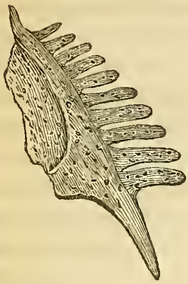

Fig. 3.

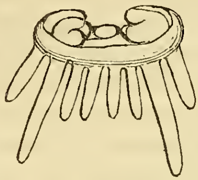

Fig. 4.

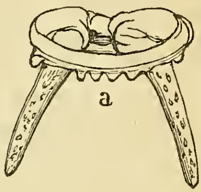

Fig. 2 is an accurate drawing of a specimen in my own cabinet, magnified six times in linear dimensions. By referring as above to fig. 1, it will be seen that it is the cheek or lateral portion of the shield. This is by far the most common fragment, and it is fortunately very well characterized by its pectinate form. Fig. 3, from a fragment in my own possession, magnified to the same scale, represents the tail or termination of the animal. The two longer processes are continuations of the last costal arches, while the four intermediate smaller appendages, and the two exterior ones, of similar size, are attached merely to the margin of the crustaceous covering, and are similar to the fringe of the cheek in Fig. 2. Fig. 4, from a specimen in Mr. Carley's cabinet, is evidently the same as fig. 3, but with the lesser processes broken off, as at *a*. Before other parts had been examined, this last had deceived one of our best naturalists, who mistook it for the *anterior* instead of the posterior termination of a crustacean. Since I communicated to you my account of the entire fossil, (Vol. xliv, p. 346,) I have discovered that the best specimens are covered with elegant tubercles, showing in this respect a close analogy to the Ceraurus pleurexanthemus of Dr. Green. A fragment is not unfrequently found, which if it belongs to this species, would indicate a central process from the posterior margin of the shield, running down over the middle of the body, like a Chinese cue of hair. My best specimen, already referred to, is broken at this point, and does not settle the question with regard to such a process.

Contemporaneous fossils.—Strophomena alternata, S. semiovalis, numerous crinoidean joints, Orthis testudinaria, Cryptolithus tesselatus, Calymene senaria, Isotelus megistos, and numerous branched corallines, are associated immediately with the Ceraurus. The Isotelus gigas is found below it, and the Cryptolithus terminates perhaps sixty feet above it. The particular locality at which the best specimens are found is about sixty feet in altitude below the reservoir at Cincinnati, and about one hundred feet above low water of the Ohio.

3. *Cambridge Observatory.*—The deficiency of instruments at this observatory is about to be supplied upon a scale of munificence, worthy of the princely liberality of the Boston merchants. David Sears of Boston has given five thousand dollars for the erection of an observatory tower, which will be furnished with instruments from a contribution of twenty thousand dollars, which was subscribed within sixty days from the date of the Sears donation, and to which he himself gave an additional sum of five hundred dollars. The ready patronage, which has, upon this occasion, been so generously extended to American astronomy, is most honorable to the republic, and no country can point to a larger donation to science, in proportion to its wealth. The other donors are Peter C. Brooks, who gave one thousand dollars; Samuel Appleton, William Appleton, John P. Cushing of Watertown, Joseph Peabody of Salem, Thomas H. Perkins, Jonathan Phillips, Robert G. Shaw, and George C. Shattuck, each of whom gave five hundred dollars; Nathan Appleton, Abbot Lawrence, Amos Lawrence, Israel Munson, Theodore Lyman, Nathaniel West of Salem, D. L. Pickman of Salem, George Howland of New Bedford, Gideon Howland of New Bedford, John A. Parker of New Bedford, William Rotch, jr. of New Bedford, James Arnold of New Bedford, N. W. Neal of Salem, John Parker, William Pratt, John Wells, Ezra Weston, J. W. Ward, Josiah Quincy, Samuel Falls, Francis Parkman, Martin Brimmer, Thomas Lee, Francis C. Gray, Horace Gray, Henry Oxnard, William Lawrence, N. J. Bowditch, George W. Lyman, Charles Lyman, George F. Parkman, Thomas B. Wales, Daniel P. Parker, John L. Gardner, George Ballett, Edmund Dwight, William Sturgis, Nathaniel Silsbee of Salem, John C. Gray, Ozias Goodwin, James Davis, jr., Dr. John Codman, John Quincy Adams, Dr. Wm. J. Walker, Charles G. Coffin of Nantucket, Jared Coffin of Nantucket, J. W. Barrett of Nantucket, G. R. Upton of Nantucket, Dwight Boyden, Henry Plympton, F. Tudor, H. Codman, Samuel C. Gray, William Amory, J. Ingersoll Bowditch, Thomas B. Curtis, Bates & Co., Joseph Grinnell of New Bedford, J. J. Dixwell, James S. Amory, Samuel T. Armstrong, J. Chickering, Dr. John Ware, John M. Forbes, George H. Kuhn, Joseph Whitney, Andrew E. Belknap, S. Austin, jr., F. Bassett, Richard D. Harris, and Thomas Wetmore, of whom the first eighteen gave two hundred dollars each, the following thirty, one hundred dollars each, and the remainder smaller

sums, principally of fifty dollars. In addition to these individual contributions, the Society for the Diffusion of Useful Knowledge gave one thousand dollars; and this lead to the societies was almost simultaneously given by the American Academy of Arts and Sciences with the still larger donation of three thousand dollars; the other societies which contributed, are the American, Merchants and National Insurance Companies, and Humane Society, each of which gave five hundred dollars; the Neptune and Washington Insurance Companies, each of which gave three hundred dollars; the Equitable Safety Insurance, which gave two hundred and fifty dollars; and the Tremont Insurance Company, which gave two hundred dollars.

The location of the present observatory is very bad on many accounts; so that about a year since, the Corporation of Harvard University had wisely profited by an advantageous opportunity to purchase the best possible site in its vicinity for astronomical purposes. The position is elevated, and commands in every direction a clear horizon, without any danger of molestation from trees, houses, smoke, or other causes, and with hills well situated for the erection of meridian and prime vertical marks. Upon this, which is known as Summer House Hill, the Sears Tower will be erected, with the other buildings for magnetic, meteorological and astronomical observations, and the house for the observer. The funds invested in the observatory, when it is completed, will amount to thirty-five or forty thousand dollars; consisting, besides the above twenty-five thousand dollars, in the house and lands given by the College, the extensive magnetic apparatus given by the American Academy, a telescope for occultations and eclipses from Francis Peabody of Salem, Mr. Bond's astronomical clock, transit telescope, telescope for occultations, and his other instruments; and lastly, the never-to-be-forgotten little comet seeker belonging to President Quincy, with which Mr. Bond first detected the head of the recent comet, and was enabled to make his observation of the 9th of March, and to which instrument we are largely indebted for the contribution of these funds. The new instruments which will probably be purchased if the funds should prove to be sufficient, are, an equatorial telescope of the largest class, being of the same dimensions with the celebrated Pulkova telescope; a transit circle; a small equatorial of six feet focal length; a comet seeker of the largest size; and a zenith sector. With these instruments, the observatory will be as well endowed as any in the world, for the class of observations to which it will be principally devoted.

4. *Notice of Botanical Collections.*—We take much pleasure in announcing that three enterprising botanists are now engaged in exploring the most interesting portions of the *far West*, and that their collections

of dried plants will be offered to subscribers, in sets, as they come to hand. Two of these collectors, Mr. *Charles A. Geyer*, (well known as the botanist of Mr. Nicollet's official northwestern expedition,) and Mr. *Lüders*, who are for the present attached to Sir. Wm. Stewart's party, have by this time reached the Rocky Mountains. The particular field of Mr. Geyer's operations, and the extent of his journey, were undecided at the time of his departure from St. Louis. Mr. Lüders expects to spend the next winter, and perhaps the ensuing summer, at a station of some Roman Catholic missionaries on the upper waters of Lewis and Clarke's, or Great Snake River. These botanists being well acquainted with the vegetation of the general Valley of the Mississippi and of the lower Missouri, will doubtless avoid the common and better known plants of this region; and thus their collections may be expected to prove unusually choice and valuable.

The third collector, Dr. *Lindheimer*, a very assiduous botanist, intends to devote a few years to the exploration of Texas; and he pledges himself to exclude from his sets all the common plants of the southwestern United States.

These several collections will be assorted and distributed, and for the most part ticketed, by Dr. Engelmann of St. Louis; assisted, as far as need be, by the authors of the *Flora of North America*, who promise to determine the plants, so far at least as they belong to families published in that work; and for the information of subscribers, particular notices of the *centuria* offered for sale, will probably appear in this Journal, as they come to hand. The number of sets being limited, earlier subscribers will receive a preference. The three explorers are entirely independent of each other; and their collections are to be separately subscribed for.

The price of the Rocky Mountain collections of Geyer or of Lüders, is fixed at ten dollars (or two guineas) per hundred; that of Dr. Lindheimer's Texan collections at eight dollars (or £1, 13s. 6d. sterling) per hundred—payable on delivery of the sets at St. Louis, Missouri, by Dr. George Engelmann; at New York by Wiley & Putnam, 161, Broadway, and Stationers' Hall Court, London; and Prof. A. Gray, of Harvard University, Cambridge, Massachusetts, to either of whom subscribers may address themselves (*post paid*) by mail. The additional expense of transportation, doubtless trifling in amount, will be charged upon the sets deliverable in London.

The writer of this notice cheerfully states that the dried specimens made by these botanists which have fallen under his observation, are well selected, very complete, and finely prepared; and he cordially joins Dr. Engelmann in recommending the enterprise to the patronage of botanists.

For the purpose of obtaining some immediate pecuniary aid in the prosecution of his present arduous undertaking, Mr. Geyer also offers for sale, (through the parties above mentioned,) a selection from his collections of the last year in Illinois and Missouri; consisting of twenty sets of one hundred and fifty species of plants, which are offered at six dollars per set. A list of this collection, with critical remarks, and descriptions of some new species it contains, received from Dr. Engelmann too late for present insertion, will find a place in the ensuing number of this Journal. A. Gr.

5. *Iodine in Phanerogamic Plants and Mosses.*—At a meeting of the Botanical Society of Edinburgh, on the 7th of December last, "Mr. Brand read 'a notice of the presence of iodine in some plants growing near the sea,' by G. Dickie. The author found, by chemical examination of specimens of *Statice Armeria* from the sea-shore, and of others from the inland and higher districts of Aberdeenshire, that the former contained iodine, and that soda was more abundant in them, while potassa prevailed in the latter. Iodine was also found in *Grimmia maritima;* and Mr. P. Grant of Aberdeen has also found it in *Pyrethrum maritimum.* An analysis was made of specimens of *Statice Armeria*, *Grimmia maritima*, *Lichina confinis*, and *Ramalina scopulorum*, all growing near the same spot, and occasionally during storms exposed to the sea spray: all these plants, with the exception of the Lichen, contained iodine. The specimens having been washed previously to analysis, the iodine could not have been derived from saline incrustation. All these vegetables were healthy, and the author of the paper has been led to conclude that the marine Algæ are not the only plants which possess the power of separating from sea water the compounds of iodine, and of condensing them in their tissues, and this without any detriment to their healthy functions."—*Gardener's Chronicle.*

6. *Disengagement of Carbonic Acid by the Roots of Plants.*—"It appears from the researches of Messrs. Wiegmann and Polsdorff as reported in the last number of the 'Annals of Chemistry,' that the roots of living plants disengage carbonic acid, and that this acid is capable of decomposing the silicates of the soil, which resist even the action of nitro-muriatic acid. This most curious discovery throws a new light upon the importance of carbonic acid to vegetation, and explains clearly, what has been by no means evident, namely the manner in which flinty substances prove beneficial to vegetation, and how minerals so hard as feldspar are made to contribute to the maintenance of plants. Plants of tobacco, oats, barley, clover, &c. were grown in quartz sand which had been heated red hot, and then digested for sixteen hours in

dilute nitro-muriatic acid. One would have thought that, after such treatment, the quartz could have contained nothing capable of sustaining vegetable life; nevertheless the plants grew in it, and their ashes were found to contain potassa, lime, magnesia, and silicious earth, which had been obtained from the decomposition of the quartz sand by the decomposition of the roots."—*Gardener's Chronicle.* (We see no proof nor probability that the carbonic acid in such cases is *disengaged* from the roots.)

7. *Filariæ in the Blood of a living Dog.*—MM. Gruly and Delafond exhibited to the Academy of Sciences, at their session, Feb. 6th, numerous specimens of an Entozoon, allied to the Filariæ, obtained from the blood of an apparently healthy dog. Physiologists have been for a long time aware of the presence of Entozoa in the blood of reptiles and fishes, but this is the first instance in which they have been detected in the blood of a mammal. It is of great importance to physiology, pathology, and natural history, to prove not only their existence in the blood itself, but that they circulate with it in the higher animals.

The entozoa in question, have a length of 0.25 millimetre, and a diameter of 0.003 to 0.005 millimetre. Body transparent, colorless; anterior extremity obtuse, posterior terminated by a thin filament. Their motions are very active, swimming with an undulating movement among the globules. They were detected in the blood drawn from the coccygeal arteries, external jugular veins, capillaries of the conjunctiva, mucous membrane of the mouth, skin and muscles. The urine and excrements contained none. Their diameter is less than that of a blood globule, which will allow them to pass wherever the blood circulates.—*Comptes Rendus*, *Feb.* 6*th*, 1843.

8. *Experiments of Karsten*, *relative to the formation of the* "*images of Möser;*" extracted from letters of Humboldt to Arago.—"On placing a medal on a glass plate, and under the last a metallic plate, Karsten has ascertained that an image of the medal is formed upon the upper surface of glass, when an electrical spark is made to fall on the medal. If the medal rests on several plates of glass, and the last on metal, the spark produces images on all the plates, but only on their upper surfaces; the most feeble being the most distant from the medal. To render the images visible, they must be exposed to the vapor of iodine or mercury. The *spark* is necessary for the production of images. M. Karsten has not succeeded with the electricity of the pile." (Berlin, 10th March.)

"I have seen experiments of M. Karsten; the effect is instantaneous, and the figures very distinct. The electricity emanating with greater intensity from the prominent or convex part of the medal, changes the molecular state of the glass, in passing to its lower surface. The image is rendered visible, by the most gentle breath. The vapor is deposited in little drops on all the parts of which the molecular condition is changed, whilst it is deposited uniformly where no such change exists. (Berlin, 22d March.)—*Comptes Rendus*, *April* 3, 1843.

9. *Great Comet of* 1843.—This splendid comet, which was seen in the sunshine on the 28th day of February last by thousands of spectators in New England, and which for a month after adorned the evening sky with its long and brilliant train, has excited uncommon interest in all quarters of the globe. A letter from Mr. John Taylor, of Liverpool, to the Editors of this Journal, states that in the *Isle of France*, (S. lat. 20°,) the comet was seen in great splendor from the 28th of February to the 8th of March, (and doubtless later,)—the train resembling "a stream of fire from a furnace." At *Bombay*, (N. lat. 19°,) the train was discovered shortly after sunset March 4, as a long, straight beam of light streaming from the western horizon towards the zenith. The next night the nucleus, or at least the lower termination of the comet, became distinctly apparent. From this time onward, numerous observations were taken at that place, but with what precision remains to be seen. Similar accounts have been received from various places on both sides of the equator; yet we have no evidence that by any of these early observers (except Mr. Clarke of Portland) was the position of the nucleus accurately determined. This deficiency is matter of great regret, as it is obvious that good measures of the place of the nucleus taken within a week after the perihelion passage, would far outweigh in value those which were made during the latter part of the month of March.

It appears quite probable that the train of this comet was seen in the evening before the perihelion passage, at Bermuda, Philadelphia, and Porto Rico, on the 19th, 23d and 26th of February. Some of the observations on which this statement is founded, need however further investigation before they are given to the public.

In stating at p. 413 of the last volume of this Journal the distance of the nucleus of the comet from the sun on the 28th of February, as measured by Mr. F. G. Clarke, of Portland, Me., an error was committed, which is corrected in the following valuable memoranda, which have been kindly furnished me by that gentleman. The nucleus and also every part of the tail, as seen by him, in strong sunshine, were as well defined as the moon on a clear day. The nucleus and tail bore the same appearance, and resembled a perfectly pure white cloud, without any variation except a slight change near the head, just sufficient to distinguish the nucleus from the tail at that point. The denseness of the nucleus was so great that Mr. C. has no doubt that it might have been visible upon the sun's disk if it had passed between it and the observer. This dense appearance he considers due in part to the fact that the tail was foreshortened by projection, and so directed with reference to the earth, that the nucleus must have been seen through a considerable mass of the matter of the tail. Notwithstanding the difficulties resulting from the nearness of the comet to the sun shining in its strength, Mr. C. succeeded in obtaining with an instrument of reflection the following measurements, viz.

Feb. 28, 3h. 2m. 15s. P. M., Sun's farthest limb from nearest limb of nucleus, ... 4° 6′ 15″

Feb. 28, 3h. 6m. 20s. P. M., Sun's farthest limb from farthest limb of nucleus, ... 4° 7′ 30″

Feb. 28, 3h. 9m. 40s. P. M., Sun's farthest limb from extremity of tail, ... 5° 6′ 30″

The first of these measures Mr. C. considers reliable within 15″; and the other two may be taken as near approximations. Due allowance must of course be made for the motions of the two bodies during the period of observation. When the sun was in the plane of the meridian, the angle made by the line joining the centres of the sun and nucleus, with the lower vertical, on the eastern side, was about 73°. These data must evidently supersede those derived from the observations which were made at Waterbury, without the use of instruments.

E. C. H.

10. *Second Comet of* 1843.—M. Victor Mauvais, an astronomer attached to the Paris Observatory, discovered May 3, 1843, a telescopic comet on the limits of the constellations *Cygnus* and *Pegasus*. It is a feeble nebulosity, of an oval shape, and about 3′ diameter, with a sensible condensation of light towards the centre. It was seen by Sir J. South at Kensington, on the 10th of the same month.—*Lond. Ath. May* 13, 1843.

11. *Meteoric Observations*, *April* 20, 1843.—On the night of April 20, 1843, (the anniversary of the great meteoric shower of April, 1803,) I watched alone in the open air, at intervals during the entire night, which was one of uncommon sereneness. The number of meteors noted by me, did not exceed what I assume to be the average number, visible after midnight, at other seasons,—or from twelve to fifteen an hour for an individual observer.

The next night was likewise very clear, but I made no observation. Persons who were abroad to a late hour, informed me, that without giving any special attention, they remarked that shooting stars were unusually frequent. As however no reckoning was kept of the number actually seen, it might be unsafe to deduce any very positive inference from this information.

It was intended to watch for shooting stars on the morning of the 2d of January last, but on that morning as well as on the next, the sky at this place was overcast, and the intended observations were defeated. E. C. H.

12. *Hundredth Anniversary of the American Philosophical Society.*—This society celebrated its centennial meeting in Philadelphia on the 26th of May last and the four following days, closing on the evening of Thursday the 30th. An opening address was delivered by Dr. Robert Patterson, embracing a sketch of the origin and progress of the society. A large number of scientific laborers were assembled, and forty five papers were read on different departments of scientific research. The following is a list of the papers read.

Friday Morning, May 26th.

1. On Phosphorogenic Emanation, by Professor JOSEPH HENRY, of New Jersey College, Princeton.
2. On the Family Proboscidea, their general character and relations, their mode of dentition and geological distribution, by ISAAC HAYS, M. D.
3. On Analytical Trigonometry, by Professor THEODORE STRONG, of Rutgers College, Brunswick, N. J.
4. On Two Storms, which occurred in February, 1842, by Professor ELIAS LOOMIS, of Western Reserve College, Hudson, Ohio.

Friday Evening.

5. Historical sketch of Continental Paper Money. Part second. By SAMUEL BRECK, Esq.
6. On the Theory of Earthquakes, by Professor H. D. ROGERS, of the University of Pennsylvania.

Saturday Morning, May 27th.

7. History of the progress in establishing an Observatory at Washington City. Description of the building erecting, and of the instruments ordered for the Depot of Charts and Instruments of the U. S. Navy, by Lieut. J. M. GILLISS, U. S. N.
8. On the Influence of the Microscope upon the Science of Anatomy, by W. E. HORNER, M. D., Professor in the University of Pennsylvania.
9. On the Tides and Currents of the Atmosphere and Ocean, by WILLIAM C. REDFIELD, Esq. of New York.
10. On the Hourly and other Variations of the Magnetic Elements, of the Temperature and Pressure of the Air, and of the Force of Vapor, deduced from two years' observations at the Magnetic Observatory at the Girard College, by Professor A. D. BACHE, of the University of Pennsylvania.
11. Biographical Memoir of the Hon. Edward Livingston, by HENRY D. GILPIN, Esq.

Saturday Evening.

12. On the Launch of the Three-deck Ship, the Pennsylvania, by JOHN LENTHALL, Naval Constructor.
13. Method of Arranging the Spider's Lines in the Micrometer of a Transit Instrument, by JOHN LOCKE, M. D. of Cincinnati.
14. On the Tails of Comets, by Professor W. A. NORTON, of Delaware College, Newark.
15. On the Decomposition of Carbonic Acid by the Light of the Sun, by JOHN W. DRAPER, M. D., Professor in the University of New York City.
16. Letter from Chancellor Livingston to the Society of Arts of New York on Air Springs, Air Beds, &c. Letter from Count Rumford to Chancellor Livingston on Steam Carriages. Communicated by the Rev. Professor ALONZO POTTER, of Union College.

Monday Morning, May 29th.

17. Observations on the Comet of February, 1843, made at the Observatory of the Central High School, with Results of the Computations relative to its Orbit, by SEARS C. WALKER, Esq. and Professor KENDALL, of the Central High School.
18. Abstract of a Memoir on the Ethnography of the Ancient Egyptians, with specimens illustrative of its conclusions, by SAMUEL GEORGE MORTON, M. D.
19. The Social and Intellectual state of the Colony of Pennsylvania, previous to the year 1743, by JOB R. TYSON, Esq.
20. Measurements of the Fœtal Cranium, by CHARLES D. MEIGS, M. D., Professor in the Jefferson Medical College.
21. On Hippuric Acid, by Professor JAMES C. BOOTH and MARTIN BOYE, Esq.
22. On a new method of determining the Velocity of a Projectile, by Professor JOSEPH HENRY, of New Jersey College, Princeton.
23. Description of a new Thermometer of Contact, by Professor A. D. BACHE, of the University of Pennsylvania.
24. Descriptions of Water Spouts, by Captain LAVENDER, of Princeton, N. J., and by R. C. TAYLOR, Esq.

Monday Evening.

25. Transformation of the Series $S=ax+bx^2+cx^3$, &c., by Professor THEODORE STRONG, of Rutgers College, Brunswick, N. J.
26. An account of a Geographical Exploration of the Sources of the Mississippi, by J. N. NICOLLET, Esq.
27. On the Physical Phenomena attending Solar Eclipses, by Professor STEPHEN ALEXANDER, of New Jersey College, Princeton.

Tuesday Morning, May 30th.

28. On the Instruments of the Astronomical Observatory of the United States Military Academy, West Point, and the Observations made upon the Comet of February, 1843, by Professor WM. H. C. BARTLETT, of the United States Military Academy.
29. An outline of the Physical Geography and Geology of Maryland, in reference to its Agricultural Condition and Resources, by JULIUS T. DUCATEL, M. D. of Baltimore.
30. On the Geology of a Portion of the Island of Cuba, by R. C. TAYLOR, Esq.
31. On Coprolites, by ISAAC LEA, Esq.
32. On the effects of Gloomy Seclusion upon the Health of Individuals of the African Race, by B. H. COATES, M. D.
33. Description of some new Fossil Shells from the Tertiary of Virginia, by HENRY C. LEA, Esq.
34. Notice of the Meteorological Observations now making at the Military Posts of the United States, by T. G. MOWER, M. D., Surgeon U. S. A.
35. On the law of Cooling of Atmospheric Air for various suddenly diminished pressures, by JAMES P. ESPY, Esq.
36. On a new form of Mountain or other Barometer, by J. H. ALEXANDER, Esq. of Baltimore.

Tuesday Evening.

37. On some Fossil Ferns, of the Family of Sigillaria, in the Coal Strata of Pennsylvania, by R. C. TAYLOR, Esq.
38. Comparison of the Dimensions of the Earth, obtained from Measurements made in the Survey of the State of Massachusetts, with Accredited Mean Determinations, by SIMEON BORDEN, Esq. and R. T. PAINE, Esq. of Boston.
39. On Impressions produced upon Sensitive Plates in the Dark, by P. B. GODDARD, M. D. and JOSEPH SAXTON, Esq.
40. Comparison of Magnetic Observations at Philadelphia, and Toronto, U. C., on the occasion of an unusual disturbance of the magnetic elements, May 6th, 1843, by Professor A. D. BACHE, of the University of Pennsylvania.
41. Two Inedited Letters of Franklin upon Subjects of Science, communicated by GEO. BANCROFT, Esq. of Boston.
42. On the Theory of the Turbine Water Wheel, and the results of its practical application, by ELLWOOD MORRIS, C. E.

*43. On a Theory of Combinatorial Aggregation, by Professor SYLVESTER, F. R. S., late of University College, London.

44. Geological Notices, by Professor HENRY D. ROGERS, State Geologist.
45. On the Structure of the Fœtal Heart, in reference to Cyanosis, by CHARLES D. MEIGS, M. D.

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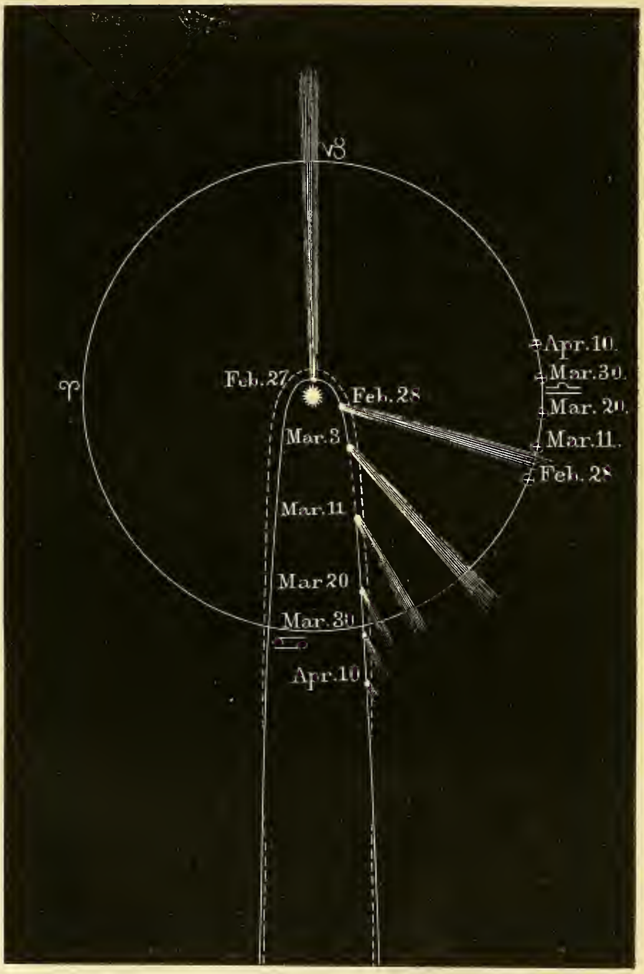
* Not read.



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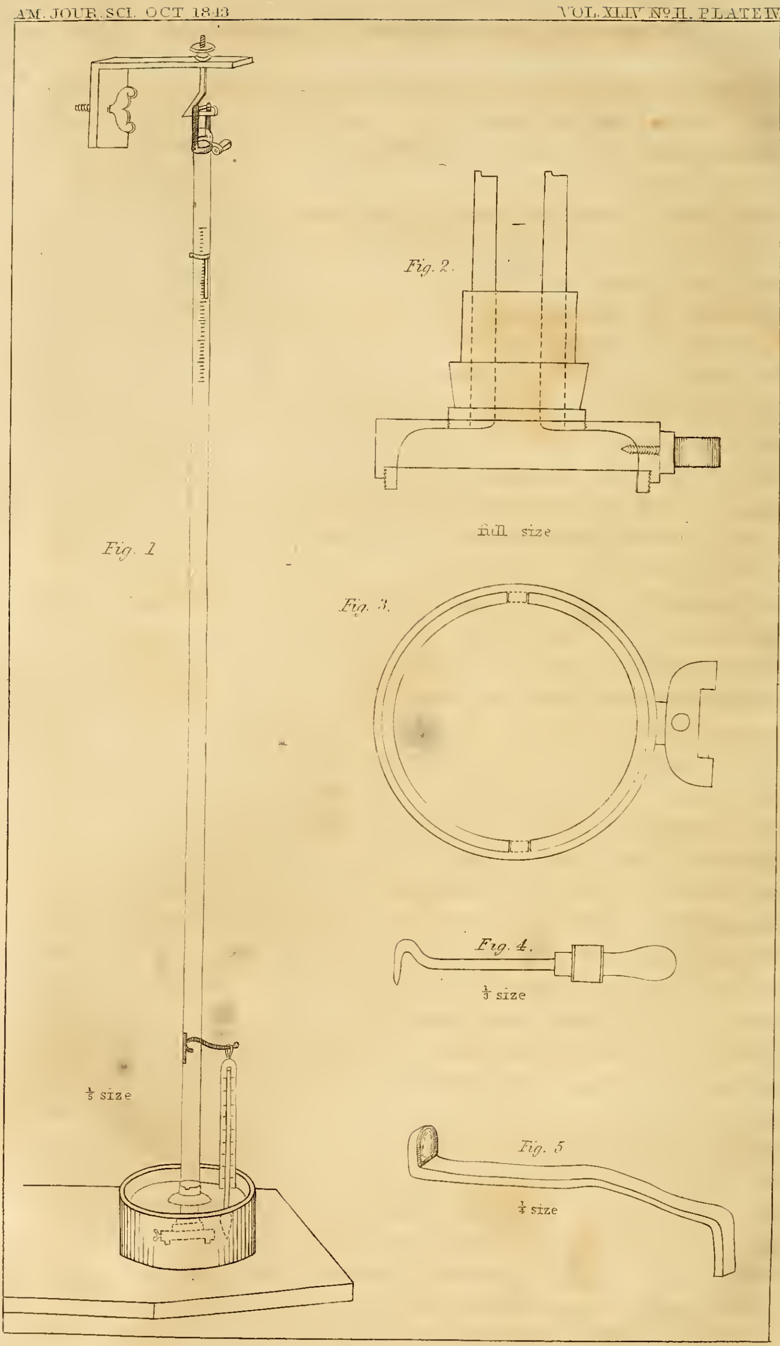

PROF. ALEXANDER'S BAROMETER

THE

AMERICAN

JOURNAL OF SCIENCE, &c.

Art. I.—*On a New Form of Mountain or other Barometer;* by J. H. Alexander, Esq.—(with a plate.)

The modifications of shape and details, which different ingenious individuals have proposed or executed upon barometers, are now so numerous, that, if on the one hand one might be supposed dispensed from adding to the variety, equally on the other the want of full acceptation, with any, serves to shew the object yet unattained, and the whole subject therefore open still for reflection and effort. I question much if, generally, in the judgments of those who have had more especial occasion for making observations with mountain-barometers, all the modern complications of structure, introduced with the best intentions, are not found to have contributed to the embarrassment of the observations if not of the result; by rendering necessary a number of merely collateral operations and by hiding accidental defects, which are only least harmful when soonest found out.

Mr. Hassler seems to partake of an opinion like this, from the account which has been recently published* of the new portable barometer of his construction—an instrument characterized by all the originality and much of the appropriateness, which belong to all the works of this distinguished philosopher. I mention this instrument in particular, that I may not obtain credit for more novelty than really belongs to the arrangement which I propose. Touching the respective merits of either or of any other arrangement, it is not my purpose now to speak.

* Doc. 176, H. R. 2d session, 27th Congress.

observation of coincidences therefore to be much more precise and agreeable.

The reading is not nearer than to 0.01 of inches; which is quite refined enough for any purpose to which I may expect its application, and indeed I might say for every purpose, except the determination of some atmospheric constants, when all recognized corrections would have to find their place. Whenever in such case or any other, all those corrections are employed, it will be appropriate to read the measurement closer and closer. At present there seems to be no need.

For instance, in the climate of Baltimore, for at least half the year, a change of 1° Fahr. in the dew-point implies an elevation or depression of the barometer of more than 0.016: such a change is very common, in the same locality, within the hour—such a difference highly probable, at the same moment, between places not very remote even from one another. Yet in the multitude of barometers read to thousandths, who reads the hygrometer?

So, in the application of the barometer to other than strictly meteorological purposes, the correction of a zenith distance for refraction when the angle is as great as 85°, is upon 0.01 of an inch of mercury only 0.2 of a second—a quantity to be sure generally admitted in calculation, but not materially affecting the most of astronomical or geodetic results. In zenith distances not so great, more usual, and more reliable, the amount of correction is still less: an altitude of 45° varies for $\frac{1}{100}$ in the barometer less than two hundredths of a second, a quantity in all ordinary calculations to be safely neglected. Similar considerations apply to this instrument, when rendered portable for the determination of heights. If the thermometer and hygrometer remain the same, a variation of $\frac{1}{1000}$ in the barometric column would correspond to a difference of level of little more than 10 inches; a space far within what any barometric observation has yet pretended to answer for.

The correction, too, arising from the specific gravity of the mercury, a particular rarely registered with the measurements, encloses within no narrow margin the apparent accuracy of a reading to thousandths of inches. If we take 13.6 as a mean specific gravity, giving with a certain temperature and dryness a stand of 30 inches, a specific gravity of 13.601 would give an equivalent stand under the same circumstances of only 29.9978 inches.

I apprehend it would be but accidental, if two barometers, made even by the same person but at different periods, or from any cause containing mercury of different lots, agreed in the specific gravity as nearly as this.

Finally, not to make too long discussion of such a point, it is even rare to find two observers, in direct succession upon the same instrument, reading to a coincidence as close as the thousandth of an inch. I thought myself therefore justified in discarding a graduation which, as I before said, multiplies the refinement without increasing the certainty.

Nevertheless, in limiting the tube-graduation to tenths of inches, I have not shut out the means of subdivision to thousandths in a ready and unexceptionable manner; whenever such subdivision should be requisite. These means consist in the application of a reading microscope or micrometer, whose position on the tube is held and regulated by a spring and clamp. The zero is adjusted, first, to the now magnified image of the mercury surface: and then, by the motion of the screw, the space between said surface and the nearest division on the tube below, is measured in hundredths of inches by the comb of the micrometer, and thousandths on the head of the screw in a well-known manner.

6. A piece of watch-spring, about three inches in length, is bent round at one end, so as to embrace three quarters of the circumference of the tube: the other end, which will then project from the tube, has merely a light triangular notch made in its upper edge to catch the loop of string or wire, which suspends the thermometer. In order to prevent any unsteadiness or wriggling motion, which would be otherwise likely in so narrow a strip, a piece of an inch length, from the same spring, is fastened with a single rivet at right angles to the former, so as to be vertical or nearly so when the clasp is made; or, as was done in the present instance, a cruciform piece is cut out of a wide clock-spring.

7. Nothing more need be said of the float than that it is of ivory, worked as thin and light as is consistent with its safety; and that the distance, between its under surface and the parallel fiducial edge of the rectangular notch seen in it, must be exactly equal with the space between the zero mark on the tube and that other line next above the zero mark, which has been spoken of already in §4. It is obvious that in such case, when the image of said fiducial edge seen by reflection on the tube, coincides with the

appropriate mark, the under surface of the float and of course the surface of the mercury beneath it in the cistern must also coincide with the zero of the graduation.

I apprehend that this symmetrical position of the float around the tube, tends to make such a coincidence of the zero mark and the mercury-surface more exact than is likely to be attained in any other arrangement; which places the float at a greater distance, and entirely on one side of the tube. No single point of the mercury-surface, perhaps, is ever to be taken as precisely in the normal plane of the base to the atmospheric column equilibrating the barometric column: but this equilibrium is made by pressure of an infinite number of atmospheric columns whose bases are in different horizontal planes, or, what is the same, by the pressure of an aggregate column whose base is as irregular as the cistern surface. A float, then, whose horizontal base extends equally all round the axis of equilibrium, may be supposed to present the fairest average of these irregularities and a general resultant of these several pressures.

In any event, its exactitude may be taken as within any such methods as the estimation of the capacity of the cistern, an English manner of construction; or the ivory point used in some continental barometers; or the *minimum visibile*, the most used but I think the most objectionable of all.

8. The cistern is a porcelain or glass dish of suitable size; which, when occasionally cleaned, admits of the application of heat to drive off all moisture. Wood is not favorable for such a purpose; because it receives a very smooth surface only with difficulty. It is besides hygrometric: and all varnish which might be used to remedy this disqualification brings another quite as bad —in the action, which the resinous components of such varnish are apt to have on the mercury.

What has been so far described, contains all the parts necessary for establishing a stationary barometer. I suppose, of course, though I have not yet mentioned it, that the mercury is pure, that its specific gravity is ascertained, that the tube has been boiled, and that in immersing it in the cistern no substance has been placed in contact with the mercury, likely to act upon or soil it. As a suitable implement for this purpose, I have figured in fig. 5 a tool, which Mr. Green applies in such cases. It is of iron, covered with clean undyed leather: the spheroidal pad at one end takes the place of the finger; and the pressure, which from its shape can be exerted by the whole hand and wrist to keep it in place, will be, in several positions of the tube, of the highest convenience.

The additions, necessary for constituting a mountain barometer, are shewn in figs. 2 and 3; of which the first is a vertical section, and the other a ground plan seen from below, of the appliances.

In fig. 2, is seen surrounding the tube, shewn by dotted lines, a steel cylinder, cemented to the tube. The middle part of said cylinder is left more massive than either extremity, and is worked into an octagonal shape to allow its being firmly clamped in a vice. The lower extremity is cut into a screw thread, which fits the screw of the inverted, bottomless, iron disk that is to form the termination of the tube. This disk is made of a piece of a gun barrel. The lower edge of this is also screw-cut, and is fitted with the ring seen in fig. 3. The notches in that ring, which are also shewn, serve for catching a lever or handle by which the ring is screwed up. The single line in fig. 2, between the ring and the disk, is a section of the bottom of the sub-cistern; which bottom is a plate of Russia-iron, hammered so thin as to be flexible, and secured by the ring before mentioned.

One side of the disk is tapped with a screw-thread and furnished with a screw, as seen in both drawings, for opening or shutting off the communication of the atmosphere. As this is only opened, when the tube is immersed in the cistern; and as one's fingers should be carefully kept from the mercury, fig. 4 represents a tool which I use for unscrewing and screwing: the hooked end of it, as can be readily imagined, fitting in the hole of the handle seen in fig. 3. Inasmuch as a great deal of power cannot be exerted with such a tool, the handle is necessary in order to tighten up the screw more effectually after the withdrawal of the barometer from the cistern.

I should mention that all the permanent screws—those connecting the disk with the cylinder or collar, and the ring with the disk—are laid in a cement, which Mr. Green contrived, and which, fusible only at a very high heat, has the property of quickly hardening. An idea of the tightness thus given to all the joints may be formed, when I state that in an experimental arrangement of this kind, wherein the disk had been turned out of a piece of cast-iron, supposed to be uniform enough,—the tube being inverted and immersed in water of about 130°,—the expanded mercury, not able to escape by any of the joints, actually forced its way in two or three places through the metal itself; that is, through flaws in its texture, though they were so microscopic as not to be observable to the naked eye, except by the emission of the quicksilver. So comminuted was the stream, and so great the force of emission, that the metal ascended in graceful wreaths, like smoke, to a height not less than two feet.

Having now explained the different parts of the instrument, I shall describe the various steps which were taken in the composition of it.

First, upon the open end of the tube was cemented the collar; and they were then ground to remove any irregularities, and make them fit evenly. Then the tube was clamped in a vice; and the disk was screwed on in cement, as far as possible. Clean mercury was then sifted into the tube, until it filled a part of the disk. The whole was then boiled over a spirit-lamp by successive portions; the flame being finally brought about as near to the end of the tube, as is shewn in fig. 2. The whole was then replaced in the vice, the plate of Russia-iron laid in its place, and the ring screwed on in cement, as tightly as might be. The tube was then removed, the side-screw taken out, while the instrument was held in a position somewhat inclined, but not so far as by possibility to uncover the end of the tube by the mercury now contained in the disk; and warm mercury was sifted down through the screw-hole, until it overflowed. The overflowing was regulated by the pressure of the finger against the elastic bottom, the tube being held nearly horizontal; and the screw was inserted and turned tight. I should have said that due care was taken, before inserting the screw, to disentangle and expel any air that might have been taken in with the mercury; and the proof of success in that regard was afforded, in giving the instrument, held in proper position, some smart shocks in order to see if any air could be forced up the tube above the cylinder. None shewed itself; and I think because there was none there.

This mode of terminating the tube, I regard as one of the most important modifications which it has been my aim to describe; and the merit of its suggestion belongs to Mr. Green, the artist who constructed the various portions of the instrument. The end which we had in view, in various plans which were contrived and tried, was to provide some means of following the expansions and contractions of the mercury, arising from changes of temperature; so as to furnish the air no chance of getting in. This the elastic plate does very well; indeed I may say completely: the heat of the hand being enough, in a few minutes, to cause a convex appearance, and the pressure of the atmosphere, on its removal to a cold room, sufficient to make it concave.

It was of course interesting, to have the plate, and by consequence the disk, of as small a diameter as would be compatible with the other conditions requisite. After some experiments, therefore, to ascertain what elasticity was attainable by hammering out the Russia-iron, consistent with uniformity and strength, I calculated the following table, shewing the relations existing between the clear inside diameter of the disk and that of the tube, on the assumption that the range of temperature does not exceed 90° Fahr., and that the mercury is always pressed upon by the plate.

Inside tube-diameter. Inch.	Inside diameter of disk, or exposed plate diameter. Inches.
0.20	0.90
0.25	1.13
0.30	1.35
0.40	1.81
0.50	2.26
0.60	2.71
0.70	3.16
0.75	3.39

These proportions have been found to suit very well in practice in the instances where they have been applied.

I have before spoken of the advantage in precision, which the symmetrical disposition of the float is presumed to afford to the measurements. I give here the formula, for calculating the amount of correction which is to be applied to make up for the partial sinking of the float, and the consequent elevation of the mercury in the tube and cistern.

It is manifest, that the heavier the float, with the same diameter, the deeper it (and of course the zero of the graduation) will sink in the cistern; and also, that with a float of constant weight and diameter, the larger the cistern the less will be the vertical rise of the fluid in it, in consequence of the displacement by the float. If then we call

W, the weight of the float—for instance in grains;

w, the weight of a cubic inch of mercury at any given temperature;

Δ, the diameter of the cistern in inches;

D, the diameter of the float "

d, the external diameter of tube "

δ, the internal diameter " "

And π, the circumference of a circle whose diameter is unity; the sum of the correction will be

$$-\left(\frac{W}{w}\cdot\left(\frac{1}{\frac{\pi}{4}\cdot(D^2-d^2)}+\frac{1}{\frac{\pi}{4}\cdot(\Delta^2+\delta^2-D^2)}\right)\right);$$

reducing to a common denominator, we have

$$-\frac{W\cdot\overline{(\Delta^2+\delta^2-d^2)}}{\frac{\pi}{4}\cdot w\cdot(D^2-d^2)\cdot(\Delta^2+\delta^2-D^2)}.$$

If we neglect the change of specific gravity by a change of temperature, as we may in this case safely do, and take $w=3426.56$ grs. (which corresponds to a specific gravity for mercury of 13.5728 at 62° F., and very near 13.6 at the maximum density of water,) and give to π its numerical value of 3.14156, etc., the correction becomes in round numbers

$$-\frac{W\cdot\overline{(\Delta^2+\delta^2-d^2)}}{2691\cdot(D^2-d^2)\cdot(\Delta^2+\delta^2-D^2)}.$$

Such is the quantity to be subtracted from the reading, in order to give the height of the barometric column unaffected by the weight of the float.

It only remains to be added, that in comparing the few barometers of this construction, which have so far been made, with some others, among which were those of Mr. Troughton, of Mr. Hassler, and of Mr. Green—I mean coming from the hands of the persons named—the former, after deducting the amount of correction, stood uniformly higher than any of the latter; which I attribute not so much to the individual precautions in securing a better vacuum as to the greater precision with which the zero of the measurement can be ascertained.

Baltimore, Md., May 25, 1843.

Art. II.—*Notice of "Molluskite," or the fossilized remains of the soft parts of Mollusca;* by Gideon Algernon Mantell, Esq., LL. D., F. R. S., G. S., &c.

[Read before the Geological Society of London, January, 1843.]

Since the interesting discovery by Dr. Buckland of the nature and origin of the fossil remains termed *coprolites*, substances having the same general appearance and composition, but destitute of the spiral structure, and distributed in amorphous masses in the strata, have commonly been placed in the same category under the name of *pseudo-coprolites.*

In the blocks of *firestone* or upper green sand, which are seen at low water along the shore at Southbourne in Sussex, concretions of this kind are thickly interspersed among the shells which abound in those rocks. In my earliest geological researches along the Sussex coast, these fossil bodies particularly arrested my attention, but I failed to obtain any clue to their origin, until the important memoir on coprolites by Dr. Buckland, pointed out the right path of enquiry, and offered a satisfactory solution of a problem which had baffled the attempts of previous observers.

That a large proportion of the concretionary and nodular masses of the substance in question is the mineralized egesta of fishes and other marine animals, there can be but little doubt; although it is rarely possible to detect the traces of intestinal structure which are so commonly impressed, more or less distinctly, on the coprolites of the chalk, wealden, and lias. But in the rocks at Southbourne, instances are not unfrequent in which the coprolitic matter (I use the term for the sake of convenience) occurs in the state of casts of shells belonging to the genera Cucullæa, Venus, Trochus, &c., which abound in the firestone; and in these examples the substance appears to have originated from the soft bodies of the mollusca. In Sussex, in the layers of firestone which occur at the line of junction with the galt, pseudo-coprolites are very abundant. They are not uncommon in the beds of galt, at Ringmer and Norsington near Lewes, and at Bletchingly in Surrey; and they abound in the same argillaceous deposit at Folkstone in Kent. Dr. Fitton, in his elaborate memoir on the strata below the chalk, (Geological Transactions, Vol. IV, part 2, page 111,) has given an accurate description and analysis of the coprolitic nodules and concretions which occur at Folkstone. Dr. Fitton states, "that they resemble coprolites in their chemical composition, though no traces of animal structure are apparent in them. They sometimes enclose portions of shells, but no fragments of bone or scales of fishes have been detected. In some cases they are of a very irregular figure, surrounding or incorporated with fossil remains, especially of ammonites, *the anterior of which is filled up with matter of the same kind.*" The last quoted remark of this eminent geologist, bears immediately on the subject of the present communication.

In the grey Shanklin sand these substances also abound in some localities. I have observed them in western Sussex, in Surrey, in the Isle of Wight near Ventnor, and in Kent. But in no locality do they occur in greater number and variety, than in the "Iguanodon quarry" of Kentish rag near Maidstone, belonging to Mr. W. H. Beusted, to whose talents, zeal, and liberality, geology is indebted for many important discoveries.

Mr. Beusted having long paid attention to this subject, had the kindness to submit to my examination (more than two years since) several specimens of Rostellariæ, Trigoniæ, Cucullæa, and other shells, the cavities of which were filled with a dark brown substance, in every respect identical with the nodular and irregular concretions of coprolitic matter, which abound in the surrounding sandstone. At the same time Mr. Beusted expressed his conviction that the carbonaceous substance was derived from the soft bodies of the mollusca, and that the concretionary and amorphous portions of the same matter, dispersed throughout the sandstone of this bed, were fossilized masses of the soft bodies of the animals which had become disengaged from their shells, and floated in the sea, till enveloped in the sand and mud, which is now concreted into the sandstone called Kentish rag. The evidence collected by Mr. Beusted appears to me so conclusive, and so confirmatory of the correctness of the opinion I have previously advanced, that I beg to place before the Society the following abstract of his correspondence with me on the subject.

"The bed of Kentish rag in my quarry which lies immediately beneath the stratum that contained the remains of the Iguanodon, abounds in the usual shells of the Shanklin sand, particularly in Trigoniæ, (generally T. alæformis,) and there is an abundance of a dark brown coprolitic looking matter, of which I send you specimens. In some instances this material actually forms the entire casts of the univalves and bivalves, and I think there can be no doubt that it is derived from the soft bodies of the animals which inhabited the shells found in connection with it, fossilized in this peculiar manner. There are many examples which look more like true coprolites of fishes, and some of these contain shells partly crushed, as if they had been the partially digested contents of the intestinal canal. I am therefore inclined to think that the dark material which now occupies the shells, was the soft body of the mollusc, that those of a concretionary form which are imbedded in the stone are coprolites, and that the shapeless portions of this substance distributed in the rock have originated from floating masses of dead shell-fish. In illustration of the manner in which such an accumulation of materials as I find in my quarry, may have been formed, I beg to call your attention to the following extract from the American Journal of Science for 1837, which seems to me to afford an explanation of some of the appearances that I have attempted to describe.

" 'One of the most curious phenomena of the year 1836, was the fatal effect of an epidemic among the molluscous animals or shell fish of the Muskingum River, in the state of Ohio. It commenced in April and continued until June, destroying millions of that great race, which peoples the beds of streams. As the animals died, the valves of the shells opened, and decomposition commencing, the muscular adhesions gave way and the fleshy portions rose to the surface of the water, leaving the shells in the bed of the stream. As masses of the dead bodies floated down the current, the headlands of islands, piles of fixed drifted wood, and the shores of the river in many places were covered with them, and the air in the vicinity was tainted with the putrid effluvia exhaling from these accumulations of decomposing animal matter. The cause of the disease among the shelly tribes remains as much a mystery as that of the Asiatic cholera among the human race.'

"Now nearly the whole of the shells which occur in the bed of Kentish rag, appear to have been dead shells. I mean that from the open state of the valves it is probable that the animals for the most part, were dead before they were enveloped in the sand and mud; and from the large quantity of water-worn fragments of wood perforated by lithodomi, that is imbedded with them, it would seem that this stratum had constituted a bank of drifted wood and shells, presenting a very analogous condition to the phenomena above described. The gelatinous bodies of the Trigoniæ, Gervilliæ, Ostreæ, Rostellariæ, &c. detached from their shells may have been intermingled with the drift wood, in a sand bank, while in some instances the animal matter would remain in the shells, and become fossilized in the state observable in the accompanying examples."

The above remarks present a correct view of the circumstances under which the phenomena referred to occur in the quarry of Mr. Beusted.

Some of the dark substances extracted from a Trigonia, was submitted by my friend, the Rev. J. B. Reade, to a careful analysis by Mr. Rigg, who obligingly favored me with the following note: "My analysis confirms your suspicion respecting the presence of *animal carbon* in the substance which you sent for examination. After removing the lime, &c. by means of dilute hydrochloric acid from ten grains of the darker portion of the stone, there remained 1.2 grains of dark powder, which gave by analysis with oxide of copper .16 of a cubic inch of carbonic acid, and *apparently* a small portion of nitrogen. On subjecting to the same kind of analysis two grains of the darker body without previously acting upon it by any acid, .054 of a cubic inch of carbonic acid was obtained; so that from these results there is no doubt but the darker portion of these substances contains about .35 per cent. of its weight of carbon in an organized state."

The presence of animal carbon in fossil remains will, I expect, be found of frequent occurrence, not only enclosed in the shells of mollusca, but disseminated in the surrounding matrix. In the unique specimen of a fossil fox from Ruingen, which I had the pleasure of dissecting for our distinguished president, I found a considerable quantity of carbonaceous coprolitic matter within the abdominal region, and I have frequently detected its presence in the grit and sandstone of Tilgate Forest associated with the bones of reptiles. The black material which is so commonly seen to occupy some of the spiral cavities of the Paludina composing the Sussex and Purbeck marbles, and which by its contrast with the white calcareous spar, adds considerably to the beauty of those fresh-water limestones, contains a large proportion of animal carbon, doubtless derived from the soft parts of the mollusca having been enclosed at the period of its formation. A microscopical examination detects with a low power innumerable portions of the nacreous laminæ of the shells of extreme thinness, intermingled with the carbonaceous matter, together with numerous siliceous spiculæ of sponges, very minute spines of echinoderms and fragments of Polyparia; these extraneous bodies probably became entangled among the soft animal matter before the latter had undergone decomposition.

If my inferences be deemed correct, the term *molluskite* would be a proper designation for the substance in question.

N. B. This memoir was illustrated by drawings and numerous specimens of the molluskite, in some instances forming large amorphous carbonaceous masses in the sandstone, and in others filling the shells of the Trigoniæ, Terebratulæ, &c.*

Crescent Lodge, Clapham Common, (Eng.) January, 1843.

Art. III.—*An effort to refute the arguments advanced in favor of the Existence, in the Amphide Salts, of Radicals consisting, like Cyanogen, of more than one element;* by Robert Hare, M. D., Prof. Chem. Univ. Pennsylvania.

(Concluded from p. 65.)

46. Respecting the new principles which I have been contesting, Dr. Kane alleges "that the elegance and simplicity with which the laws of saline combination may be traced from them is remarkable," because he conceives, that without an appeal to those principles, the fact that the number of equivalents of acid in a salt are proportionable to the number of equivalents of oxygen in the base, would be inexplicable.

47. Thus, when the base is a protoxide, we have one atom of the protoxide of hydrogen to take its place; when the base is a sesquioxide (two of radical and three of oxygen,) three atoms of the protoxide of hydrogen take its place: if the base be a bioxide, two atoms of the protoxide of hydrogen take its place.

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* Dr. Mantell has been so kind as to forward to us, in illustration of his memoir, very distinct and satisfactory specimens of the *molluskite*, together with ammonites from the Kimmeridge clay, having elongated beaks in good preservation; also belemnites with their chambers preserved.—Eds.

48. I have already adverted to the existence of certain chemical laws, inexplicable in the present state of human knowledge. Among these is that of the necessity of oxidation to enable metallic radicals to combine with acids. But as a similar mystery exists as respects the adventitious property of combining with radicals, which results from the acquisition of an additional atom of oxygen by any of the compounds hitherto considered as anhydrous acids, the new doctrine has in that respect no pre-eminent claim to credence.

49. But if, without impairing the comparative pretensions of the prevailing doctrine, we may appeal to the fact that the acquisition of an atom of oxygen confers upon a radical the basic power to hold one atom of acid, is it not consistent that the acquisition of two atoms of oxygen should confer the power to hold two atoms of acid, and that with each further acquisition of oxygen a further power to hold acids should be conferred?

50. So far then there is in the old doctrine no more inscrutability than in that which has been proposed as its successor. Since if on the one hand it be requisite that for each atom of oxygen in the base, there shall be an atom of acid in any salt which it may form, on the other, in the case of the three oxyphosphions, for each additional atom of hydrogen extraneous to the salt radical, there must be an atom of oxygen superadded to this radical.

51. It being then admitted that, numerically, the atoms of acid in any oxysalt will be as the atoms of oxygen in the base, it must be evident that whenever an oxysalt of a protoxide is decomposed by a bioxide, there will have to be two atoms of the former for one of the latter. For the bioxide has two atoms of oxygen, and requires by the premises two atoms of acid, while the salt of the protoxide, having but one atom of oxygen, can hold, and yield, only one atom of acid. Two atoms of this salt, therefore, whether its base be water, or any other protoxide, will be decomposed by one atom of bioxide; provided the affinity of the acid for the bioxide predominate over that entertained for the protoxide, as when water is the base.

52. It follows, that the displacement of water from its sulphate, adduced by Kane, does not favor the idea that hydrous sulphuric acid is an oxysulphionide of hydrogen, more than the impression that it is a sulphate of water.

53. Of course, in the case of presenting either a sesquioxide, or a trioxide, to the last mentioned sulphate, in other words, hydrous sulphuric acid, the same rationale will be applicable.

54. The next argument advanced by Dr. Kane, is, that *some* of the acids of which the existence is assumed upon the old doctrine, are hypothetical, as they have never been isolated. This mode of reasoning may be made to react against the new doctrine with pre-eminent force, since *all* of the compound radicals imagined by it are *hypothetical—none* of them having been isolated.

55. The third argument of the respectable author above named is, that acids display their acid character in a high degree only when in the combination with water.

56. This argument should be considered in reference to two different cases, in one of which all the water held by the acid is in the state of a base, while in the other an additional quantity is present acting as a solvent. So far as water, *acting as a solvent*, facilitates the reaction between acids and bases, it performs a part in common with alcohol, ether, volatile oils, resins, vitrifiable fluxes, and caloric. Its efficacy must be referred to the general law, that fluidity is necessary to chemical reaction. "Corpora non agunt nisi soluta."

57. In a majority of cases, basic water, unaided by an additional portion acting as a solvent, is quite incompetent to produce reaction between acids and other bodies. Neither between sulphuric acid and zinc, between nitric acid and silver, nor between glacial or crystallized acids and metallic oxides, does any reaction take place without the aid of water acting as a solvent, and performing a part analogous to that which heat performs in promoting the union of those oxybases with boric, or silicic acid.

58. It is only with *soluble* acids that water has any efficacy. The difference between the energy of sulphuric and silicic acid, under the different circumstances in which they can reciprocally displace each other, is founded on the nature of the solvents which they require, the one being only capable of liquefaction by water, the other by caloric.

59. In support of his opinions the author adverts to the fact, that with hydrated sulphuric acid, baryta will combine energetically *in the cold*, while a similar union between the anhydrous vapor and the same base cannot be accomplished *without heat.* But it ought to be recollected, that to make this argument good,

it should be shown wherefore heat causes the baryta, a perfectly fixed body, to unite more readily with an aëriform substance in which increase of temperature must, by rarefaction, diminish the number of its particles in contact with the solid. If the only answer be, that heat effects some mysterious changes in affinity, (or as I would say in the electrical state of the particles,) it should be shown that the presence of water or any other base has not been productive of a similar change, before another explanation is held to be necessary. But I would also call to mind that the hydrated acid is presented in the liquid state; and if it be asked why water, having less affinity than baryta, can better cause the condensation of the acid, I reply, that it is brought into contact with the acid both as a liquid and a vapor, of neither of which forms is the earthy base susceptible. But if all that is necessary to convert anhydrous sulphuric acid into an oxysulphionide, be an atom of oxygen and an atom of metal, what is to prevent baryta and anhydrous sulphuric acid from forming an oxysulphionide of barium? All the elements are present which are necessary to form either a sulphate or oxysulphionide; and I am unable to conceive wherefore the inability to combine does not operate as much against the existence of radicals as of bases.

60. I would be glad to learn why, agreeably to the salt radical theory, anhydrous sulphuric acid unites with water more greedily than with baryta, and yet abandons the water promptly on being presented to this base. Why should it form an oxysulphionide with hydrogen more readily than with barium, and yet display, subsequently, a vastly superior affinity for barium?

61. It seems to be overlooked, that anhydrous sulphuric acid, being the oxysulphion of the sulphites, ought to form *sulphites* on contact with metals.

62. But if the sulphate of water owe its energy to that portion of this liquid, which, by its decomposition gives rise to the compound radical oxysulphion, and not to the portion which operates as a solvent, wherefore in the concentrated state, will it not react with iron and zinc, without additional water, when, with dilution, it reacts most powerfully with those metals.

63. Some stress has been laid upon the fact, that sourness is not perceived, excepting with the aid of water, as if to derive force for the new doctrine from that old and popular, though now abandoned test of acidity; but it should be recollected that it is not the

water which goes to form the compound element in the "*hydracids*," erroneously so called, which confers sourness. Will any one pretend that either sulphuric or nitric acid, when concentrated, is sour? Are they not caustic? Can any of the crystallized organic acids be said to have a sour taste, independently of the moisture of the tongue? The hydrated oily acids being incapable of uniting with water as a solvent, have none of these vulgar attributes of acidity. The absence of these attributes in prussic acid would alone be sufficient to render it inconsistent to consider them as having any connexion with the presence of hydrogen.

64. It has been remarked, that liquid carbonic acid does not combine with oxides on contact. To this I would add, that it does not combine with water under those circumstances, but, on the contrary, separates from it like oil, after mechanical mixture: nor does it, under any circumstances, unite with an equivalent proportion of water to form a hydrate. Of course, as it is not to basic water that it is indebted for its ability to become an ingredient in salts, it cannot be held that this faculty is the result of its previous conversion into an *oxycarbionide of hydrogen.*

65. Chromic acid is admitted not to require water for isolation, and cannot, therefore, be considered as oxychromionide of hydrogen. Yet the oil of bitter almonds, which consists of a *compound radical*, benzule, and an atom of hydrogen, and which is therefore constituted precisely as the salt radical doctrine requires for endowment with the attributes of an "hydracid," is utterly destitute of that acid reaction which hydrogen is represented as peculiarly competent to impart. It follows that we have, on the one hand, in chromic acid, a compound endowed with the attributes of acidity, without being a hydruret of any compound radical; and, on the other, in oil of bitter almonds, a hydruret of a compound radical, without any of the attributes of acidity.

66. The last argument in favor of the existence of salt radicals, which I have to answer, is that founded on certain results of the electrolysis of saline solutions.*

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* It is well known that Faraday employed a very simple instrument to ascertain the quantity of the gaseous elements of water yielded in a given time, by a liquid subjected to the voltaic current. It consisted of a graduated tube, through the cavity of which the current was conveyed by wires, so terminating within it, as to have an interval between them through which the current, being convey-

67. On subjecting a solution of sulphate of soda to electrolysis, so as to be exposed to the current employed, simultaneously with some water in a voltameter, Daniell alleges that, for each equivalent of the gaseous elements of water evolved in the voltameter, there was evolved at the cathode and anode, not only a like quantity of those elements, but likewise an equal number of equivalents of soda and sulphuric acid. This he considers as involving the necessity, agreeably to the old doctrine, of the simultaneous decomposition of two electrolytic atoms in the solution, for one in the voltameter; while, if the solution be considered as holding oxysulphionide of sodium, instead of sulphate of soda, the result may be explained consistently with the law ascertained by Faraday. In that case, oxysulphion would be carried to the anode, where, combining with hydrogen, it would cause oxygen to be extricated, while sodium, carried to the cathode, and deoxidizing water, would cause the extrication of hydrogen.

68. Dr. Kane, alluding to the experiments above mentioned, and some others which I shall mention, alleges that "*Professor Daniell considers the binary theory of salts to be fully established by them.*

69. Notwithstanding the deference which I have for the distinguished inventor of the constant battery, and disinclination for the unpleasant task of striving to prove a friend to be in the wrong, being of opinion that these inferences are erroneous, I feel it to be my duty, as a teacher of the science, to show that they are founded upon a misinterpretation of the facts appealed to for their justification.

70. It appears to me, that the simultaneous appearance of the elements of water, and of acid and alkali, at the electrodes, as above stated, may be accounted for, simply by that electrolyzation of the soda, which must be the natural consequence of the exposure of the sulphate of that base in the circuit. I will in support of the exposition which I am about to make, quote the language

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ed by the electrolytic process, effected the decomposition of the intervening liquid, the resulting gas being caught and measured by the tube. This instrument has been called a volta-electrometer, or voltameter.

Faraday found that when various substances were electrolyzed, a voltameter being at the same time in the circuit, that for every equivalent of water decomposed within the tube, neither more nor less than an equivalent of the other body could be decomposed.

of Professor Daniell, in his late work, entitled, "Introduction to Chemical Philosophy," page 413:—

"Thus we may conceive that the force of affinity receives an impulse which enables the hydrogen of the first particle of water, which undergoes decomposition, to combine momentarily with the oxygen of the next particle in succession; the hydrogen of this again, with the oxygen of the next; and so on till the last particle of hydrogen communicates its impulse to the platinum, and escapes in its own elastic form."

71. The process here represented as taking place in the instance of the oxide of hydrogen, takes place, of course, in that of any other electrolyte.

72. It is well known, that when a fixed alkaline solution is subjected to the voltaic current, that the alkali, whether soda or potassa, is decomposed; so that if mercury be used for the cathode, the nascent metal, being protected by uniting therewith, an amalgam is formed. If the cathode be of platinum, the metal, being unprotected, is, by decomposing water, reconverted into an oxide as soon as evolved. This shows, that when a salt of potassa or soda is subjected to the voltaic current, it is the alkali which is the primary object of attack, the decomposition of the water being a secondary result.

73. If in a row of the atoms of soda, extending from one electrode to the other, while forming the base of a sulphate, a series of electrolytic decompositions be induced from the cathode on the right, to the anode on the left, by which each atom of sodium in the row will be transferred from the atom of acid with which it was previously combined, to that next upon the right, causing an atom of the metal to be liberated at the cathode; this atom, deoxidizing water, will account for the soda and hydrogen at the cathode. Meanwhile the atom of sulphate on the left, which has been deprived of its sodium, must simultaneously have yielded to the anode the oxygen by which this metal was oxidized. Of course the acid is left in the hydrous state, usually called free, though more correctly esteemed to be that of a sulphate of water.

74. I cannot conceive how any other result could be expected from the electrolysis of the base of sulphate of soda, than that

which is here described. Should any additional illustration be requisite, it will be found in a note subjoined.*

75. I will, in the next place, consider the phenomena observed by Professor Daniell, when solutions of potassa and sulphate of copper, separated by a membrane, were made the medium of a voltaic current.

76. Of these I here quote his own account—(Philosophical Magazine and Journal, Vol. XVII, p. 172)—

"A small glass bell, with an aperture at top, had its mouth closed by tying a piece of thin membrane over it. It was half filled with a dilute solution of caustic potassa, and suspended in a glass vessel containing a strong neutral solution of sulphate of copper, below the surface of which

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* It is easy to understand how a simultaneous appearance of oxygen and acid at the anode, and soda and hydrogen at the cathode, may ensue, simply by the electrolyzation of the alkaline base from the following association of formulæ.

Anhydrous sulphuric acid is represented by the usual formula, SO_3; oxygen by the usual symbol, O; sodium by Na; water, acting as a solvent, by HO. Each atom of oxygen, sodium, or acid, is numbered from right to left, 1, 2, 3, 4, so that the change of position consequent to electrolysis may be seen.

Anode	$\overset{1.}{O}$	$\overset{2.}{O}$	$\overset{3.}{O}$	$\overset{4.}{O}$	Water. HO	Cathode.
	$\overset{1.}{Na}$	$\overset{2.}{Na}$	$\overset{3.}{Na}$	$\overset{4.}{Na}$		
	$\overset{1.}{SO_3}$	$\overset{2.}{SO_3}$	$\overset{3.}{SO_3}$	$\overset{4.}{SO_3}$		
	HO	HO	HO	HO		

Anode	$\overset{1.}{O}$	$\overset{2.}{O}$	$\overset{3.}{O}$	$\overset{4.}{O}$	O	H	*Cathode.*
		$\overset{1.}{Na}$	$\overset{2.}{Na}$	$\overset{3.}{Na}$	$\overset{4.}{Na}$		
	SO_3	$\overset{2.}{SO_3}$	$\overset{3.}{SO_3}$	$\overset{4.}{SO_3}$			
	HO	HO	HO	HO			

As the atoms are situated in the second arrangement, the atom of oxygen (1), is at the anode, the atom of sodium, $\overset{1.}{Na}$, with which it had been united, having been transferred to the second atom of sulphuric acid, which had yielded its sodium to the third atom of acid, $\overset{3.}{SO_3}$, this having, in like manner, yielded its sodium to the fourth atom of acid, $\overset{4.}{SO_3}$, from which the fourth atom of sodium, $\overset{4.}{Na}$, had been abstracted by the electrolytic power. The atom of sodium thus removed from the fourth atom of acid, is represented in union with the oxygen of an atom of water, of which the hydrogen, H, is at the cathode.

it just dipped. A platinum electrode, connected with the last zinc rod of a large constant battery of twenty cells, was placed in the solution of potassa; and another, connected with the copper of the first cell, was placed in the sulphate of copper immediately under the diaphragm which separated the two solutions. The circuit conducted very readily, and the action was very energetic. Hydrogen was given off at the platinode in a solution of potassa, and oxygen at the zincode in the sulphate of copper. A small quantity of gas was also seen to rise from the surface of the diaphragm. In about ten minutes the lower surface of the membrane was found beautifully coated with metallic copper, interspersed with oxide of copper of a black color, and hydrated oxide of copper of a light blue.

"The explanation of these phenomena is obvious. In the experimental cell we have two electrolytes separated by a membrane, through both of which the current must pass to complete its circuit. The sulphate of copper is resolved into its compound anion, sulphuric acid + oxygen (oxysulphion), and its simple cathion, copper: the oxygen of the former escapes at the zincode, but the copper on its passage to the platinode is stopped at the surface of the second electrolyte, which for the present we may regard as water improved in its conducting power by potassa. The metal here finds nothing by combining with which it can complete its course, but being forced to stop, yields up its charge to the hydrogen of the second electrolyte, which passes on to the platinode, and is evolved.

"The corresponding oxygen stops also at the diaphragm, giving up its charge to the anion of the sulphate of copper. The copper and oxygen thus meeting at the intermediate point, partly enter into combination, and form the black oxide; but from the rapidity of the action, there is not time for the whole to combine, and a portion of the copper remains in the metallic state, and a portion of the gaseous oxygen escapes. The precipitation of blue hydrated oxide doubtless arose from the mixing of a small portion of the two solutions."

77. It will be admitted, that agreeably to the admirable researches of Faraday, there are two modes in which a voltaic current may be transmitted, *conduction* and *electrolyzation*. In order that it may pass by the last mentioned process, there must be a row of anions and cathions forming a series of electrolytic atoms extending from the cathode to the anode. It is not necessary that these atoms should belong to the same fluid. A succession of atoms, whether homogeneous, or of two kinds, will answer, provided either be susceptible of electrolyzation. Both of the liquids resorted to by Daniell, contained atoms susceptible of

being electrolyzed. If his idea of the composition of sulphate of copper, and the part performed by the potassa, were admitted for the purpose of illustration, we should, on one side of the membrane, have a row of atoms consisting of oxysulphion and copper; on the other, of oxygen and hydrogen.

78. Recurring to Daniell's own description of the electrolyzing process, above quoted, an atom of copper near the anode being liberated from its anion, oxysulphion, and charged with electricity, seizes the next atom of oxysulphion, displacing and charging an atom of copper therewith united. The cupreous atom thus charged and displaced, seizes a third atom of oxysulphion, subjecting the copper, united with it, to the same treatment as it had itself previously met with. This process being repeated by a succession of similar decompositions and recompositions, an electrified atom of copper is evolved at the membrane, where there is no atom of oxysulphion. Were there *no other anion* to receive the copper, evidently the electrolyzation would not have taken place; but oxygen, on the one side of the membrane, must succeed to the office performed by oxysulphion on the other side; while hydrogen, in like manner, must succeed to the office of the copper.

79. Such being the inevitable conditions of the process, how can it be correctly alleged by Professor Daniell, the transfer of the copper being arrested at the membrane, that as this metal "*can find nothing to combine with*," it gives up its electrical charge to the hydrogen, which proceeds to the cathode? As hydrogen cannot be present, excepting as an ingredient in water, how can it be said that the copper can discharge itself upon the hydrogen, without combining with the oxygen necessarily liberated at the same time by the electrolytic process? How could the copper, in discharging itself to a cathion, escape a simultaneous seizure by an anion? Would not the oxidizement of this metal be a step indispensable to the propagation of that electrolytic process, by which alone the hydrogen could, as alleged, "*pass to the platinode*," i. e. cathode?

80. In these strictures I am fully justified by the following allegations of Faraday, which I quote from his Researches, 826, 828:—

"A single ion, i. e. one not in combination with another, will have no tendency to pass to either of the electrodes, and will be perfectly indifferent to the passing current, unless it be itself a compound of more elementary ions, and so subject to actual decomposition."

"If, therefore, an ion pass towards one of the electrodes, another ion must also be passing simultaneously to the other electrode, although, from secondary action, it may not make its appearance."

81. In explanation of the mixed precipitates produced upon the membrane, I suggest that the hydrated oxide resulted from chemical reaction between the alkali and acid, the oxide from the oxygen of the water or potassa acting as a cathion in place of that of the oxide of copper: also that the metallic copper is to be attributed to the solutions acting both as conductors and as electrolytes; so that, at the membrane, two feeble electrodes were formed, which enabled a portion of the copper to be discharged without combining with an anion, and a portion of oxygen to be discharged without uniting with a cathion. In this explanation I am supported by the author's account of a well known experiment by Faraday, in which a solution of magnesia and water was made to act as electrodes at their surfaces respectively.

82. There can, I think, be no better proof that no reliance should be placed on the experiments with membranes, in this and other cases where the existence of compound radicals in acids is to be tested, than the error into which an investigator, so sagacious as my friend Professor Daniell, has been led, in explaining the complicated results.

83. The association of two electrolytes, and the chemical reaction between the potassa and acid, which is admitted to have evolved the hydrated oxide, seem rather to have created difficulties than to have removed them.

84. In this view of the subject, I am supported by the opinion of Faraday, as expressed in the following language:—

"When other metallic solutions are used, containing, for instance, peroxides, as that of copper combined with this or any decomposable acid, still more complicated results will be obtained, which, viewed as the direct results of electro-chemical action, will, in their proportions, present nothing but confusion; but will appear perfectly harmonious and simple, if they be considered as secondary results, and will accord in their proportions with the oxygen and hydrogen evolved from water by the action of a definite quantity of electricity."

85. I cannot conceive, that in any point of view the complicated and "*confused*" results of the experiment of Daniell with electrolytes separated by membranes, are rendered more intelli-

gible by supposing the existence of salt radicals. I cannot perceive that the idea that the anion in the sulphate is oxysulphion, makes the explanation more satisfactory than if we suppose it to be oxygen. Were a solution of copper subjected to electrolysis alone, if the oxide of copper were the primary object of the current, the result would be analogous to the case of sodium, excepting that the metal evolved at the cathode, not decomposing water, would appear in the metallic form. If water be the primary object of attack, the evolution of copper would be a secondary effect.

86. It is remarkable, that after I had written the preceding interpretation of Daniell's experiments, I met with the following deductions stated by Matteuchi, as the result of an arduous series of experiments, without any reference to those of Daniell above mentioned. It will be perceived that these deductions coincide perfectly with mine.

87. I subjoin a literal translation of the language of Matteuchi from the Annales de Chimie et de Physique, tome 74, 1840, page 110:—

"When salt, dissolved in water, is decomposed by the voltaic current, if the action of the current be confined to the salt, for each equivalent of water decomposed in the voltameter, there will be an equivalent of metal at the negative pole, and an equivalent of acid, plus an equivalent of oxygen, at the positive pole. The metal separated at the negative pole will be in the metallic state, or oxidized, according to its nature. If oxidized, an equivalent of hydrogen will be simultaneously disengaged by the chemical decomposition of water."

88. Thus it seems, that the appearance of acid and oxygen at the anode, and of alkali and hydrogen at the cathode, which has been considered as requiring the simultaneous decomposition of two electrolytes upon the heretofore received theory of salts, has, by Matteuchi, been found to be a result requiring the electrolysis of the metallic base only, and, consequently, to be perfectly reconcilable with that theory.

89. In fact I had, from the study of Faraday's Researches, taken up the impression, that the separate appearance of an acid and base, previously forming a salt, at the voltaic electrodes, was to be viewed as a secondary effect of the decomposition of the water or the base; so that acids and bases were never the direct objects of electrolytic transfer.

Of Liebig's "Principles," so called.

90. Under the head of the "theory of organic acids," in Liebig's Treatise on Organic Chemistry, we find the following allegations dignified by the name of principles. Manifestly they must tend to convey a false impression to the student, that hydrogen has a peculiar property of creating a capacity for saturation, instead of being only the measure of that capacity, as is actually true, and likewise that in this respect it differs from any other radical.

91. The allegations to which I refer are as follows, being a literal translation from the French copy of the Traité of Liebig, page 7:—

"The hydrated acids are combinations of one or more elements with hydrogen, in which the latter may be replaced wholly or in part by equivalents of metals."

"The capacity of saturation depends consequently on the quantity of hydrogen which can be replaced.

"The compound formed by the other elements being considered as a radical, it is evident that the composition of this radical can exercise no influence on the capacity of saturation.

"The capacity of saturation of these acids augments or diminishes in the same ratio as the quantity of hydrogen, not entering into the salt radical, augments or diminishes.

"If into the composition of the salt radical there should be introduced an undetermined quantity of any elements, without changing the quantity of hydrogen extraneous to the radical, the atomic weight of the acid would be augmented, but the capacity of saturation would remain the same."

92. As by the advocates of the existence of "*salt radicals*," hydrogen is considered as playing the part of a metallic radical, and must, therefore, as respects any relation between it and the capacity of saturation, be in the same predicament as any other electro-positive radical, I cannot conceive wherefore laws, which affect every other body of this kind, should be stated as if particularly associated with hydrogen.*

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* There is, in some respects, a coincidence so remarkable as to the part taken by Dr. Kane and myself, with respect to hydrogen, that I quote here the language which has been held by us respectively on this subject.

Treating of hydrogen, Dr. Kane uses the following words:—"It was at one time supposed that it shared with oxygen the power of generating acids; and as sulphur, chlorine, iodine, cyanogen, &c., formed one class by combining with oxygen, so they formed a second class, called hydracids, by entering into union with

93. Would not a more comprehensive and correct idea be presented by the following language?—

94. From any combination of an acid with a base, either the base or its radical may be replaced by any other radical or base, between which and the other elements present, there is a higher affinity. Of course from acids called hydrated, from their holding an atom of basic water, either this base, or its radical (hydrogen), may be replaced by any other competent base or radical.

95. The premises being manifestly fallacious, still more so is the subsequent allegation, that in consequence of the hydrated acids being compounds formed with hydrogen, their capacity of saturation *depends* on the quantity of this element which can be replaced.

96. Is not this an inversion of the obvious truth, that the quantity of hydrogen present is as the capacity of saturation; and that, of course, the quantity of any element which can be substituted for it, must be in equivalent proportion? Would not a student, from this, take up two erroneous ideas—first, that the capacity of saturation is conferred by the radical, and in the next place, that of all radicals, hydrogen alone can give such a capacity? Is it not plain, that the assertion here made by the celebrated author, would be true of any radical?

97. Passing over a sentence which has no bearing on the topic under discussion, in the fourth allegation we have a reiteration

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hydrogen. * * * In the year 1832 I proved this view to be incorrect, that all the properties of the compounds of hydrogen combined to show that it was an eminently electro-positive body, that it took place along with iron, manganese, and zinc. * * * These views have been still farther corroborated by the researches of Graham. * * * There rests now no doubt, in the minds of philosophical chemists, that hydrogen is a metal enormously volatile."

This justifies the following language held in my letter on the Berzelian nomenclature.

"I am of opinion that the employment of the word hydracid, as co-ordinate with oxacid, must tend to convey the erroneous idea, with which, in opposition to his own definition, the author seems to have been imbued, that hydrogen in the one class, plays the same part as oxygen in the other. But in reality, the former is eminently a combustible, and of course the radical, by his own definition."

So entirely have I concurred in considering hydrogen as an aëriform metal, that, for more than twenty years, I have, in my lectures, accounted for the amalgamation of mercury when electrolyzed in contact with sal ammoniac, by inferring ammonia to be a gaseous alloy of two metallic ingredients, hydrogen and nitrogen being both aëriform metals.

and expansion of the error of those by which it is preceded. We are informed that the "*capacity of saturation augments and diminishes with the quantity of hydrogen which can be replaced*," which is again an inversion of the truth, that the quantity of hydrogen varying with the capacity, the quantity of any other radical, competent to replace it, must be in equivalent proportion.

98. Is not the concluding allegation a mere truism, by which we are informed, "that if any undetermined quantity of any element should be introduced into the composition of the radical, without changing the capacity (as measured by hydrogen), the capacity would be found the same when measured by any other radical?"

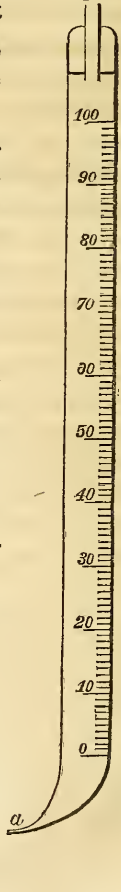
99. As all that is thus ascribed to hydrogen must be equally true of any other radical, there would have been less liability to misapprehension, had the generic term radical been employed wherever hydrogen is mentioned. But by employing the word radical to designate halogen elements, the advocates of the existence of compound radicals in amphide salts have deprived the word in question of much of its discriminating efficacy. In fact, their nomenclature would confound all ultimate elements under one generic appellation, and all their binary combinations under another, so that almost every chemical reagent, whether simple or compound, would be a salt or a radical.

100. Before concluding, I feel it to be due to the celebrated German chemist above mentioned, to add, that however I may differ from him as to the acids being hydrurets of compound radicals, I am fully disposed to make acknowledgments for the light thrown by his analytical researches on organic chemistry, and the successful effect of his ingenious theoretic speculations, in rendering that science more an object of study with physicians and agriculturists.

Art. IV.—*A New Instrument for estimating the quantity of Carbonate of Lime present in Calcareous Substances;* by J. Lawrence Smith, M. D.

Among the most ready methods used for the purpose of estimating the quantity of carbonate of lime contained in calcareous substances, are Davy's pneumatic and Rogers' methods, the one estimating it from the bulk of carbonic acid, and the other by the weight of the carbonic acid afforded by the action of an acid. The principal objection to the former, is the complication of the apparatus, and for the latter it is necessary to be furnished with a more than ordinary pair of balances, and a set of accurate weights, whereas the instrument about to be described is free from both these objections, with the additional advantage of affording more accurate results.

Fig. 1.



It appeared at first, that by taking a certain quantity of the substance to be examined, and letting fall upon it by degrees a solution of acid, the strength of which we know, that it might be possible to estimate the quantity of carbonate of lime in the same manner as the carbonates of the fixed alkalies are estimated; but for this to succeed, it is necessary that the substance should be finely pulverized, and free from any materials soluble in the acid used, but as it is not common to be furnished with these two conditions, another method had to be adopted, the principle of which is, to treat the calcareous substance with an *excess* of acid, the strength of which is known, and then to find out the amount of this excess, thereby knowing the quantity of acid taken up, from which we can easily calculate the quantity of carbonate of lime present. In the application of this principle, it will be found that any thing like difficult manipulation is avoided, and that there is no calculation required.

The first thing to be furnished with, is an instrument, which consists simply of a tube about half an inch in diameter, and ten inches long, having the principal part of it graduated in one hundred parts. The simplest form to be given to this tube, is such as is represented in figure 1, the extremity *a* being drawn out and bent downwards, leaving an opening so small as to allow a liquid to flow but slowly from the tube. To the upper part, for convenience' sake, is adapted a perforated cork, with a small tube; this is placed for the purpose of regulating the flow of the fluid, by placing upon it and withdrawing from it the finger, as we may wish to arrest or allow the liquid to flow from the extremity *a*. With this instrument, that I propose calling the *Calcarimeter* from its use, we must be furnished with two fluids, a solution of muriatic or nitric acid, and a solution of ammonia, both of which are prepared of a certain strength.*

Preparation of the acid solution.—This solution is prepared as follows: weigh out fifty grains of dry finely powdered pure carbonate of lime, or what is better, carbonate of lime precipitated from any of its solutions by carbonate of potash or soda; place this in a tea-cup or other convenient vessel, add to it about an ounce of water, (this is done simply for the purpose of moderating the action of the acid,) then take the muriatic or nitric acid of commerce, dilute it with one part of water; with this liquid fill the instrument to the 100 point, then let the acid fall gently upon the carbonate of lime, so as not to create a too great effervescence, and by proceeding carefully with the aid of a piece of litmus paper, we can find the exact point at which the carbonate of lime is all taken up, by the solution having an acid reaction. When we see that nearly all the lime is taken up we proceed very cautiously, by adding but a few drops of the acid at a time, and agitating the mixture considerably, for the purpose of bringing the insoluble carbonate well in contact with the different parts of the fluid. When the acid reaction commences, the acid is no longer added, and the point at which the acid now stands in the tube is marked, and by subtracting that from 100 we have

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* The capacity of the instrument, from 0 to 100 is one ounce, and the length of the graduation had better be from eight to ten inches; of course this will vary with the diameter of the tube. As they are all to be of the same capacity, the graduation may be made upon the tube itself or upon a piece of paper and pasted on, then varnished, first with a solution of gum arabic, and afterwards with copal varnish.

the number of degrees of acid used to dissolve fifty grains of carbonate of lime, but as it is desired that the liquid should be so made as to require 50° of it to dissolve fifty grains of the carbonate, it is diluted with the proper quantity of water. For example, suppose the fluid marked 65° after the experiment; this indicates that 35° of the acid solution were required to dissolve the 50 grains. Now instead of 35° we require it to take 50° to dissolve the same quantity, so that by making up the difference between the thirty five and fifty with water the solution is prepared; that is to say, to every thirty five parts of the acid experimented with, fifteen parts of water are added. The solution can be again tested if necessary, and slight modifications made.

Preparation of the alkaline solution.—The alkaline solution is now prepared with ease. Let fall 50° of the acid into a vessel, then make a mixture of equal parts of ammonia and water, fill the instrument to the 100°, and let it flow upon the acid, and mark the point at which the acid is neutralized; suppose it to be twenty, then 80° have been used for that purpose; but it must be so made as that it will require 100°, therefore to every eighty parts of the solution experimented with, add twenty parts of water. In making either of these solutions, one gallon can be made with the same ease as one ounce, and moreover, when they are once made, there is never any necessity of recurring to the carbonate of lime, as the acid may now be prepared with aid of the ammonia.

Fig. 2.

Thus, then, 50° of acid dissolve exactly fifty grains of pure carbonate of lime, and 100° of the ammonia neutralize fifty of the acid.

As using the same tube for both acid and alkali is attended with some inconvenience, having to wash it out after using one before introducing the other, I have used an additional tube, (fig. 2,) about the same diameter and a little more than half as long as the calcarimeter, for the acid. It has simply three marks upon it; the capacity of the tube from the point marked *a* to the lower extremity is equal to the capacity of 50° of the other tube, and the other two marks correspond to ten and five; the use that is made of these, will be hereafter explained.

Manner of performing the analysis.—Being furnished with the two tubes, the two fluids, a cup or other convenient vessel, a small piece of glass rod a few inches long, a wine-glass, and a piece of litmus paper, a portion of which has been reddened by an acid, we proceed as follows. Weigh out fifty grains of the substance to be examined, place it in the cup and add to it about one ounce of water, fill the instrument last described up to the highest mark upon the stem with the *acid;* this is done by holding it between the thumb and fore-finger, having the little finger applied to the lower opening. After the acid is poured in, before withdrawing the finger, introduce the cork and place the fore-finger of the other hand upon the opening of the tube on the cork, for the purpose of preventing the liquid flowing out, when the lower opening is left unprotected; after seeing that the acid stands exactly at the mark, it is allowed to flow gradually upon the substance. After all the action has ceased, stirring it towards the end to insure this result, we fill the graduated tube with the solution of ammonia, in the same manner as we did the last, and let it fall gradually upon the mixture of acid and calcareous substance, arresting at will the progress of the flow, by simply placing the finger upon the tube in the cork. This instrument should always be transferred to the left hand and held in an inclined position. During the addition of ammonia, the mixture should be well agitated with the glass rod, and occasionally tested by bringing a little of it upon the extremity of the rod in contact with the litmus paper, and so soon as it ceases to turn this paper red, or begins to turn the red part of it blue, the experiment is completed, and we now look at what number of degrees the fluid stands in the tube and we are furnished with the per centage of carbonate of lime contained in the calcareous substance examined.* We may be saved the trouble of testing too often, by paying attention to the strength of the reaction of the fluid upon the litmus paper.

In most marls which have served as the subjects of my experiments, more or less alumina is to be found, a part of which is dissolved by the acid, of which part a very good use can be made. While adding the ammonia, the alumina immediately

* If magnesia happens to be present, it will be estimated as lime, but this will very seldom be a cause of error, as it exists very rarely in calcareous manures, for which this instrument is particularly intended.

around where the ammonia falls, is thrown out of solution, and if we stir the liquid, the alumina will be redissolved so long as there is any free acid, so that when the flocks of alumina are no longer taken up, we are furnished with an assurance that the process is nearly completed. The acid that the alumina and iron takes up is acted upon by the ammonia, with almost the same readiness as if free, so that no cause of error is to be apprehended from that source.

It may sometimes happen from oversight, that too much ammonia is added; notwithstanding this, the analysis need not be lost. Still holding the instrument in the left hand over the cup, having of course arrested the flow of the fluid, we pour some of the acid solution into the wine-glass, introduce the small end of the acid instrument into it, and allow it to rise on the inside to either of the small marks, and add this acid to the liquid and go on as before with the experiment, and at the conclusion read off what is indicated and to it add ten or twenty according as we may have added the acid measured by the first or second mark.

After what has been said, a few words will suffice to explain how the instrument operates.

It takes 50° of acid to dissolve fifty grains of carbonate of lime, or 1° to dissolve one grain, and it takes 2° of the ammonia solution to neutralize one of the acid, and therefore in treating a substance consisting in part of carbonate of lime, for every grain that is present one degree of the acid is taken up, so that when we come to add the ammonia, we know how much of the acid is taken up by the quantity of ammonia left behind, thereby knowing the number of grains of carbonate of lime, which we multiply by two, (as fifty grains of the substance was used,) to arrive at the per centage. This multiplication is not actually performed, as the instrument is so graduated as to dispense with it.

Were it at all necessary to give any evidence of its easy application, I might state that it, along with the fluid, has been placed in the hands of persons entirely unacquainted with chemistry, and even with the principle of the instrument, and they have, with some little instruction in the manipulations necessary, obtained results only one or two per cent. out the way, in their *first* examination.

Art. V.—*On the Method of Drs. Varrentrapp and Will for estimating the Nitrogen in Organic Compounds;* by J. Lawrence Smith, M. D.

TO THE EDITORS.

As I sent you some time since, an account of a new method invented by MM. Varrentrapp and Will for estimating the quantity of nitrogen contained in organic substances,* it becomes my duty to see that you are furnished with an account of M. Reizet's investigation of this method, which investigations were presented to the Académie des Sciences in July of last year.

M. Reizet finds that the method is applicable, with some care, to substances containing large quantities of nitrogen, and small quantities of carbon, but when we analyze a substance poor in nitrogen and rich in carbon, the result is invariably inaccurate.

M. Reizet was led to suspect that there were errors attendant upon this method, from the fact that some years ago Mr. Faraday showed that organic substances containing no nitrogen, when heated with potash in contact with air afforded ammonia, a circumstance which must affect materially MM. Varrentrapp and Will's method, for they burn the organic substance, mized with potash and lime, in a tube, the tube being partly filled with air, so that when heated not only would all the nitrogen of the substance be converted into ammonia, but also the nitrogen of the air contained in the tube, so that when the nitrogen was calculated from this ammonia, a larger quantity than the substance rarely contained would be indicated.

Sugar and other substances devoid of nitrogen were burnt by M. Reizet, after the method made mention of, and he stated to me that he has obtained as much as 2 per cent. of nitrogen in burning 15 grains of sugar with the mixture of potash and lime, and the truth of which I have since witnessed. The following is a table of the results of some of his experiments upon sugar.

0.250	gram.	sugar	furnished	0.0038	gram.	nitrogen.
0.500	"	"	"	0.0075	"	"
1.000	"	"	"	0.0127	"	"
1.500	"	"	"	0.0149	"	"
2.000	"	"	"	0.0153	"	"

* See this Journal, Vol. xlii, p. 253, April, 1842.

Besides substances destitute of nitrogen, many that contained but a small quantity of nitrogen were experimented with, and always a larger quantity of ammonia was obtained than was due to the nitrogen present in the substance.

M. Reizet has also found that the mixture of alcohol and ether used, decomposes a small quantity of the bichloride of platinum, which is always added in excess, so as to form a small quantity of the protochloride, which being insoluble in this menstruum, is only a cause of error.

From these experiments, M. Reizet concludes that MM. Varrentrapp and Will's method should be used with great reserve, when new substances are the subjects of analysis, but he does not doubt that in the hands of skillful manipulators, it might become a valuable means of control.

The contents of this letter should have been communicated to you before, but I have been for some time intending to try a modification of this method, which suggested itself to me, as a means of remedying the defects pointed out by M. Reizet, but my occupations have prevented me from testing this modification, and as it is impossible for me to say when I shall be able to perform the necessary experiments, I have to content myself, in bringing it to your notice simply as a suggestion.

First, then, in preparing the mixture of potash and lime, after we have heated the mixture for the purpose of drying it, while hot, place it beneath a vessel containing oxygen gas, which gas will become condensed in the pores of the mixture, and prevent the nitrogen of the air finding its way in, when exposed to the atmosphere. Oxygen is preferred to hydrogen gas, from the fact that the former would be absorbed in larger quantity, and more effectually exclude the nitrogen. Thus much then for the preparation of the mixture. When employed, it is to be mixed with the substance to be analyzed, introduced into the tube, and the air of the tube is then to be driven out either by oxygen or hydrogen gas; if these precautions are taken, which will certainly not be found difficult of application, I doubt not that all error arising from the nitrogen of the air would be avoided.

Charleston, S. C., April 12, 1843.

Messrs. Editors—I have just received the January number of the Annalen der Chemie und Pharmacie, and there find an article of M. Will's concerning M. Reizet's experiments upon the new method of estimating the nitrogen of organic bodies. M. Will has not been able to obtain an appreciable quantity of nitrogen from the combustion of sugar and other organic substances free from nitrogen.

1.214 gram. sugar candy, burnt with the soda and lime, gave 0.00086 gram. nitrogen, which represents 0.07 per cent. of the sugar used.

0.386 gram. stearic acid gave 0.00028 gram. nitrogen.

Numerous other experiments were made with a larger quantity of the same and other substances, with similar results, and he accounts for M. Reizet obtaining ammonia from sugar, by supposing that his mixture of soda and lime contained a nitrate, probably the nitrate of potash.

Dr. Fownes has also been testing the experiments of M. Reizet, and finds them incorrect, substantiating those of M. Will. 10 grs. crystallized tartaric acid gave him 0.127 p. c. nitrogen, a quantity too small to be considered.

M. Constantin Zwenger, in his article on Elaterin, (Ann. der Chem. und Phar. Sept. 1842,) states that this substance contains no nitrogen, having satisfied himself of that fact by MM. Varrentrapp and Will's method, the substance being constituted as follows, $C^{20}H^{14}O^{5}$.

I have also examined a specimen of animal charcoal that gave M. Laurent by the old method 2.5 per cent. of nitrogen, which when burnt with the soda and lime gave me 2.6 per cent.

Yours, &c. J. LAWRENCE SMITH.

Charleston, June 9, 1843.

ART. VI.—*Remarks on the First Principles of the Differential Calculus, together with a new investigation of Taylor's Theorem;* by Prof. THEODORE STRONG.

LET φx denote any function of x, and suppose that x is changed to $x+h$, then φx becomes $\varphi(x+h)$, which it is our object to express in a series; considering x and h as indeterminate quantities, which are independent of each other.

We may evidently assume $\varphi(x+h)=\varphi x+(\mathrm{A}+\mathrm{B})\psi h$, (1), (a finite expression;) and suppose that A is a function of x and independent of h, also that B is a function of x and h, such that it $=0$ when $h=0$, and that ψh is a function of h independent of x, and such that it $=0$, when $h=0$; for according to these suppositions when $h=0$, (1) becomes identically $\varphi x=\varphi x$, as it (evidently) ought to be.

Since h is arbitrary, we may put $2h$ for h in (1), and if we use B′ to denote the value of B when h is changed to $2h$, (so that B′ is the same function of x and $2h$ that B is of x and h,) (1) becomes $\varphi(x+2h)=\varphi x+(A+B')\psi(2h)$, (1′); also since x is arbitrary we may put $x+h$ for x in (1), and if we denote the increments of A and B (arising from the substitution of $x+h$ for x in A and B, which are supposed to be functions of x,) by $\varDelta A$ and $\varDelta B$, it becomes $\varphi(x+2h)=\varphi(x+h)+(A+B)\psi h+(\varDelta A+\varDelta B)\psi h$, or substituting the value of $\varphi(x+h)$ from (1), $\varphi(x+2h)=\varphi x+(A+B)2\psi h+(\varDelta A+\varDelta B)\psi h$, (1″); and subtracting (1″) from (1′), we get $A[\psi(2h)-2\psi h]+B'\psi(2h)-2B\psi h-[\varDelta A+\varDelta B]\psi h=0$, which must be an identical equation; ∴ since A is independent of h, and B′, B, $\varDelta A$, $\varDelta B$, are not independent of it, (since each of them $=0$ when $h=0$,) we must have $\psi(2h)-2\psi h=0$ or $\psi(2h)=2\psi h$, and since h is indeterminate $\psi=1$, ∴ $2h=2h$, an identical equation, and h is arbitrary, as it ought to be; hence the equation is easily reduced to $2(B'-B)-\varDelta A-\varDelta B=0$, (2), which must be satisfied so as to be an identical equation.

Since $\psi=1$, (1) becomes $\varphi(x+h)=\varphi x+(A+B)h=\varphi x+Ah+Bh$, which shows that $Ah+Bh$ is the increment of φx arising from the substitution of $x+h$ for x, ∴ we may denote this increment by $\varDelta\varphi x$, and shall have $\varDelta\varphi x=Ah+Bh$, (3), so that (1) becomes $\varphi(x+h)=\varphi x+\varDelta\varphi x$, (4).

First Principles of the Differential Calculus.

We may consider h as an increment of x, and denote it by $\varDelta x$, and (3) becomes $\varDelta\varphi x=A\varDelta x+B\varDelta x$, (3′), or $\frac{\varDelta\varphi x}{\varDelta x}=A+B$, (3″), which must manifestly be an identical equation, and be satisfied so as to leave $\varDelta x$ indeterminate; ∴ since A is independent of $\varDelta x$, (or h,) the first member of the equation must be considered as having a term which is independent of $\varDelta x$, ∴ if we denote this term by $\frac{d\varphi x}{dx}$, we get $\frac{d\varphi x}{dx}=A$, (4′), or $d\varphi x=\frac{d\varphi x}{dx}\cdot dx=A dx$, (4″); where x is called the independent variable, φx a function of x, and dx, $d\varphi x$ are called their differentials, and $\frac{d\varphi x}{dx}=\mathrm{A}$ is called the differential co-efficient, since we must multiply dx by it to obtain $d\varphi x=\frac{d\varphi x}{dx}.dx=\mathrm{A}dx$. The same results are readily obtained by writing for $\Delta\varphi x$ in the first member of (3′), $d\varphi x+\Delta'\varphi x=\mathrm{A}\Delta x+\mathrm{B}\Delta x$, the d relating to the term that involves the first power of Δx only, and the Δ' to the remaining part of the right member of (3′), $\therefore$ we get $d\varphi x=\mathrm{A}\Delta x=\mathrm{A}dx$, by using d for Δ, in the right member of the equation; this process shows the propriety of calling the method of obtaining the expression $d\varphi x=\mathrm{A}dx$, (together with its various applications,) the differential calculus, since $\mathrm{A}dx$ is only a part of the entire difference $\mathrm{A}h+\mathrm{B}h$, obtained by putting $d\varphi x=\mathrm{A}h=\mathrm{A}dx$. We consider the method which we have given (deduced from considering (3′) or (3″) as an identical equation) for obtaining (4′) or (4″), as being the true foundation of the differential calculus.

These remarks however are to be understood as referring to the principles of the science; for in practice the common method of regarding $\frac{d\varphi x}{dx}=\mathrm{A}$ as expressing the limit of the ratio $\frac{\Delta\varphi x}{\Delta x}=\mathrm{A}+\mathrm{B}$ when Δx is diminished in infinitum, is generally more simple and expeditious than any known method, and is therefore by no means to be abandoned.

Again, the method of Leibnitz, which consists in rejecting the term $\mathrm{B}\Delta x$ in comparison with the term $\mathrm{A}\Delta x$ in (3′) when Δx is indefinitely small, so that $\Delta\varphi x=\mathrm{A}\Delta x$, or denoting these suppositions by using d instead of Δ, $d\varphi x=\mathrm{A}dx$, has its practical advantages.

Finally, we may consider, (if we please,) $\frac{d\varphi x}{dx}=\mathrm{A}$ as denoting the operation that must be performed on φx in order to obtain A, the co-efficient of the first power of h (only) in the expansion of $\varphi(x+h)$; for it is only this co-efficient that is obtained by the several methods that we have noticed; and we may observe that if we change h into dx, we shall get $\varphi(x+dx)=\varphi x+\mathrm{A}dx+\mathrm{B}dx$, and that the term $\mathrm{A}dx$ is the differential of φx, so that we have $d\varphi x=\mathrm{A}dx$, or $\frac{d\varphi x}{dx}=\mathrm{A}$.

Investigation of Taylor's Theorem.

We shall now resume (2), $2(B'-B)-\varDelta A-\varDelta B=0$, ($a$), which we may put under the form $2(B'-B)-\varDelta(A+B)=0$, (a'), which is to be satisfied so as to be an identical equation; hence since B' is the same function of x and $2h$, that B is of x and h, it is manifest that B must be of the same form as $\varDelta A$, also that B' has the same form as $\varDelta A$, excepting that we must use $2h$ in B' where we use h in $\varDelta A$ or B.

If we substitute $\frac{d\varphi x}{dx}$ for A in (3) it becomes $\varDelta\varphi x=\frac{d\varphi x}{dx}h+Bh$ (3‴), which substituted in (4) gives $\varphi(x+h)=\varphi x+\frac{d\varphi x}{dx}h+Bh$, ($b$); since A is a function of x, the form of $\varDelta A$ must be similar to that of $\varDelta\varphi x$ when we use A instead of φx, ∴ we may put $\varDelta A=\frac{dA}{dx}h+A'h$, (5), where $\frac{dA}{dx}$ is independent of h, and A' is a function of x and h, such that it $=0$ when $h=0$; since $A=\frac{d\varphi x}{dx}$ we get $\frac{dA}{dx}=\frac{d\left(\frac{d\varphi x}{dx}\right)}{dx}$, or (if we denote $\frac{d\left(\frac{d\varphi x}{dx}\right)}{dx}$ by $\frac{d^2\varphi x}{dx^2}$ as is customary,) $\frac{dA}{dx}=\frac{d^2\varphi x}{dx^2}$, which reduces (5) to $\varDelta A=\frac{d^2\varphi x}{dx^2}h+A'h$, (5′); hence (from what has been said) we may represent B and B' by $B=B_{\prime}h+\theta(x, h).h$, $B'=B_{\prime}2h+\theta(x, 2h).2h$, (6), where $B_{\prime}$ is supposed to be independent of h, and $\theta(x, h)$ denotes the same function of x and h, that $\theta(x, 2h)$ does of x and $2h$, these functions being such as to $=0$ when $h=0$, so that the forms of B and B' are similar to that of $\varDelta A$, as they ought to be; and $\varDelta B=h[\varDelta B_{\prime}+\varDelta\theta(x, h)]$, (7). By substituting the values of $\varDelta A$, B, B' and $\varDelta B$ in (a), and rejecting the common factor h, it becomes after a slight reduction $2B_{\prime}-\frac{dA}{dx}-A'+4\theta(x, 2h)-2\theta(x, h)-\varDelta B_{\prime}-\varDelta\theta(x, h)=0$, which is to be an identical equation, ∴ since $2B_{\prime}$ and $\frac{dA}{dx}$ are independent of h, and since the other terms of the equation are not independent of it, (since each of them $=0$ when $h=0$,) we must put $2B_{\prime}-\frac{dA}{dx}=0$, or $B_{\prime}=\frac{1}{2}\frac{dA}{dx}=\frac{1}{2}\frac{d^2\varphi x}{dx^2}$,

$\therefore B = \frac{1}{2}\frac{d^2\varphi x}{dx^2}h + \theta(x,h).h$, which substituted in $(3''')$ gives $\varDelta\varphi x = \frac{d\varphi x}{dx}h + \frac{1}{2}\frac{d^2\varphi x}{dx^2}h^2 + \theta(x,h).h^2$, $(3^{\prime v})$; and since the form of $\varDelta A$ must be similar to that of $\varDelta\varphi x$, when we use A instead of φx, we may denote $\varDelta A$ by $\varDelta A = \frac{dA}{dx}h + \frac{1}{2}\frac{d^2A}{dx^2}h^2 + \theta'(x,h).h^2$, $(5'')$, $\theta'(x,h)$ denoting a function of x and h, such that it $=0$ when $h=0$; and if (according to the usual method) we denote $\frac{d\left(\frac{d^2\varphi x}{dx^2}\right)}{dx}$ by $\frac{d^3\varphi x}{dx^3}$, we get $\frac{d^2A}{dx^2} = \frac{d^3\varphi x}{dx^3}$; and we may here observe that we shall denote any expression of the form $\frac{d\left(\frac{d^{n-1}\varphi x}{dx^{n-1}}\right)}{dx}$ by $\frac{d^n\varphi x}{dx^n}$, where n is supposed to be a positive integer greater than zero. Instead of using the equation that remains after putting $2B_{,} - \frac{dA}{dx} = 0$, we shall use (a') in what follows; and since the forms of B and B′ are to be similar to that of $\varDelta A$, we may by $(5'')$ represent them by $B = B_{,}h + B_2h^2 + \theta''(x,h).h^2$, and $B' = B_{,}2h + B_2(2h)^2 + \theta''(x,2h).(2h)^2$, $\theta''(x,h)$ being the same function of x and h that $\theta''(x,2h)$ is of x and $2h$, each of these functions being $=0$ when $h=0$; and since $A = \frac{d\varphi}{dx}$, $B_{,} = \frac{1}{2}\frac{d^2\varphi x}{dx^2}$, we get $A + B = \frac{d\varphi x}{dx} + \frac{1}{2}\frac{d^2\varphi x}{dx^2}h + B_2h^2 + \theta''(x,h).h^2$; hence substituting the values of B, B′, and A+B in (a'), it becomes $2\left(\frac{1}{2}\frac{d^2\varphi x}{dx^2}h + 3B_2.h^2 + \theta''(x,2h).(2h)^2 - \theta''(x,h).h^2\right) - \varDelta\left(\frac{d\varphi x}{dx} + \frac{1}{2}\frac{d^2\varphi x}{dx^2}h + B_2h^2 + \theta''(x,h).h^2\right) = 0$, (a''').

If we develope $\varDelta\left(\frac{d\varphi x}{dx} + \frac{1}{2}\frac{d^2\varphi x}{dx^2}h + \&c.\right)$ by $(5'')$, we get $\varDelta\left(\frac{d\varphi x}{dx} + \frac{1}{2}\frac{d^2\varphi x}{dx^2}h + \&c.\right) = \frac{d^2\varphi x}{dx^2}h + \frac{d^3\varphi x}{dx^3}h^2 + \&c.$ which being substituted in (a'''), rejecting the terms which destroy each other, dividing by h^2, then putting the terms which are independent of h (or which do not $=0$ when $h=0$) equal to zero, we get $2\,.\,3B_2 - \frac{d^3\varphi x}{dx^3}=0$, or $B_2=\frac{1}{2\,.\,3}\,\frac{d^3\varphi x}{dx^3}$; hence $(3'^{v})$ becomes $\varDelta\varphi x = \frac{d\varphi x}{dx}h+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h^2+\frac{1}{2\,.\,3}\,\frac{d^3\varphi x}{dx^3}h^3+\theta''(x,\,h)\,.\,h^3$, (3^{v}); and since $\varDelta A$ has the same form as $\varDelta\varphi x$, $(5'')$ becomes $\varDelta A=\frac{dA}{dx}h+\frac{1}{2}\frac{d^2A}{dx^2}h^2 + \frac{1}{2\,.\,3}\,\frac{d^3A}{dx^3}h^3+\theta'''(x,\,h)h^3$, $(5''')$, and as B and B′ must be of similar forms, it is evident from what has been done that we may put $B=B_{,}h+B_2h^2+B_3h^3+B_4h^4+$&c., and $B'=B_{,}(2h)+B^2(2h)^2+B_3(2h)^3+B_4(2h)^4+$&c.; ∴ substituting these values and $A+B=\frac{d\varphi x}{dx}+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\frac{1}{2\,.\,3}\,\frac{d^3\varphi x}{dx^3}h^2+B_3h^3+$&c. in (a'), we get $2\left(\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\frac{1}{2}\frac{d^3\varphi x}{dx^3}h^2+(2^3-1)B_3h^3+(2^4-1)B_4h^4+\text{\&c.}\right) - \varDelta\left(\frac{d\varphi x}{dx}+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\frac{1}{2\,.\,3}\,\frac{d^3\varphi x}{dx^3}h^2+B_3h^3+\text{\&c.}\right)=0$, (a'^{v}), which is under a more convenient form than (a''').

If we develope $\varDelta\left(\frac{d\varphi x}{dx}+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\frac{1}{2\,.\,3}\,\frac{d^3\varphi x}{dx^3}h^2+\text{\&c.}\right)$ by $(5''')$, reject the terms which destroy each other, divide by h^3, then put the terms which are independent of h, equal to zero, we shall get $2(2^3-1)B_3-2\left(\frac{1}{2\,.\,3}+\frac{1}{2\,.\,4}\right)\frac{d^4\varphi x}{dx^4}=0$, or $B_3=\frac{1}{2\,.\,3\,.\,4}\,\frac{d^4\varphi x}{dx^4}$, and substituting this value of B_3 in (a'^{v}), we shall in the same way find $B_4=\frac{1}{2\,.\,3\,.\,4\,.\,5}\,\frac{d^5\varphi x}{dx^5}$, and so on; hence $A+B=\frac{d\varphi x}{dx}+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\frac{1}{2\,.\,3}\,\frac{d^3\varphi x}{dx^3}h^2+\frac{1}{2\,.\,3\,.\,4}\,\frac{d^4\varphi x}{dx^4}h^3+$ &c. and substituting this in $\varphi(x+h)=\varphi x+(A+B)h$, we get $\varphi(x+h)=\varphi x+\frac{d\varphi x}{dx}h+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h^2+\frac{1}{2\,.\,3}\,\frac{d^3\varphi x}{dx^3}h^3+\frac{1}{2\,.\,3\,.\,4}\,\frac{d^4\varphi x}{dx^4}h^4+$ &c., (A), the law of continuation being evident.

(A) is the theorem of Taylor which we proposed to investigate; and we have obtained it without making any use of the binomial theorem. It may not be improper here to observe, that although (A) has been found on the supposition that x is indeterminate, yet it may be applied when a particular value is assigned to x, provided that none of the quantities φx, $\frac{d\varphi x}{dx}$, $\frac{d^2\varphi x}{dx^2}$, &c. becomes infinite when the particular value of x is substituted in them, and if any one of the aforesaid quantities is infinite, the true development will be given by (A) no further than to the term that first becomes infinite; and in order to obtain the development of the rest of the series, recourse must be had to particular processes of algebra. If we substitute the particular value $x=0$ in (A), and denote what φx, $\frac{d\varphi x}{dx}$, &c. become when we substitute $x=0$ in them, by $\varphi' x$, $\frac{d\varphi' x}{dx}$, &c., then if we write x for h, (which we may evidently do,) (A) will be changed to $\varphi(x)=\varphi' x+\frac{d\varphi' x}{dx}x+\frac{1}{2}\frac{d^2\varphi' x}{dx^2}x^2+$ &c., (A′), which is Maclaurin's theorem; and which is applicable always when the quantities $\varphi' x$, $\frac{d\varphi' x}{dx}$, &c. are none of them infinite; should any of these quantities be infinite, then we are to apply particular processes to obtain the development (when it is possible). If h is negative, the odd powers of h in (A) are negative, and must have the sign $-$; and similar remarks are applicable to (A′).

New Brunswick, April 21, 1843.

Art. VII.—*Notice of a portion of Dr. Dekay's Report on the Fishes of New York;* by D. Humphreys Storer, M. D.

[Read before the Boston Society of Natural History, June 21, 1843.]

When I read to this Society a few weeks since a notice of Dr. Dekay's Report on the Reptiles of New York, I promised to review his report on the fishes also of that state. Unavoidable circumstances have prevented me thus far from redeeming my pledge; but fearing I may still longer be detained from giving that careful attention to the volume referred to, which, from the munificent patronage under which it has been produced, will undoubtedly be looked upon as authority in the department upon which it treats, by scientific men abroad, if not in this country, I would at the present time offer you a few general remarks upon the descriptions of those species which are also found in the waters of Massachusetts. This I feel compelled to do, not merely to prevent the propagation of many errors which are observable throughout the pages before me, but also to enter my feeble protest against the efforts of any individual to stand on ground which is already occupied—to redescribe, under new generic and specific names, genera and species which have been known and acknowledged by scientific men.

Previous to presenting you these rough notes, I would remark, I have endeavored to persuade another, possessing more leisure and much more accurate knowledge of many of the fluviatile species contained in this volume than myself, to perform this duty, but he has refused to attempt it, and I have no alternative left me but to perform it myself. I cannot commence, however, without observing that I have left untouched the descriptions of the western and southern species, trusting the ichthyologists of those sections of our country will feel likewise called upon to point out any glaring defects which they may find to exist. I would only add, if at any future period I should ascertain that any of my observations now made are erroneous, I shall, upon this floor, correct them. Entertaining for Dr. Dekay no feelings save such as should actuate every naturalist, I shall be most happy, if I have unintentionally wronged him, to do him full justice—to prove to him, what I feel you already know, that my sole object is to establish the truth.

There is undoubtedly much valuable information in the volume before us; prepared as it is by one who in a manner represents the zoologists of the state of New York, it cannot be otherwise. It is not my purpose to dwell upon its merits; they will undoubtedly be fully appreciated by all who peruse it; I would merely point out to you some of its defects. And as I have been enabled to devote but fragments of time to its examination, I will follow the arrangement of our author.

On page 16, Dr. Dekay has thought proper to form a new genus, to contain a species which he calls *Pileoma semifasciatum;* but the description and figure of this fish place it in the genus *Etheostoma* of Rafinesque.

Boleosoma tessellatum, (p. 20.)—This species was described by me before this Society in April, 1841, and the description, accompanied by a figure, was published in the number of your Journal for January, 1842, under the name of *Etheostoma Olmstedi*, from its discoverer, Mr. Charles H. Olmsted, of East Hartford, Conn. Dr. Dekay observes, "it approaches the genus Etheostoma in the form of its head, but its opercules are said not to be scaly." In this species the preoperculum is destitute of scales, although the artist in the figure before us has pretty liberally distributed them over both the gill-covers; and the lower portion of the operculum is naked. Such appearances would almost justify Rafinesque when he formed his genus, to say the "gill-covers without scales." The genus Perca is described as having bony opercula, and the genus Labrax differs from Perca in having scaled opercules; still the upper portion of the opercula of the Perches exhibit a greater or less number of well marked scales. If Dr. Dekay is dissatisfied with Rafinesque's genus, I am unwilling he should thus presumptuously attempt to expunge my specific name; personal friendship, as well as the most common rules of scientific etiquette, prompt me to act on the defensive.*

Pomotis appendix, (p. 32.)—Inasmuch as one of the characters of the genus Pomotis is "opercule with an elongated membrane at its angle," Mitchill's specific name of the fish here described is evidently inappropriate, and it will undoubtedly be changed by some future ichthyologist who shall have an opportunity of examining it.

Uranidea quiescens, (p. 61.)—About a year and a half since, Mr. Olmsted, of whom I have previously spoken, sent me a specimen of what appeared to him to be a new species. I at once recognized it to be the *Cottus viscosus* of Haldeman. That specimen is now in your cabinet. It is, in the volume before us, described as a new species under a new genus.†

Gasterosteus quadracus, (p. 67.)—Dr. Dekay in describing this species remarks, "Dr. Storer describes a membrane attached to the ventral spine, which escaped my notice." In a living specimen now swimming before me while I am writing this no-

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* Since these remarks were penned, Mr. Olmsted assures me that he examined in the state collection at Albany, with Prof. Emmons, the gentleman who discovered it, the identical specimen which Dr. Dekay describes in his report as the *Boleosoma tessellatum*, and that it is the same species that he sent me, and which I described two years since as the *Etheostoma Olmstedi*.

† Mr. Olmsted also states that the specimen which Dr. Dekay calls *Uranidea quiescens*, was examined by him in the same collection, in company with Mr. Haldeman, who unhesitatingly pronounced it to be his *Cottus viscosus*.

tice, a beautiful scarlet colored membrane is perfectly obvious, extending from the posterior edge of the ventral spine. In five specimens preserved in spirits, this membrane is equally distinct but colorless.

Rhombus triacanthus, (p. 137.)—The original description of this species by Peck is very accurate. When speaking of the situation of the spines from which it derives its name, he says: "There is a small horizontal spine, pointing forwards, at the beginning of the dorsal fin; another at the beginning of the anal fin; and a third, arising from the sternum and pointing backwards a little before the anus." Dr. Dekay describing the fins, observes: "Pectorals long and pointed. Anterior to this fin, is a broad acutely tipped movable spine; and before this, a broad axe-shaped movable plate or spine (see figure) occupying the place of the ventrals." This is unintelligible to us. A very minute spine at the origin of the anal fin, pointing forwards, and an equally minute spine situated in front of the anus, pointing backwards, are all we have ever been able to find upon the abdomen, although the figure illustrating this species exhibits three spines in this situation. All the specimens we have examined have had the edge of the gill-covers smooth; the superior anterior angle of the operculum in the figure referred to, shows two prominent sharp spines.

Gunnellus mucronatus, (p. 153.)—Under the description of *Murænoides guttata*, Yarrell, in his "British Fishes," observes: "A specimen of a spotted gunnel from America, for which I am indebted to the kindness of Mr. Audubon, proves on comparison to be in every respect so similar to the British gunnel, that there is little doubt it is the same species." 2d. edit. Vol. I, p. 271. Dr. Dekay says of this species, "it resembles the G. vulgaris of Yarrell, but from the above description is evidently distinct from that species." I would only remark, that Yarrell, with specimens of both the foreign and native fishes before him, would be as likely to be correct as our countryman, who does not intimate that he ever saw a foreign specimen.

Lophius Americanus, (p. 162.)—Had our author told us he had compared our species with a specimen of the foreign fish Lophius piscatorius, and had he pointed out any differences which he had noticed to exist during that examination, we should undoubtedly have yielded our assent to his opinion; but until some ichthyologist has compared the two, and shown distinctions, we shall continue to believe our species to be the old Lophius piscatorius of Linnæus, Cuvier, Pennant, Donovan, Fleming, and others.

Batrachus tau, (p. 168.)—We have here an instance of the absurdity of retaining the specific name first given to a species, whether it has any or no significancy—whether it is appropriate or not. I will use the words of Dr. Dekay: "The apparently odd specific name of *tau*, given by Linnæus, is derived from the Greek name of the letter T; such a figure being produced on the head by two elevated lines in the dried specimens." In other words, this species cannot be identified when living, because its scientific (not its natural) characters do not appear until it is dead.

Ctenolabrus uninotatus, (p. 174.)—This fish, which is made a new species by Cuvier, in which he is followed by Dekay, is a mere variety of the common burgall, which may be seen by an occasional visit to the fish market.

Pimelodus catus, (p. 182.)—It is very questionable whether this specific name should be retained, when there are several species of the genus, all of which our author calls cat-fish.

Labeo gibbosus, (p. 194.) *Catostomus tuberculatus*, (p. 199.) The former of these species I introduced into my report on the fishes of Massachusetts as a new species, upon the authority of Lesueur, with his description, never having met with a specimen myself. I was exceedingly surprised to find the two species in the volume before me classed under distinct genera. My friend Mr. W. O. Ayres, of East Hartford, Ct., whose zeal in the cause of ichthyology is equalled only by his accurate knowledge of the species in his vicinity, has determined that so far from being distinct genera, they are not even distinct species, but that they really are one and the same fish.

Stilbe chrysoleucas, (p. 204.)—Dr. Dekay has marked out what he considers a new genus, and calls it Stilbe, and as a synonym of the species under this genus, he places "Leuciscus chrysoleucas, New York shiner, Storer, Fishes of Massachusetts, p. 88." Either Dr. Dekay or myself, are in error. He says there "is a short spine before the dorsal fin, which is short. Anal fin long." The identical specimen which furnished my description, which prompted Dr. Dekay to quote it as a synonym, has belonged to the cabinet of this society, since the hour it was described; it is now on your table for examination. The anal fin, Klein in his characteristics of the genus Leuciscus, calls "short." In Dr. Dekay's new genus it is "long;" the species which he describes has fourteen rays in its anal fin; the species before you has one ray less, thirteen. The difference of the length of the anal fin then, consists in the thickness of a single membranous ray. But it is said to have a "short spine before the dorsal fin;" that spine I did not notice in the description of this species in my report, because it does not exist in the specimen before you, nor have I been able to find a vestige of a spine in ten other specimens I have examined, since the publication of the volume before me. In all other respects, it is clearly a Leuciscus, and the Leuciscus chrysoleucas.

Fundulus fasciatus, (p. 216.)—This is an Hydrargyra of Lesueur. Respecting the imperfect elaboration of this genus, I must beg leave to dissent from Dr. Dekay.

Fistularia tabacaria, (p. 233.)—Our author while speaking of the spotted pipe-fish, tells us, "its geographic range is therefore from Brazil to the coast of New York, and probably still farther north; for Smith, in his History of Massachusetts, speaks of having seen two specimens of this fish from the coast of Martha's Vineyard, in 41° 30′ north latitude." I shall make no comments upon the work here referred to, but would refer Dr. Dekay to Silliman's Journal, Vol. XXXVI, for a general notice of it, while I add that Dr. Smith's collection of fishes was purchased several years since by this Society, and that in that collection, were the two specimens Dr. Dekay refers to, and that one of those specimens was described by me in my report, as the Fistularia serrata, and that that same specimen was sent by me to Dr. Dekay, and was by him also figured and described on p. 232, as the F. serrata.

Osmerus viridescens, (p. 243.)—In the synonyms of this species Dr. Dekay has arranged, "the Smelt, O. viridescens, Storer, Mass. Rep. p. 108." In my report, I catalogued this species as the "Eperlanus Artedi," and gave as a reason for so doing that "Cuvier does not acknowledge this to be distinct from the European fish, and therefore Artedi's name has the priority." I have yet to learn that this species is distinct from the European smelt.

Baione fontinalis, (p. 244.)—Dr. Dekay has thought proper to form a new genus for what is unquestionably a young brook trout—*Salmo fontinalis.*

Clupea elongata, (p. 250.)—We could have overlooked Dr. Dekay's copying our description of the common herring of Massachusetts, without giving us credit for the same, had he not unfortunately transferred an error contained in said description into his pages. Instead of the eyes being "two diameters apart," the distance between the eyes is less than the diameter of the eye.

Alosa tyrannus, (p. 258.)—Our common alewive is here catalogued under Latrobe's specific name of *tyrannus*, although it is acknowledged to be "absurd and unmeaning," because Peck's prior name of *serrata* "is a mere name without any specific character, or clue to its identity." Why is not Peck's name as appropriate here as that of *Pimelodus catus?* The specific character is evidently indefinite in both—in neither more so, however, than the name of *tyrannus* in this species; and we are not a little surprised that the New York ichthyologist had not retained the very acceptable and appropriate name of Dr. Mitchill, *vernalis*, for this species.

Amia occidentalis, (p. 269.)—Why is not this species the Amia calva, described and figured by Kirtland in Vol. III, No. 41, of the Boston Journal of Natural History, published in November, 1840, as inhabiting Lake Erie? Its size is the same as that species; the number of its fin-rays differ but slightly. Dr. Dekay, it is true, while pointing out its characters, says "tail unspotted;" but then he afterwards acknowledged, when speaking of the colors—"I can say nothing, as I had only a dried specimen."

Lota inornata, (p. 283.)—I read a description of this species to this Society, April 21, 1841, under the name of Lota brosmiana; that description, accompanied by a figure, appeared in your Journal for January, 1842.

Brosmius vulgaris, (p. 289.)—Dr. Dekay was right in doubting the identity of the American cusk with the European species. I satisfied myself a long time since, that I had committed an error in my report. As our author has never seen a specimen of Lesueur's cusk, I would here point out its differences from the foreign species. It is of a more elongated form; its dorsal and anal fins are united to the caudal fin; its eyes are oblong, and there is an immense difference in the number of their fin-rays. Dr. Dekay seems to have misunderstood my account of the color of this species. He says "the cusk of Storer, is uniform dark slate." I described, as I here state, in my report, a specimen twenty five inches in length, and weighing between three and four pounds. That small specimen was of an uniform dark slate color; but on the next page, I say "in a specimen weighing twenty pounds," (or in other words, in the more mature fish,) "the color is brown upon the back, with yellowish sides and white abdomen." This, the adult fish, you perceive is not of "an uniform dark slate."

Lumpus anglorum, (p. 305.)—In my account of the lump fish in my report, when speaking of the ridge in front of the dorsal fin, I remarked, "this ridge is formed of distinct rays, which are very visible in the dried specimen." Dr. Dekay, who must have had my report before him when he described that species, because he quotes me as having seen "one which weighed seventeen pounds," observes "the dorsal lump without any vestige of rays; at least, I found none in two which I examined." A dried specimen belonging to the cabinet of this Society lies before you, in which the rays are perfectly obvious. If Dr. Dekay had dissected a specimen, he would never have made such an error; eight rays are distinctly seen upon removing the flesh.

Anguilla tenuirostris, (p. 310.)—The description of this species is one of the most accurate in the volume before us; and had it borne its true specific name, it should have been left untouched. This species is Lesueur's "Muræna Bostoniensis," and I cannot see an effort made to erase it, without protesting against such a step. Dr. Dekay says "it may possibly be the Bostoniensis of Lesueur, as given in his brief sketch of the Murænidæ of the United States; but the description is too incomplete to enable me to determine it with certainty." How does it differ from the Bostoniensis? Lesueur's species is "above of a dark olivaceous brown, throat and abdomen grayish, region of the anus yellow ochre, towards the tail reddish." Dekay's species is "grayish olive above, yellowish beneath." Lesueur's, "jaws acute and short." Dekay's, "head small, tapering to the jaws." Lesueur's, "length about twenty four inches." Dekay's, "length one to two feet." The differences I cannot perceive; they cannot be pointed out. Dr. Dekay says, "I think it probable, but am not so certain, that the common eel of Massachusetts, noticed by Dr. Storer, may also be referred to this species." I would only remark, I never saw but one species of eel in the Boston market; that eel, Lesueur saw in the same market, and called it Bostoniensis; that same eel, Dekay has admirably described nearly thirty years subsequently, as a new species.

Syngnathus fasciatus, (p. 319.)—This species I described and figured several years since in my report, under the name of *S. Peckianus*. Dr. Dekay says his species differs from mine in the following particulars: "The body of our species, in front of the dorsal fin, is heptangular; head and rostrum proportionally longer; the greatest depth of the rostrum scarcely exceeding twice the greatest depth of the head. The dorsal fin longer than the head, measured to the posterior part of the operculum." My description reads as follows—"on each side of the anterior portion of the body are three ridges, and one passes from the neck through the middle of the abdomen to the vent;" these seven ridges, I supposed made it heptangular. The length of the head differs much in its proportions to the whole length of the fish in this species, and cannot be relied upon. Thus in three specimens lying before me at this moment, one measuring eight inches, has the head one inch long; another measuring eight and a half inches, has the head precisely as long as the former; and a third seven inches long, has the head seven eighths of an inch long. As great a difference is noticeable in the depth of the rostrum. I also, in my report, state the dorsal fin to be longer than the head.

Syngnathus viridescens, (p. 321.)—In my report, I called this species *fuscus*. The difference between Dekay's fish and that which I described is this—in his specimen its color was "dark olive green above;" in mine, the "body was of an irregular dull brown color above." You can judge whether a mere shade of color constitutes a specific character.

Lactophrys camelinus, (p. 341.)—I do not refer to this page to say aught of the genus, which appears to me however to be altogether unnecessary, but merely to observe, I regret that any scientific man should allow himself to refer to a mere popular work, particularly if that work has been publicly pronounced, and proved to be, full of errors and unworthy the slightest confidence. I would again refer him to the notice of a History of Fishes of Massachusetts, contained in Silliman's Journal for 1839.

Spinax acanthias? (p. 359.)—After quoting this species with a query, upon my authority, Dr. Dekay observes, "I am almost inclined to suspect our species distinct from that of Europe." This remark would have seemed much less singular if he had given any reasons for his suspicion. The line which he quotes from me thus in parenthesis, could have weighed nothing with him, ("under the lateral line, a series of white circular spots," Storer,) because this appearance exists also in the foreign fish. Thus Yarrell, in his description of this species, says, "the upper part of the head, body, and fins, slate gray; under parts yellowish white; *young specimens generally exhibit a few white spots.*" 2d edit. Vol. II, p. 526. I stated the specimen which I described to be "thirty four inches in length"—you perceive it was an immature fish.

Had not these remarks become already so protracted, I should have made a few observations upon the Scopelus Humboldtii, Ammodytes Americanus? Hippocampus Hudsonius? and others. I hope that some ichthyologist possessing the leisure, will furnish a more extended notice of the volume I have thus cursorily examined.

It is to be regretted that Dr. Dekay should have studiously neglected the labors of Rafinesque. With all his eccentricities, and want of method and frequent want of accuracy, there is much worthy of commendation in his "Ichthyologia Ohioensis"—much which should claim the grateful remembrance, at least, of American ichthyologists.

Art. VIII.—*On Greek Verbal Roots in English;* by Prof. J. W. Gibbs.

Greek verbal roots are liable to various changes or modifications, which disguise them more or less to the English eye, and prevent the full appreciation of the meaning of many important terms. Among these are some employed in natural science.

Modern philologists have attempted with great labor to classify these changes or reduce them to general principles, and to give a philosophical account of their origin. We propose to give their results so far as the English language is concerned.

I. The following are euphonic processes, having for their object merely to relieve the organs of speech, or to please the ear.

1. The radical vowel *a* is sometimes changed into *e*. This is effected by *attenuation* or *precession* of vowel sound. See Prof. A. Crosby: Greek Gram. § 29. Examples will occur as we proceed.

2. The radical letters, particularly a vowel and liquid, are sometimes transposed; as, *drag*on for *drac*on, 'sharp-sighted,' from √*darc*,

'to see,' by transposition √*drac; tme*sis, 'a separation,' from √*tam*, 'to cut,' by transposition and precession of vowel √*tme;* em*ble*m, 'something inlaid,' from √*bal*, 'to cast' or 'lay,' by transposition and precession of vowel √*ble.*

3. The last consonant of the root sometimes adapts or accommodates itself to the first consonant of the suffix; as, *cryp*t, 'hidden,' from √*cryb*, 'to hide,' by accommodation √*cryp;* pro*lep*sis, 'anticipation,' from √*lab*, 'to take,' by precession of vowel and accommodation √*lep; ap*sis, 'juncture,' from √*aph*, 'to join,' by accommodation √*ap; prac*tical, 'doing,' from √*prag*, 'to do,' by accommodation √*prac;* apo*plec*tic, 'striking down,' from √*plag*, 'to strike,' by precession of vowel and accommodation √*plec; hec*tic, 'habitual,' from √*hech*, 'to have,' by accommodation √*hec; dog*ma, 'an opinion,' from √*doc*, 'to seem,' by accommodation √*dog;* para*dig*m, 'an example,' from √*dic*, 'to show,' by accommodation √*dig.*

4. The last consonant of the root sometimes assimilates itself to the first letter of the suffix; as, *com*ma, 'a segment,' from √*cop*, 'to cut;' *lem*ma, 'a received truth,' from √*lab*, 'to take,' by precession of vowel √*leb.*

5. The last consonant of the root is sometimes cut off before the suffix by syncope; (1.) *n*; as, *cli*mate, 'a country in reference to its geographical position,' from √*clin*, 'to lean;' (2.) *d*; as, *phrase*, 'a speaking,' from √*phrad*, 'to say;' (3.) *th*; as, *plas*tic, 'forming,' from √*plath*, 'to form,' by dropping the final *th* and strengthening the vowel √*plas;* (4.) the digamma or *u*; as, *plei*ad, the name of a star, from √*pleu*, 'to sail,' by dropping the final *u* and then protracting the vowel *e*, √*plei.*

6. The final vowel of the root is sometimes strengthened before the suffix by an epenthetic *s*; as, *caus*tic, 'burning,' from √*cau*, 'to burn;' *schis*m, 'a division,' from √*schid*, 'to divide,' by dropping the final *d* and strengthening the vowel √*schis; spas*m, 'a contraction,' from √*spa*, 'to draw.'

II. The following changes arise from internal inflection, or change of vowel within the root itself.

1. The radical vowel *a* is sometimes protracted by internal inflection; as, sy*ste*m, 'things standing together,' from √*sta*, 'to stand,' by protraction and precession of vowel *ste.* So em*ble*m from √*bal; tme*sis from √*tam.*

2. The radical vowel *a* is sometimes changed into *o* by internal inflection; as, *od*e, 'a song,' from √*aed*, 'to sing;' *tom*e, 'a volume,' from √*tam*, 'to cut;' *ton*e, 'a note,' from √*tan*, 'to stretch;' para*bol*e, 'a comparing,' from √*bal*, 'to cast' or 'lay.'

III. The following were originally emphatic processes for expressing with more force the idea of continued action.

1. The radical vowel *a* is sometimes protracted; as, *lem*ma, 'a received truth,' from √*lab*, 'to take,' by protraction and precession of vowel √*leb; phen*omenon, 'something appearing,' from √*pha*, 'to show,' by protraction and precession of vowel, and by the addition of *n*, √*phen*.

2. The radical vowel is sometimes strengthened by a nasal; as, *tymp*anum, 'a drum,' from √*typ*, 'to strike.'

3. The radical vowel is sometimes strengthened by guna, that is, *u* is changed into *eu;* as, *zeug*ma, 'a juncture,' from √*zyg* or *zug*, 'to join;' penta*teuch* from √*tych* or *tuch*.

4. The two first letters of the root are sometimes repeated; as, syn*agog*ue, 'an assembling together,' from √*ag*, 'to lead' or 'bring.'

5. The form of the root is sometimes lengthened, (1.) by the addition of a vowel; as, *æsthe*tic, 'pertaining to rhetoric or taste,' from √*aesth*, 'to perceive;' *Gene*sis, 'origin,' from √*gen*, 'to produce;' (2.) by the addition of the consonant *n*; as, dia*phan*ous, 'shining through,' from √*pha*, 'to show;' or *t*; as, *bapt*ize, 'to immerse,' from √*baph*, 'to immerse,' by accommodation and strengthening √*bapt;* (3.) by the addition of a vowel and consonant; as, *auxe*sis, 'increase,' from √*aug*, 'to increase.'

LIST OF GREEK VERBAL ROOTS IN ENGLISH.

1. √*a*, (Gr. √ἀ, =Sansc. √*wâ*,) breathing; whence *air* for *aer*, (√*a*+ suff. *er*,) the fluid which we breathe; *aerial*, (√*a* + suff. *er* + suff. *i*+Lat. suff. *al*,) pertaining to the air.

2. √*aed*, (Gr. √ᾀδ,) by internal inflection *oed*, (Gr. ᾠδ,) singing; whence *ode*, (√*oed* +*e* mute,) a song; *tragedy*, (√*trag* + √*oed* + suff. *y*,) literally a goat-song.

3. √*aesth*, (Gr. √αἰσθ,) with lengthened form *aesthe*, (Gr. αἰσθε,) perceiving; whence *æsthetic*, (√*aesthe*+double suff. *tic*,) relating to perception, particularly of the beautiful.

4. √*aeth*, (Gr. √αἰθ,) shining; whence *ether*, (√*aeth*+suff. *er*,) the shining upper air.

5. √*ag*, (Gr. √ἀγ, =Lat. √*ag*,) by internal inflection *og*, (Gr. ὠγ,) leading or bringing; whence *paragoge*, (pref. *para*+√*ag* repeated+ suff. *e*,) a bringing or putting on of a letter or syllable to the end of a word; *synagogue*, (pref. *syn*+√*ag* repeated+*ue* mute,) a congregation of Jews; *demagogue*, (√*dem*+√*ag* repeated +*ue* mute,) a people-leader.

6. √*aph*, (Gr. √ἀφ or ἁφ, =Lat. √*ap*,) joining; whence *apsis*, plur. *apsides*, (√*aph*+suff. *sid*,) literally a juncture.

7. √*arch*, (Gr. √ἀρχ,) beginning, leading; whence *arch*, adj. chief; *archon*, (√*arch*+suff. *on*,) a Grecian magistrate; *monarch*, (√*mon* +

√*arch*,) one ruling alone; *archetype*, (√*arch* with union-vowel *e*+√*typ*+*e* mute,) first impressed, original; *architect*, (√*arch* with union-vowel *i*+√*tec*+suff. *t*,) a chief builder; *archduke*, (√*arch*+√*duc* with *e* mute,) a chief duke.

8. √*aug*, (Gr. √αὐγ, =Lat. √*aug*, Eng. *eke*,) with lengthened form *auxe*, (Gr. αὐξε,) increasing; whence *auxesis*, (√*auxe*+suff. *sis*,) increase, as the name of a rhetorical figure.

9. √*ba*, (Gr. √βα,) going; whence *basis* or *base*, (√*ba*+suff. *sis* or *se*,) a stepping, that on which any thing rests.

10. √*bal*, (Gr. √βαλ, =Lat. √*bal* in *balister*,) by internal inflection *bol*, (Gr. βολ,) by transposition and lengthening of the vowel *ble*, (Gr. βλη,) casting or laying; whence *symbol*, (pref. *syn*+√*bol*,) what by comparison suggests something else; *parabole*, (pref. *para*+√*bol* +suff. *e*,) a comparing or laying along side; *parable*, (the same form more fully Anglicized,) a species of extended comparison; *emblem*, (pref. *en*+√*ble*+suff. *m*,) something inlaid.

11. √*baph*, (Gr. √βαφ,) with final radical strengthened by *t*, *bapt*, (Gr. βαπτ,) dipping; whence *baptize*, (√*bapt*+suff. *ize*,) to administer the sacrament of baptism.

12. √*bo*, (Gr. √βο,) feeding; whence *botany*, (√*bo*+triple suff. *tany*,) the science of plants; *proboscis*, (pref. *pro*+√*bo*+suff. *sc*+suff. *is*,) literally what feeds before.

13. √*camp*, (Gr. √καμπ, = Sansc. √*kamp*,) with final radical strengthened by *t*, *campt*, (Gr. καμπτ,) bending; whence *anacamptic*, (pref. *ana*+√*campt*+suff. *ic*,) reflected.

14. √*cau*, (Gr. √καυ,) burning; whence *caustic*, (√*cau* strengthened by *s*+double suff. *tic*,) burning; *cautery*, (√*cau*+suff. *tery*,) an instrument for burning; *holocaust*, (√*hol* with union-vowel *o*+√*cau* strengthened by *s*+suff. *t*,) an offering which was wholly burnt.

15. √*chra*, (Gr. √χρα,) by lengthening the vowel *chre*, (Gr. χρη,) using; whence *catachresis*, (pref. *cata*+√*chre*+suff. *sis*,) abuse, as the name of a rhetorical figure.

16. √*chri*, (Gr. √χρι,) anointing; whence *chrism*, (√*chri* strengthened by *s*+suff. *m*,) unguent; *Christ*, (√*chri* strengthened by *s*+suff. *t*,) literally the anointed.

17. √*chro*, (Gr. χρο,) coloring; whence *chrome*, (√*chro*+suff. *me*,) a metal which affords beautiful colors.

Note.—The three preceding numbers, √*chra*, √*chri*, and √*chro*, are regarded as collateral roots, all signifying primarily *to touch the surface*.

18. √*chy*, (Gr. √χυ,) pouring; whence *parenchyma*, (pref. *para*+pref. *en*+√*chy*+suff. *ma*,) the spungy substance of the viscera.

19. √*cla*, (Gr. √κλα,) breaking; whence *iconoclast*, (√*icon* with union-vowel *o*+√*cla* strengthened by *s*+suff. *t*,) an image-breaker.

20. √*clin*, (Gr. √κλιν, =Lat. √*clin*, Eng. *lean*,) leaning; whence *clinic*, (√*clin*+suff. *ic*,) pertaining to a bed; *climate* or *clime*, (√*clin* +suff. *mate* or *me*,) a country in reference to its geographical position; *enclitic*, (pref. *en*+√*clin*+double suff. *tic*,) inclining.

21. √*cap* or *cop*, (Gr. √καπ or κοπ,) cutting; *capon*, (√*cap*+suff. *on*,) the name of a bird; *comma*, (√*cop*+suff. *ma*,) a segment; *apocope*, (pref. *apo*+√*cop*+suff. *e*,) a cutting off, as the name of a grammatical figure.

22. √*cra*, (Gr. √κρα,) mixing; whence *crasis*, (√*cra*+suff. *sis*,) a mixing, as the name of a grammatical figure.

23. √*cri*, (Gr. √κρι, =Sansc. √*krî*, Lat. √*kre* or *kri*,) sifting or separating; whence *crisis*, (√*cri*+suff. *sis*,) a separation, decision; *critic*, (√*cri*+double suff. *tic*,) pertaining to judging.

24. √*cryb*, (Gr. κρυβ,) with final radical strengthened by *t*, *crypt*, (Gr. κρυπτ,) hiding; whence *crypt*, (√*crypt*,) hidden, a vault; *apocrypha*, (pref. *apo*+√*cryb*+suff. *a*,) hidden, applied to books which were laid up at home and not read in public.

25. √*cy*, (Gr. √κυ,) containing; whence *cyst*, (√*cy* strengthened by *s*+suff. *t*,) a bag or tunic containing morbid matter in animal bodies; *cyma* or *cyme*, (√*cy*+suff. *ma* or *me*,) literally something contained.

26. √*darc*, (Gr. √δαρκ, =Sansc. √*dric*,) by transposition *drac*, (Gr. δρακ,) seeing; whence *dragon*, (√*drag* for *drac*+suff. *on*,) sharp-sighted, the name of an animal.

27. √*de*, (Gr. √δε,) binding; whence *anademe*, (pref. *ana*+√*de* +suff. *me*,) a chaplet of flowers; *diadem*, (pref. *dia*+√*de*+suff. *m*,) a head-band worn by kings.

28. √*dem*, (Gr. √δεμ,) by internal inflection *dom*, (Gr. δομ,) building; whence *dome*, (√*dom*+*e* mute,) a house.

29. √*dic*, (Gr. δεικ, =Sansc. *diç*, Lat. *dic*, Eng. *teach*,) showing; whence *paradigm*, (pref. *para*+√*dic*+suff. *m*,) an example; *apodictic*, (pref. *apo*+√*dic*+double suff. *tic*,) demonstrative.

30. √*do*, (Gr. √δο, =Sansc. *dâ*, Lat. *da*,) giving; whence *dose*, (√*do*+suff. *se*,) quantity given; *antidote*, (pref. *anti*+√*do*+suff. *te*,) a counteracting medicine; *apodosis*, (pref. *apo*+√*do*+suff. *sis*,) the application of a similitude.

31. √*doc*, (Gr. √δοκ, =Lat. *doc*,) thinking or seeming; whence *dogma*, (√*doc*+suff. *ma*,) an opinion.

32. √*dra*, (Gr. √δρα,) acting; whence *drama*, (√*dra*+suff. *ma*,) an action labored after the rules of art.

33. √*dram*, (Gr. √δραμ,) by internal inflection *drom*, (Gr. δρομ,) running; whence *dromedary*, (√*drom*+suff. *ad*+Lat. suff. *ary*,) a species of camel; *syndrome*, (pref. *syn*+√*drom*+*e* mute,) a concur-

rence; *hippodrome*, (√*hipp* with union-vowel *o*+√*drom*+*e* mute,) a place for running horses.

34. √*ep*, (Gr. √*Fεπ*, =Sansc. √*watsh*, Lat. √*voc*,) saying; whence *epic*, (√*ep*+suff. *ic*,) narrative.

35. √*erg*, (Gr. √*Fεϱγ*, = Eng. *work*,) by internal inflection *org*, (Gr. *Foϱγ*,) working; whence *organ*, (√*org*+suff. *an*,) an instrument; *energy*, (pref. *en*+√*erg*+suff. *y*,) efficacy; *liturgy*, (√*lit* with union-vowel *o*+√*erg*+suff. *y*,) public service.

36. √*eth*, (Gr. √*ἐϑ*,) to be wont; whence *ethic*, (√*eth*+suff. *ic*,) relating to morals.

37. √*gam*, (Gr. √*γαμ*,) marrying; whence *polygamy*, (√*poly*+√*gam*+suff. *y*,) marriage with several.

38. √*gen*, (Gr. √*γεν*, =Sansc. *dzhan*, Lat. *gen* and *gna* or *na*,) by internal inflection *gon*, (Gr. *γον*,) and with lengthened form *gene*, (Gr. *γενε*,) producing; whence *oxygen*, (√*oxy*+√*gen*,) acid-making; *cosmogony*, (√*cosm* with union-vowel *o*+√*gon*+suff. *y*,) the origin of the world; *Genesis*, (√*gene*+suff. *sis*,) origin, the name of the first book of Moses.

39. √*glyph*, (Gr. √*γλυφ*,) cutting in; whence *glyph*, a cavity intended as an ornament; *hieroglyph*, (√*ἱεϱ* with union-vowel *o*+√*glyph*,) a sacred character.

40. √*gno*, (Gr. √*γνο*, =Sansc. √*dzhnâ*, Lat. √*gno* or *no*, Eng. *know*,) knowing; whence *gnome*, (√*gno*+suff. *me*,) something known, a maxim; *gnomon*, (√*gno*+suff. *mon*,) knowing, the style or pin of a dial; *gnostic*, (√*gno* strengthened by *s*+double suff. *tic*,) knowing, belonging to a sect of oriental philosophers.

41. √*graph*, (Gr. √*γϱαφ*, = Lat. √*scrib*, Eng. *grave* and *scrape*,) digging; whence *graphic*, (√*graph*+suff. *ic*,) descriptive; *telegraph*, (√*tele*+√*graph*,) an instrument for communicating to a distance; *hagiographa*, (√*hagi* with union-vowel *o*+√*graph*+suff. *a*,) sacred writings.

42. (Gr. √*ἁϱπ*, =Lat. √*rap*,) seizing; whence *harpy*, (√*harp*+suff. *y*,) a fabulous monster; *harpoon*, (√*harp*+French suff. *oon*,) a harping-iron.

43. √*hech* or *sech*, (Gr. √*ἑχ* or *σεχ*,) by transposition *sche*, (Gr. *σχε*,) having; whence *hectic*, (√*hech*+double suff. *tic*,) habitual; *cachexy*, (√*cac*+√*hech*+suff. *sy*,) ill habit; *scheme*, (√*sche*+suff. *me*,) a plan.

44. √*id*, (Gr. √*Fιδ*, =Sansc. √*wid*, Lat. √*vid*, Eng. *wit*,) with lengthened form *ide*, (Gr. *Fιδε*,) seeing; whence *idea*, (√*ide*+suff. *a*,) an image.

45. √*lab*, (Gr. √*λαβ*, =Sansc. √*labh*,) by lengthening the radical vowel *leb*, (Gr. *ληβ*,) taking; whence *astrolabe*, (√*astr* with union-

vowel *o*+$\sqrt{}$*lab*+*e* mute,) literally a star-taker; *prolepsis*, (pref. *pro* +$\sqrt{}$*leb*+suff. *sis*,) anticipation; *lemma*, (*leb*+suff. *ma*,) a received truth.

46. $\sqrt{}$*lamp*, (Gr. λαμπ,) shining; whence *lamp*, a light made with oil and a wick.

47. $\sqrt{}$*leg*, (Gr. $\sqrt{}$λεγ, =Lat. $\sqrt{}$*leg*,) by internal inflection *log*, (Gr. λογ,) gathering, speaking; whence *prolegomena*, (pref. *pro*+$\sqrt{}$*leg*+ suff. *omena*,) preliminary observations; *lexicon*, ($\sqrt{}$*leg*+double suff. *sicon*,) a dictionary; *dialogue*, (pref. *dia*+$\sqrt{}$*log*+*ue* mute,) a conversation.

48. $\sqrt{}$*lip*, (Gr. $\sqrt{}$λιπ, =Lat. *liqu*,) leaving, failing; whence *ellipsis*, (pref. *en*+$\sqrt{}$*lip*+suff. *sis*,) an omission; *eclipse*, (pref. *ec*+$\sqrt{}$*lip*+ suff. *se*,) literally a failure.

49. $\sqrt{}$*lit*, (Gr. $\sqrt{}$λιτ,) supplicating; whence *litany*, ($\sqrt{}$*lit*+double suff. *any*,) a form of supplication.

50. $\sqrt{}$*ly*, (Gr. $\sqrt{}$λυ, =Lat. $\sqrt{}$*lu* in *solvo*,) loosing; whence *analysis*, (pref. *ana*+$\sqrt{}$*ly*+suff. *sis*,) a resolving.

51. $\sqrt{}$*mach*, (Gr. $\sqrt{}$μαχ,) fighting; whence *naumachy*, ($\sqrt{}$*nau*+ $\sqrt{}$*mach*+suff. *y*,) a fight of ships; *monomachy*, ($\sqrt{}$*mon* with union-vowel *o*+$\sqrt{}$*mach*+suff. *y*,) a single fight.

52. $\sqrt{}$*man*, (Gr. $\sqrt{}$μαν,) to be mad; whence *mania*, ($\sqrt{}$*man*+suff. *ia*,) madness.

53. $\sqrt{}$*math*, (Gr. $\sqrt{}$μαθ,) with lengthened form *mathe*, (Gr. μαθε,) learning; whence *philomath*, ($\sqrt{}$*phil* with union-vowel *o*+$\sqrt{}$*math*,) a lover of learning; *chrestomathy*, ($\sqrt{}$*chrest* with union-vowel *o*+$\sqrt{}$*math* +suff. *y*,) useful or necessary learning; *mathematical*, ($\sqrt{}$*mathe*+ double suff. *matic*+Lat. suff. *al*,) pertaining to the science of quantity.

54. $\sqrt{}$*nem*, *nom*, (Gr. $\sqrt{}$νεμ, νομ,) pasturing, ruling; whence *nomad*; *antinomian*; *astronomy*; *economy*.

55. $\sqrt{}$*op*, (Gr. $\sqrt{}$ὀπ,) seeing, whence *optic*; *synopsis*; *autopsy*.

56. $\sqrt{}$*path*, (Gr. $\sqrt{}$παθ, =Lat. $\sqrt{}$*pat*,) suffering; whence *pathos*; *pathic*; *apathy*.

57. $\sqrt{}$*pau*, (Gr. $\sqrt{}$παυ,) ceasing; whence *pause*.

58. $\sqrt{}$*pemp*, *pomp*, (Gr. $\sqrt{}$πεμπ, πομπ,) sending; whence *pomp*, literally a sending under escort.

59. $\sqrt{}$*pen*, *pon*, (Gr. $\sqrt{}$πεν, πον, =Lat. $\sqrt{}$*pen* in *penury*,) laboring; whence *geoponic*, laboring the earth.

60. $\sqrt{}$*pet*, (Gr. $\sqrt{}$πετ, =Sansc. $\sqrt{}$*pat*, Lat. $\sqrt{}$*pet*,) by syncope of the radical vowel and extension *pto*, (Gr. πτο,) falling; whence *symptom*.

61. $\sqrt{}$*pet*, (Gr. $\sqrt{}$πετ, =Lat. $\sqrt{}$*pat*,) spreading out; whence *petal*.

62. $\sqrt{}$*pha*, *phe*, (Gr. $\sqrt{}$φα, φη, =Lat. $\sqrt{}$*fa*,) speaking; whence *prophet*; *euphemism*.

63. √*pha*, (Gr. √φα, =Sansc. √*bhâ*,) with lengthened form *phan*, *phaen*, (Gr. φαν, φαιν,) appearing; whence *phase; diaphanous; phenomenon.*

64. √*pher*, *phor*, (Gr. √φερ, φορ, =Sansc. √*bhri*, Lat. √*fer*, Eng. *bear*,) bearing; whence *periphery; metaphor; phosphor.*

65. √*phil*, (Gr. √φιλ,) loving; whence *philter; philomel.*

66. √*phleg*, *phlog*, (Gr. √φλεγ, φλογ, =Sansc. √*bhrâdzh*, Lat. *flag*, and *fulg*, Eng. *blink*,) shining, burning; whence *phlegm; phlogiston.*

67. √*phrad*, (Gr. √φραδ,) saying; whence *phrase; periphrasis; paraphrase.*

68. √*phrag*, (Gr. √φραγ,) enclosing; whence *diaphragm.*

69. √*phtheg*, (Gr. √φθεγγ,) saying; whence *apophthegm.*

70. √*phthi*, (Gr. √φθι,) wasting away; whence *phthisis.*

71. √*phy*, (Gr. √φυ, =Sansc. √*bhû*, Lat. √*fu*, Eng. *be*,) being born; whence *physic; symphysis; neophyte.*

72. √*plac*, *ploc*, (Gr. √πλακ, πλοκ,) folding; whence *epiploce*, implication, a figure of rhetoric.

73. √*plag*, *pleg*, (Gr. √πλαγ, πληγ,) striking; whence *apoplexy; hemiplexy.*

74. √*plath*, (Gr. √πλαθ,) forming; whence *plastic.*

75. √*pleu*, (Gr. √πλευ,) sailing; whence *pleiad.*

76. √*pneu*, (Gr. √πνευ,) breathing; whence *pneumatic; pneumonic.*

77. √*po*, (Gr. √πο, =Sansc. *pâ*, Lat. √*po* in *potus*,) drinking; whence *symposium.*

78. √*poe*, (Gr. √ποι,) with lengthened form *poee*, (Gr. ποιε,) making; whence *poet*; *poem; epopee.*

79. √*prag*, (Gr. √πραγ,) doing; whence *pragmatic*; *praxis.*

80. √*pri*, (Gr. √πρι,) sawing; whence *prism.*

81. √*psa*, *pse*, (Gr. √ψα, ψη,) rubbing; whence *palimpsest*, an old parchment rubbed over or prepared anew.

82. √*psal*, (Gr. √ψαλ,) playing on an instrument; whence *psalm; psaltery.*

83. √*pty*, (Gr. √πτυ, =Lat. √*pitu* in *pituita*, Eng. *spit*,) spitting; whence *ptysmagogue.*

84. √*rheu*, *rhe*, (Gr. ῥευ, ῥε, =Sansc. √*sru*, Lat. √*ru* in *rivus*,) flowing; whence *rheum; rhetoric; diarrhea; catarrh.*

85. √*scad*, *scand*, (Gr. √σκαδ, σκανδ, =Sansc. √*scand*, Lat. √*scand*,) mounting; whence *scandal.*

86. √*scep*, *scop*, (Gr. √σκεπ, σκοπ, =Sansc. √*paç*, Lat. √*spec*, Eng. *spy*,) seeing; whence *scope; episcopal; bishop.*

87. √*schid*, (Gr. √σχιδ, =Sansc. √*tshhid*, Lat. √*scind*, Eng. *sheathe*,) dividing; whence *schism.*

88. √*spa*, (Gr. √σπα, =Lat. √*spa* in *spatium*,) drawing; whence *spasm*.

89. √*spar*, (Gr. σπαϱ, =Sansc. √*sphar*, Lat. √*spar* in *spargo*,) scattering; whence *sperm*.

90. √*spend*, *spond*, (Gr. √σπενδ, σπονδ,) pouring out; whence *spondee*.

91. √*sta*, *ste*, (Gr. √στα, στη, =Sansc. √*sthâ*, Lat. *sta*, Eng. *stay*,) standing; whence *apostate; metastasis; apostasy; system.*

92. √*stal*, *stol*, (Gr. √σταλ, στολ, =Eng. *stall*,) placing, sending; whence *peristaltic; diastole; apostolic; apostle; epistle.*

93. √*steph*, (Gr. √στεφ,) crowning; whence *Stephen*, a proper name.

94. √*stig*, (Gr. √στιγ, =Lat. *stig* in *instigate*, Eng. *stick*,) marking; whence *stigma*.

95. √*streph*, *stroph*, (Gr. √στϱεφ, στϱοφ,) turning; whence *strophe*.

96. √*tag*, (Gr. ταγ,) arranging; whence *tactic; syntax.*

97. √*tam*, *tom*, (Gr. √ταμ, τομ, =Lat. √*tem*,) by transposition and lengthening of radical vowel *tme*, (Gr. τμη,) cutting; whence *tome; atom; anatomy; epitome; tmesis.*

98. √*tan*, *ton*, (Gr. ταν, τον, =Sansc. √*tan*, Lat. √*ten*, Eng. *thin*,) stretching; whence *tone; tonic; hypotenuse.*

99. √*thaph*, (Gr. √θαφ, in θάπτω, =Sansc. √*tap*, Lat. √*tap*,) burying; whence *cenotaph*.

100. √*thraph*, *throph*, (√θϱαφ, θϱοφ,) nourishing; whence *atrophy*.

101. √*the*, (Gr. √θε, =Sansc. √*dhâ*, Lat. √*do* in *condo*,) placing; whence *thesis; theme; anathema; antithetic.*

102. √*thel*, (Gr. √θελ,) willing; whence *monothelite*.

103. √*ther*, (Gr. √θεϱ, =Lat. *ferv*,) to be warm; whence *thermal; anthracite.*

104. √*ti*, (Gr. √τι,) honoring; whence *Titus*, *Timon*, proper names.

105. √*trap*, *trop*, (Gr. √τϱαπ, τϱοπ,) turning; whence *trope*.

106. √*tych*, (Gr. √τυχ,) with guna of radical vowel *teuch*, (Gr. τευχ,) making; whence *pentateuch*.

107. √*typ*, (Gr. √τυπ, =Eng. *tap*,) striking; whence *type*, *tympanum*.

108. √*za*, *zo*, (Gr. √ζα, ζο, =Sansc. √*dzhiw*, Lat. √*viv*, Eng. *quick*,) living; whence *azote; zoology.*

109. √*ze*, *zy*, (Gr. √ζε, ζυ,) *boiling;* whence *apozem; zeolite; zumic; azyme.*

110. √*zo*, (Gr. √ζο, =Sansc. √*yu*, Lat. √*ju* in *jus*,) binding, girding; whence *zone*.

111. √*zyg*, (Gr. √ζυγ, =Sansc. √*yudzh*, Lat. √*jug*, Eng. *yoke*,) with guna of radical vowel *zeug*, (Gr. √ζευγ,) joining; whence *syzygy; zeugma.*

Art. IX.—*Remarks on Tides and the Prevailing Currents of the Ocean and Atmosphere;* by W. C. Redfield.

[Read before the American Philosophical Society at their centennial meeting, May 27th, 1843.]

The summary remarks and suggestions which follow, relate chiefly to the systematic currents of the ocean and the atmosphere; and were drawn up on short notice in the summer of 1838 at the request of a gentleman attached to the U. S. Exploring Expedition,* and were designed for reference, correction, and verification, by the scientific observers of the Expedition.

The views thus submitted I had derived, in previous years, from somewhat extensive examinations of the observations which had been made by voyagers and travellers in different seas and countries, and they are offered without an array of particular references to the numerous facts and observations from which they have been derived. This course was adopted, on that occasion, as being the least laborious, and because it was the undoubted design of the observers of the expedition to subject all general views and theories to the test of direct observations.

As a substitute, however, for those specific observations from which my results had been drawn, I delineated on maps and charts which were furnished me for the purpose, not only the general outlines or courses of the systems of general winds and currents which I had found to prevail in the Pacific Ocean and other seas, but also, some of the particular observations by which in my view, the existence of these currents had been established. These maps, seven in number, were lost by the unfortunate wreck of the Peacock, near the mouth of the Columbia River.

It is not my design to bestow further labor upon this extensive subject till the observations and results of the expedition shall have been published. But as observations on meteorology and the cognate branches of terrestrial physics may have been more limited in the expedition than I could have had reason to apprehend, particularly in the Atlantic, I venture now to lay before the Society my unfinished memoir of that period, even without those specific delineations which would have been afforded by the lost maps, which I have not yet attempted to reconstruct.

* James D. Dana Esq., geologist of the expedition.

I proceed now to the remarks which were addressed to the gentlemen of the expedition.

The preparation and departure of the Expedition fitted out by the goverment of the United States for the scientific examination of distant seas and countries, naturally awakens feelings of interest and expectation in the American public, as well as among the friends of science, in this and other countries. In such feelings the writer of these remarks fully participates, and the opportunities for useful observation which the Expedition is likely to afford, on various natural phenomena which have engaged his attention, may perhaps justify the following statements and suggestions, addressed to those who are to conduct the movements and perform the scientific labors of the expedition.

The instructions which have been drawn up by Sir J. F. W. Herschel, for observations in meteorology, and by M. Arago, for the discovery vessel, the *Bonite*, together with the valuable reports which have been made to the U. S. Naval Lyceum by its committee and other distinguished individuals, with direct reference to this expedition, have presented many important topics of investigation.* There are still, however, some points of interest and importance which seem to deserve more particular notice.

Indeed, the subjects of natural science which invite the investigation of the Expedition, are too numerous and important to be easily exhausted.

OF TIDES.

The valuable labors of Prof. Whewell and Mr. Lubbock have greatly enlarged our knowledge of the tides; owing chiefly to the fact that these gentlemen have followed the method of direct induction from actual observations, made at different localities. To the directions given by Prof. Whewell for obtaining the correct establishment, or true time of high water at the full and change of the moon, nothing more need be added.

It is a question of some importance, however, if it be not already determined, whether the main tidal wave of the North Atlantic be derived directly from the great Southern Ocean, as Prof. Whewell supposes, or, whether it mainly follows a circuit of revolution, north of the equator, around an elongated axis or

* See Naval Magazine for January, 1837, Vol II, p. 64, *et seq.*

neutral position, situated in mid ocean, somewhere between 18° and 26° north latitude, as had been suggested at an earlier period.*

A like question arises in regard to the tide-waves of both the North and South Pacific. The inquiry is therefore presented, whether the tidal wave in the North Pacific ocean does not move in a circuit, around a central position not greatly distant from the Sandwich Islands, the wave moving westerly in mid ocean in the intertropical or equatorial latitudes and easterly in the higher latitudes; and whether the tide-wave of the South Pacific does not follow a like course, around a central point or position at or near Tahita or the Society Islands. If this view of the course of the tide-waves should be sustained by observations in the Pacific, the tide-wave on the western coast of North America will be found moving southeastward, and together with the counter-wave from the South Pacific, might fully account for the extraordinary convergence and height of the tides in the Bay of Panama. Such a system of revolution in the tidal waves of the great oceans may account, also, for the absence of any considerable tides at the Sandwich and Society Islands, and at the Windward Islands of the Antilles.

Such circuits of revolution in the tides, would bear some analogy to those which, as I apprehend, are exhibited in the system of *currents* in the several oceans, as well as in the system of general winds, which likewise prevail. These systems of revolution and compensation, in the currents of the aqueous and aerial oceans, I have ventured to refer directly to the law of gravitation, as connected with unstable equilibrium and with the rotary and orbital movements of the several zones and meridians of the earth's surface.†

As connected with the enquiry on tides, it is important to ascertain the direction of the main stream of flood tide in the *offing*, at the several islands and prominent headlands which are most exempt from the local influences of reefs and shallows.

CURRENTS OF THE ATLANTIC.

The great system of aqueous circulation, which appears to be developed under various modifications in the several oceans on

* See note in Silliman's Journal for Oct. 1833, Vol xxv, p. 132.

† The law of equilibrium in our system, I apprehend, is a law of motion, not of rest.

both sides of the equator, has been glanced at in the foregoing remarks on tides. One of the most active, if not the best known current of this oceanic system, is the Gulf Stream of the North Atlantic. It appears to be established that a main portion of the Gulf Stream moves from the American coast towards the Azores and the Canary Islands, and thence along the coast of North Africa, turning westward till it again coincides with the equatorial current in its course towards the Caribbean Sea. This great circuit of the ocean current is found to coincide, mainly, with that which is also performed by the general winds in the basin of the North Atlantic. For the trade winds, on leaving the tropical latitudes, pass eastwardly through the temperate zone, but in a more irregular manner, sweeping around the track of ocean known as the *grassy sea* and the belt of summer calms, which lies a few degrees north of the tropic, known to navigators as the *horse latitudes*. It is in this extratropical region of calms that the major axis of this great elliptical circuit of general winds appears to lie. It is this calm region that separates the general westerly winds of the higher latitudes from the trade winds of which they are the counterpart; and it is chiefly these westerly winds of the higher latitudes which, in the performance of their great circuit of revolution, are again merged in the regular trade winds.* But let us return to the consideration of the more limited currents which prevail in the ocean.

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* I may add, that so far as the writer is concerned, the first exhibition of this view is found in my communication published in Silliman's Journal for April, 1831, Vol. xx, p. 50. In this instance, however, I have ascribed the currents of the ocean solely to the force of the winds, in compliance with the common theory; a view which I soon after found reason to abandon. The outlines of the great systems of horizontal revolution in the winds I have also sketched in my summary of "Facts in Meteorology," which appeared in Silliman's Journal for October, 1833, Vol. xxv, pp. 122–135. Previous to this period, I had examined the journals of whalers who had cruised on the "off shore ground" of the North Pacific, in that belt of calms and light winds near the latitude of 30°, which is the favorite resort of the sperm whales in that sea, and which corresponds to the so called "horse latitudes" of the Atlantic. From this and other like evidence I had arrived at the conclusions which I now maintain.

Sir John F. W. Herschel maintains the connection or continuity of the trades with the prevailing westerly winds of higher latitudes; and refers to the well reasoned explanations of Capt. Basil Hall, based on the common theory. He also adds an important suggestion on the velocity of winds which subside from a higher position in the atmosphere, and which may serve to explain the steady violence which sometimes pertains to westerly gales in the United States and on the North Atlantic.—*Treatise on Astronomy, section* 200 *and ante.*

Having noticed that portion of the Gulf Stream which, on passing the bank of Newfoundland, moves towards the Azores and the African coast, we will now follow that considerable portion of the Stream which is found to pass towards the western coast of the British islands and along the coast of Norway, till it enters the polar basin. From this frozen region it again emerges in the great polar current, covered with floating ice, which, skirting the coasts of Labrador and Newfoundland, falls in with the Gulf Stream at the southern extremity of the Grand Bank, and now becomes, mainly, a subaqueous current, the deeper portion of which can be traced only by its propelling effect on the deeply immersed icebergs, which it forces athwart the warm tropical stream, till they become dissolved by the higher temperature of the latter.

Observations of the temperature made in sounding at various depths in the Gulf Stream, and particularly in the region where it overruns or crosses the polar current, would be of high interest, and of great value in estimating the dynamics of the ocean currents.

As connected with the foregoing outline of the main system of superficial currents in the North Atlantic, I propose now a particular inquiry, relating to a single branch of this system of ocean streams, which perhaps may serve to show the origin or character of some currents which pursue opposite directions in other oceans. *From what source, then, is that southwesterly current derived which commonly prevails along the coast of the United States, in the direction which is opposite to the Gulf Stream?*

I am aware that this is usually considered by seamen as an eddy current, derived from the Gulf Stream; but from this view I am compelled to dissent. For, in the first place, this current never assumes the gyrating form of an eddy; but continues its course, when unobstructed by gales, in a direction which is generally parallel to the coast. But, secondly, in case this current be derived from the Gulf Stream, it must necessarily partake of the same elevated temperature; whereas, the reduction of temperature which occurs on crossing the northwestern limit of the Gulf Stream is most remarkable, and is almost without a parallel in the Atlantic, except in the immediate vicinity of ice.

It appears vain to allege the proximity of soundings or shallows as explaining this extraordinary change of temperature, for this cannot avail if the waters of the counter current be derived from the Gulf Stream, to say nothing of the erroneous character of the position here noticed.

From the evidence which is afforded by numerous facts and observations, it appears that the current in question is neither more nor less than a more sluggish prolongation of the polar or Labrador current, which sweeps along the northeastern shores of this continent and the island of Newfoundland. And this current, if I mistake not, may be directly traced in its gradations of temperature, by the thermometer, from off the southern coasts of Newfoundland and Nova Scotia through the entire distance to Cape Hatteras, if not to Florida.

An eddy current offsetting from the Gulf Stream, would no where be so likely to be met with as at the point of intersection of this stream with the extremity of the Grand Bank of Newfoundland, and sweeping from thence upon the southern shores of the island of that name; and yet, the harbor of St. John's on the southern coast of Newfoundland is known to have continued ice-bound in 1831 so late as the month of June, although in the latitude of Paris. This fact is a convincing proof of the unimpeded continuation of the polar current to the southward, in this region, notwithstanding the near proximity of the Gulf Stream.

That Col. Jonathan Williams and others should have ascribed the reduced temperature of the ocean near our shores simply to the effect of shoals or shallow soundings, need not excite our surprise, as such striking reductions of temperature are found on the Great Bank of Newfoundland, and on that of the Lagullas, off the Cape of Good Hope, and while so little has been known of the system of ocean currents, and the proximate origin and courses of the colder streams of this system. And it is well known, that the low temperature of the sea on these banks and shallows has been ascribed to the effects of radiation. But, if I mistake not, it has been shown that a non-luminous body is incapable of radiation through water; and should this be otherwise, any possible effect of this kind is wholly overborne by the cold of the great polar currents, which constantly traverse the banks and shoals referred to.

If I am correct in this view, it is the reduced temperature of the currents from the polar regions, or, from contiguous ocean depths, which has led Williams, Davy and others to support the erroneous, or at least very questionable generalization, which ascribes a reduced temperature to the sea on all banks and shallows. If the ocean was devoid of currents, I think we might expect an *increase* of temperature on shoals in summer, or in warm latitudes, and a *reduction* of temperature in winter. A friend who made a full set of observations in crossing the Atlantic, informs me that on arriving at soundings in the English Channel, he found an increase of 2° in the temperature of the waters.*

Perhaps I may be allowed to refer, for a moment, to the geological agencies of the polar currents. It is well known that extensive fields and packs of ice, including many icebergs of vast magnitude, are constantly carried by the polar currents towards the lower latitudes. On reaching certain regions, such as the banks of Newfoundland and the Lagullas of Southern Africa, the ice is brought into proximity or contact with the warm counter-currents of the system, which flow from the torrid zone, where the ice is soon dissolved. The numerous masses of earth, rocks, beach bowlders, and sedimentary matter, which are borne by the ice in great profusion from the cliffs, the shores and the sea-bottom of the Arctic regions, and probably also from the Antarctic, are thus added continually to the vast submarine deposits which there accumulate. May not the continuance of this transporting process, through a long series of ages, be deemed sufficient to account for the existence and present extent of the great banks referred to; without particular reference to the evidence of successive elevations and subsidences, in extensive areas of the earth's crust?

CURRENTS OF THE SOUTHERN AND PACIFIC OCEANS.

That the currents of the Atlantic Ocean are connected with, and form an extension of those of the Indian and Southern oceans, has been proved by the researches of Rennel and others. Hence it follows, that the drain of these currents must be compensated by other currents which pass from the Atlantic to those seas, by some unknown or unexplored route, currents which move either at the surface or at lower depths. If these compensating currents exist at the surface, as is quite probable, on what meridians of the extreme South Atlantic are they to be found?†

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* George W. Blunt, Esq.

† The consideration of the connection of the currents of the North Atlantic and the Arctic seas with those of the North Pacific, through Bhering's Strait, has been purposely omitted, as being less important in a general view, and beyond the expected range of observation by the expedition.

In view of an attempt to penetrate the Antarctic regions, it seems important to ascertain those routes by which the warmer currents of the great Southern ocean enter the polar basin, and on what routes or meridians they again emerge as ice-bearing currents, moving towards the lower latitudes. The thermometer will prove an important auxiliary in determining these localities, and the course of the polar currents from the Antarctic basin is now partially known, by the course of the icebergs which descend to the lower latitudes. It is by following the course of the warmer currents which enter the polar basin that the nearest approach will probably be made to the Antarctic pole; and the same system of continuous current might afford the means of final escape, should a ship be compelled to winter in the ice of that perilous region.

As regards the great system of currents in the Pacific, we may infer from the facts already known, that a current from the Antarctic region sets to the northward, several degrees west of Cape Horn, which unites its waters with those of the more temperate latitudes in their flow to the coasts of Chili and Peru, and thence towards the equator. If an ice current does not thus unite with that of the coast, the latter is mainly supported by the great afflux of the extratropical currents from the west, which, in performing their constant circuit of revolution, next sweep from the coast of Peru towards the equatorial latitudes, where they continue their course to the westward, again to leave the intertropical latitudes with an elevated temperature, which is in turn conveyed to the higher latitudes.*

The numerous archipelagos of islands and the extensive groups of coral reefs in the Pacific, serve to intercept the regular westerly progress of its warm intertropical currents, and to determine more than one circuit of compensation and revolution in each hemisphere. This class of obstructions partly supplies the place of a continent, in defining separate basins of revolution for the currents of this vast ocean, and this is particularly the case in the South Pacific, where these obstructions are scattered over wide areas. Hence, strong currents setting to the eastward have

* From information which I have gathered, I entertain no doubt of the blending of this ice current with the general current towards the equator on the west coast of South America; and the very reduced temperature which this current carries to the equator, at or near the Gallapagos Islands, is proof of the fact.

been found in various parts of the Pacific, below the latitude of 30°, moving in direct opposition to the influence of the strongest portion of the trade winds.* Thus the system of currents, as we shall find of the winds, becomes more complex and irregular in this vast ocean than in the Atlantic; which, at least so far as relates to winds, is contrary to representations which have been often erroneously made by scientific writers; representations which doubtless were founded in general reasonings on the calorific theory of winds.

Good observations on the direction, strength, and temperature of the currents, in all parts of the Pacific, will prove of great importance, and should be made and registered, most carefully, by the expedition.

The obstacles which thus modify the natural system of currents are least numerous in the North Pacific, where the trending of its continental coasts, except in high latitudes, is highly favorable to a strong development of the regular geographical currents, near to these coasts. Hence, on the coasts of China and Japan we find a current which fully represents the Gulf Stream of the Atlantic. This current, I find, was frequently noticed, incidentally, by the officers of Cook's last exploring expedition, and its velocity stated, in some instances, at five miles an hour. Other observations, to which I have had access, have confirmed the existence of this current, and have shown the elevated temperature which this stream carries from the lower latitudes; so that near one thousand miles east of the coast of Japan, in lat. 41° north, the temperature of the surface water has been found at 79½° of Fahrenheit.† In the South Pacific, near the coast of New Holland there is found, also, a like warm current, pursuing its southern circuit, through the higher latitudes of that hemisphere.

But owing as I apprehend, to the great width of the Pacific, and to the consequent absence of a defined ocean boundary near its central meridians, there is here less of apparent regularity and

* This counter current, running to the eastward, is sometimes found in the equatorial regions of the Pacific and other seas, and bears some analogy to the westerly monsoons of the Indian and Pacific oceans.

† Voyage of Capt. Dupetit Thouars. Other and earlier observations had attracted my attention, particularly in the cruising voyages of our American whalers, but I now refer to this as a more recent and convenient authority.

system, both in currents and winds, than perhaps in any other ocean; the constant and reciprocal equatorial and polar tendencies of oscillation not permitting a single circuit of revolution to extend from Asia to America without deflection. Hence we find more apparent irregularity and complexity in the currents and winds of mid ocean, in this vast sea, than in those regions which are more nearly adjacent to the continental coasts.

A knowledge of the currents and winds of the Pacific Ocean, I am convinced, will serve to remove all mystery and all doubt from the once vexed question of the first peopling of its islands, from the Asiatic continent; in spite of the long urged objection of the opposition of the trade winds. A case is still recent where the wreck of a Japanese junk was drifted the entire distance to the Sandwich Islands, with its surviving crew; thus completing nearly half of the great circuit of winds and currents in the North Pacific. But we shall find an additional means of transport near the equator, which is afforded in the N. W. monsoon of the Indian and Pacific oceans, and which, according to my inquiries, is found to extend, at one portion of the year, as far eastward as the Society Islands; or more than half the distance from the Indian Ocean to the coast of South America.

OF GENERAL WINDS, OR PREVAILING CURRENTS OF THE ATMOSPHERE.

One of the most remarkable characteristics of the atmosphere is its constantly progressive action; exhibited in movements which are more or less rapid, and mainly horizontal.

To whatever general cause these movements may be ascribed, they are found in most countries to predominate in particular directions in the surface winds, but more uniformly at higher elevations. The greatest uniformity of the surface winds has been noticed chiefly in certain zones or regions which, for the most part, lie between the parallels of 30° latitude, north and south; limits which comprise half of the earth's surface. These more regular winds have hitherto been known best on the great routes of commerce, on the Atlantic and certain portions of the Indian oceans, and hence have been called the Trade winds.

In order to account for the supposed uniform character of the trade winds, a general theory of winds has been adopted, of much plausibility, founded on the alleged effects of calorific rarefaction in the equatorial region. Aided by successive emendations, this theory continues to receive the general sanction of the scientific world.

It is not my design, in this communication, to discuss theories. But the facts and results which I have delineated on the accompanying maps,* indicate courses of circulation in the atmosphere which are nearly and mainly horizontal; while the common theory alleges a course or circuit of circulation, in each hemisphere, which is essentially vertical, the warm air being supposed to ascend near the equator to great elevations and there flow outwards, to supply the inward current from the higher latitudes; the obliquity from a north and south direction being of course due to the earth's rotation. I propose, therefore, to state in a summary way, some of the facts and considerations which, in my own view, serve to invalidate this calorific theory.

1. The specific difference of mean temperature in the intertropical winds as compared with equal zones of extratropical winds, is inadequate and wholly disproportioned to the dynamical effects which are exhibited in these winds. I am not aware that any successful attempt has been made to prove the converse of this objection.

2. The rising of the whole body of the trade wind in the equatorial latitudes, in the manner alleged, has never been confirmed by observation; and, as I apprehend, may safely be denied. Nor has any proof of the fact been offered, other than inferences drawn from common but very limited phenomena, which I think may be explained in a more satisfactory manner.

3. The perpetual snow line of the Andes has been found near one thousand feet higher in 16° to 18° south latitude than at the equator, or on the parallel of the equatorial calms of the Atlantic. This fact, in a region so favorable to an equable development of natural influences, I deem to be wholly conclusive against the theory.†

4. The semiannual change, to the north and south, of the locality of the trade winds and the belt of equatorial calms, which results from the change of seasons, bears no adequate proportion to

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* Those lost in the Peacock.

† See the observations of Mr. Pentland in the Journal of the London Geographical Society. Also, Penny Cyclopedia, Vol. VII, Art. CLIMATE.

the alternate geographical declination of the sun, nor to the actual geographical change in the zone of greatest temperature, which follows the sun's declination.*

The semiannual change of the locality in the trade winds is believed to be greatest in the Atlantic, where it does not appear to average more than 7° or 8° of latitude; while the annual range of the sun's declination exceeds 46°, and the actual transfer of the zone of heat, which follows the declination, appears to be nearly 40° of latitude. These facts, also, I deem to be conclusive against the theory.

5. Even within the ordinary geographical limits of the trade winds, there are extensive portions of the system of winds which, in their course and direction, do not accord with the received theory, but appear wholly irreconcilable with its requirements.

To illustrate this objection, I refer, first, to a circuit of intertropical winds in the equatorial basin of the North Atlantic, which appears to extend from the delta of the Quorra, the ancient Niger, for more than two thirds the distance to the coast of South America; in which circuit the winds revolve to the right, with more or less of regularity, around a central and probably elongated axis. And second, to the existence and great extension into open sea of those portions of the monsoon winds which blow obliquely *from* the equator, in directions where there can be none of the continental rarefaction which has been alleged as explaining these alternating winds. For if the winds of the equatorial latitudes rise to the higher regions, the monsoon winds of the Indian Ocean, on departing from the south side of the equator, could never be made to sweep eastwardly upon the earth's surface for even six thousand miles, as they now do annually, instead of ascending four or six miles in altitude, to flow off from the equator as superior winds.†

* In other words, an essential geographical change in the locality of heat, of some months' duration, does not change, materially, the locality of the trade winds. Hence, these winds are not, mainly, the result of heat.

† M. Bougainville says, "from the 23d of February to the 3d of March, we had westerly winds, constantly varying between S. W. and N. W., with calms and rains; every day either a little before noon or soon after, we had sudden gusts of rain accompanied with thunder. It was strange to us to meet with this extraordinary wind under the tropic, and in that ocean so much renowned above all other seas for the uniformity and freshness of the E. and S. E. trade winds; which are

6. The sixth objection which I offer to the common theory of the trade winds, consists in the frequent occurrence, in our American climate, of the highest summer heats for several days in succession, sometimes irrespective of the immediate heat of the sun, which heated air, as appears from comparative observations, is mainly brought to us by geographical transfer along the earth's surface, and which appears to depart in the same manner. This could never happen if the most heated portions of the atmosphere necessarily ascend from the surface. A like objection is derived from the frequent interstratification and horizontal transfer of currents of unequal temperatures and hygrometrical conditions, which appear to move over great distances without any obvious change in their relative altitudes.

Having already noticed, in the course of these remarks, the system of horizontal circuits of revolution pursued by the winds on each side of the equator, it is now only necessary that I refer the observers of the expedition to the particular delineations of these circuits, and of the alternating system of monsoon winds, on the maps which are furnished herewith.*

It must not be supposed, however, that these circuits of revolution in the great winds, are generally uniform or strictly defined in their location or development, even on the open ocean. On the contrary, the winds which proceed outward from the trades, often overlie those which at the same time are returning into the trades. This often occurs extensively, on different meridians along the same parallel; besides the incidental fluctuations and disturbances to which the winds are always liable, and the shifting of their field of revolution to the north or south, by the change of seasons. But the general result, is a continued and

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said to reign in it all the year round. We shall find more than one opportunity to make the same observation."

This relates to the southern Pacific in long. 110° to 115° west from Greenwich, and serves to show an extension of the westerly monsoon winds at that season, even to the meridian of California. Numerous observations have tended to confirm the vast extension of these winds in the intertropical latitudes of the Pacific, opposite to the alleged course of the trade winds. Over the whole western coasts of intertropical America, the course of the winds is also at variance with the calorific theory.

* In the absence of the maps referred to, some general notion of the system of monsoon winds thereon delineated, may perhaps be obtained by referring to the summary description of these winds found in this Journal for October, 1833, Vol. xxv, p. 124–125.

successive series of laminated or stratified currents, overlapping and moving upon each other in like series of subordinate circuits, the major axes of which, in the northern summer, are principally found in the calms of the horse latitudes.

The calms and light winds which are peculiar to this last mentioned region in summer, result not so much from any general suspension of the aerial movements, as from the absence of that brisk relative motion which commonly prevails in other latitudes. For, the predominating movements of the atmosphere being either from the east or west, in conformity with the law of the earth's rotation, and there being little movement of the surface winds in these directions along the parallels in which lie the axes of atmospheric revolution, it follows, that only the more sluggish northerly and southerly winds chiefly prevail on these parallels, in mid ocean, at this season. And I may here suggest, that a like explanation is mainly applicable to the calms of the equatorial region, both between the regular trades, and the Indian monsoons.

Towards the eastern borders of a basin of revolution, such as the North Atlantic, there appears to be less of sluggishness in the aerial currents which move to or from the lower latitudes; which here appear more clearly defined and more strongly developed, and hence are more readily traced in their course; as is seen in the northerly winds which gradually merge in the N. E. trades, in the region between Madeira and the Canaries, and thence to the tropic. While, near the western borders of the Atlantic and over the adjacent coasts of America, the opposite southerly and southwesterly winds of the circuit are often well developed at the earth's surface, at least in the warm season. Like characteristics pertain to the system of winds in like latitudes, in other circuits of atmospheric revolution, in different oceans.

That the N. E. trade winds have not sooner been traced in their horizontal curves into the southwest winds, may be owing in part to the frequent overlying of the southwesterly upon the easterly winds, which occurs mostly towards the exterior portion of the trades; and partly, to a neglect to inquire into the actual and successively varying directions of the trade winds, in the central and western parts of the ocean basins, in the intertropical latitudes. In these latitudes, in the regions here mentioned, the N. E. trade winds are more often found nearly at east, and veering to E. S. E. or S. E., than has been generally imagined.

But the courses traversed by storms, in the trade-wind latitudes of the western Atlantic, and in corresponding latitudes in the western portions of other seas, as shown by my own inquiries and those of Col. Reid, I conceive to have proved this horizontal course of atmospheric circulation, in the clearest manner; and it was this kind of evidence which first brought conviction to my own mind.* In pursuing this branch of the evidence we are thus able fully to establish the western half of the north Atlantic circuit of revolution in the general winds; while, the better defined courses of the regular winds from the latitude of Madeira to the trades, in the eastern Atlantic, is such as to remove all reasonable doubt of a nearly continuous circuit of revolution, from left to right, around the region of extratropical calms, called the horse latitudes.

I may add, on this occasion, that if further proofs were wanting of this horizontal circuit of revolution in these general winds, it is found in the *rotation* of the great storms, from right to left in the northern hemisphere, around their several moving axes, while pursuing their natural course of progression in this great aerial circuit. The question has often been asked, why should all these storms revolve in this direction, rather than in the opposite? And why the contrary rotation which is noticed in the southern hemisphere? Now I have been convinced for several years, that this law results from the conditions which necessarily attend the earth's rotation. For, in the westwardly movements of the atmosphere upon the earth's surface, obliquely from the equator towards the poles, the narrowing of the meridional spaces and the reduced velocities of rotation in the earth's crust on the parallels newly arrived at by the surface wind, with the constant retardations of eastern movement in the front of the mass which results

* See my published maps of 1830 and 1835 containing the tracks of storms; also, my communications in Silliman's Journal and the Nautical and Naval Magizines, since April, 1831; likewise, the charts &c., of Col. Reid, R. E., published in the Professional Papers of the Royal Engineers, and his elaborate work on the Law of Storms, issued at the time these remarks were in preparation; a copy of which work was received and forwarded to the expedition. More recently the labors of Mr. Piddington of Calcutta have afforded much additional evidence, as relates to the Indian and China seas.

therefrom, conjoin to induce a rotary tendency in the incumbent winds, in the very direction in which the storms are found to revolve.*

This dynamical tendency to gyration in the atmospheric currents or winds which are in contact with the earth's surface, is constantly productive of sensible effects, particularly as we proceed from the intertropical to the higher latitudes. This, I apprehend, is the chief cause of the changes and variableness of the winds in these latitudes, and also of the remarkable increase of the barometrical oscillations, the great storms being only the more strongly marked cases of gyratory action; while the numerous weaker or abortive cases which go to fill up the intervals of space, and partly overlie each other, and which are also modified by the ordinary disturbances of temperature and locality, have excited little notice or inquiry. It is this law of terrestrial rotation which, as I apprehend, is maintained by Prof. Dové of Prussia, in his attempts to show the elements of gyration in the general winds; a writer with whose labors I have been but lately and partially acquainted.

The general correctness of the foregoing view of the prime cause of local gyrations in the atmosphere, as well as the rotation in great storms, may be shown by an experiment made on the surface of a common globe; which I have occasionally pointed out to friends interested in these inquiries. Let a concave surface of wood or other substance, of a circular form and a diameter equal to five or ten degrees of the globe, be prepared and perforated with a small hole in the center, through which a pin may be loosely placed, to serve as an axis. Then let the concavity be lined with flannel or other yielding material, and placed upon the top of the globe near the equator. Then cause the globe to revolve from west to east in the direction of the earth's rotation, while the concave body is guided, carefully, by the pin at the axis, in the direction of the storm tracks which are found on my

* It will be noticed that the rotation of the great storms, as well as more ordinary atmospheric gyrations, is opposite to that of the great natural circuit of winds in which they are carried forward. Thus, if a general current of revolution swept around such a lake as Ontario from left to right, the eddies and local gyrations near its borders and in the body of the stream would exhibit a contrary rotation, from right to left.

chart of 1835,* and so as to impinge with equal weight and surface on all sides of the pin or axis, and the incumbent body will be found to revolve from right to left, in the manner of the storms of the northern hemisphere.

This experiment requires delicate management, and is more difficult because of the necessary rigidity of the incumbent surface, causing one part partially to counteract another; but in the case of a fluid, where all the particles move freely upon each other, no such impediment exists.

As it is chiefly the lower stratum of wind which is thrown into gyration from this cause, it must be evident, as above suggested, that within the geographical limits of the trade winds the great circuit of aerial revolution must be a nearly horizontal one, and that the storm tracks mark distinctly the usual course of this revolution. Consequently, the main outflowing course of the trade wind from the equatorial latitudes is not in the upper regions of the atmosphere.†

It was my design to have followed these general remarks with a detailed explanation of the delineations of the several systems of prevailing winds which I have placed on the maps before referred to. This was particularly my intention as relates to the extensive developments of the monsoons, and the several belts of light winds and calms which may be viewed as the anticlinal and synclinal axes, so to speak, of the several systems of general winds. But the lateness of the call and my necessary avocations have prevented me from fulfilling this labor, in time for the expedition.

This imperfect summary of the results of inquiries which I have pursued with no little interest, is now commended to the gentlemen of the expedition for their impartial examination; and with the expectation, and desire, that truth only, as apart from any favored theories, will be the object of their researches in natural science.

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* This chart may be found in this Journal, Vol. xxxi, for October, 1836; also in London Nautical Magazine, April, 1836, and Col. Reid's work on the Law of Storms.

† This is also shown by the extraordinary heat of the summers in countries near the western boundaries of the great oceans, this heat being conveyed horizontally by the surface winds from the lower latitudes; while in winter the results are modified and an opposite state of temperature induced, by causes which are peculiar to continental meteorology.

Art. X.—*Abstract of the Proceedings of the Fourth Session of the Association of American Geologists and Naturalists.*

(Concluded from page 165.)

Saturday, April 29, 1843.—After the reading of the minutes, *Mr. Dana* read a paper on the distribution of corals, alluding to a former statement of his, (p. 145) that the reef-forming corals are limited in their distribution in our present seas by the temperature of 66° Fah., and also to the fact of the absence of corals from the Gallapagos under the equator, and their presence at the Bermudas, added farther, that owing to these currents the coral zone limited by this temperature was singularly contracted on the western shores of the continents by the extra-tropical currents, and expanded on the eastern by the intratropical currents. On the western shores of America it was reduced to 16° of latitude in width, and in those of Africa to at least 12°, while in the mid ocean the zone is fully 56° wide, and on the eastern coast of Asia and New Holland 64°. These facts with regard to the influence of oceanic currents, may explain many anomalies in the distribution of fossils. Mr. D. denied the truth of Mr. Darwin's principle, that islands with barrier reefs are subsiding, and those with fringing reefs rising, and stated that he was not satisfied by his observations, that any elevations or subsidences were *now* in gradual progress in the Pacific or on the South American coast. His observations fully confirmed Mr. Darwin's theory, that *atolls* or coral islands were once barrier reefs around high islands, which high islands have disappeared by subsidence. From the distribution of these islands, he drew conclusions different from those of Mr. Darwin, with regard to the areas of subsidence. If a line were drawn in an E. S. E. direction from New Ireland by the Navigator and Society Islands, the islands to the north, with two or three exceptions, would be found to be low and coral, and those to the south, high and basaltic.* These coral islands diminish in number and size as we recede to the north from this line, and over a large area between the Sandwich Islands and the equator there is an open ocean without islands. As we go south from the same line, the extent of the reefs around the high islands diminishes. From these facts he concluded that the subsidence in progress during the formation of these islands was greater under the equator where the coral islands were few and small, than farther south, where they are numerous and large : that still greater subsidence took place over the open seas where the currents were too

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* See the map accompanying Mr. Dana's article, published in the present volume of this Journal, page 131.

rapid to permit the growth of the coral to keep the islands at the surface: that the subsidence south of this line, where high islands prevail, was less than on the line, and still less than to the north of it. The large area of subsidence thus indicated, is not less than five thousand miles long and three thousand wide, and covers at least fifteen millions of square miles. He also alluded to the singular fact that the longer diameter of this area of subsidence corresponded to the trend of the Navigator, Society and Sandwich Island groups, and the low or coral archipelago.

As present opinions seem to require a balance motion in changes of elevation, he suggested that inasmuch as the tertiary rocks of the Andes and North America indicate great elevations since their deposition, that possibly during this great Pacific subsidence, America, the other scale in the balance, was in part undergoing as great or greater elevation. The absence of corals from the western tropical parts of this continent, was explained by reference to the extratropical currents before alluded to.

The President enquired of Mr. D. if he considered this subsidence to have taken place *equally* over the entire area.

Mr. D. replied, that it could hardly be expected that this effect should have been *exactly* uniform, considering the vastness of the area, but that considered as a whole it must have been nearly uniform.

The President objected to the supposition of Mr. Dana, that this vast amount of subsidence was due to the gradual refrigeration of the earth from a heated state; he thought it susceptible of mathematical demonstration that the amount of subsidence was too slight to account for the changes which must have taken place in the Pacific. Indeed, the earth must have acquired a nearly statical equilibrium of temperature before the existence of the corals, which probably did not make their appearance until the post-tertiary period.

Mr. Redfield said he had been much gratified with Mr. Dana's references to the fixed memorials of the warm currents in the Pacific, as afforded in the coral formations. Previous to the sailing of the Exploring Expedition he had the satisfaction of reviewing with Mr. Dana, partly in reference to a great dynamical question which regards the currents of the atmosphere and the ocean, some of the evidence which he, Mr. R. had before obtained of the character and courses of the great system of currents in the Pacific, and of the low temperature of the extratropical currents which sweep towards the equator along the western coasts of North and South America.* The effect of these cold currents was such that the temperature of the sea at the Gallapagos Islands, under the equator, was from ten to twenty degrees below that of the Gulf of Mexico, and the waters of the Gulf Stream on the coast of the United States. Moreover, the observations had shown a great difference in the temperature as taken on opposite sides of these islands. This difference he had ascribed to the low temperature of the inflowing current from the southern hemisphere, which from the observations obtained he had supposed to sweep near these islands. Analogous effects had also been noticed on the western coast of the African continent; and the obvious bearing of the observations which he had collected from other parts of the Pacific was to establish the existence and approximate outlines of a general system of currents in that vast sea, and which, as in other oceans, controls and modifies the temperature of its waters, most remarkably, even on the same parallels of latitude.

He had mapped out the results of his former inquiries on this prevailing system of both winds and currents on blank charts furnished by Mr. Dana, which were intended for the use, as well as correction, of the observers of the Expedition. These maps he regretted to say had been lost before the end of the cruise, in the wreck of the Peacock. He was now pleased to find, however, that the main results as to ocean temperature and currents, which had cost him no inconsiderable labor at earlier periods, no longer rested on scattered observations made by different navigators; but that Mr. Dana, among other highly valuable labors, had carefully examined and brought to bear on the case the undeniable evidence of innumerable *register thermometers* afforded in the living corals, which serve to mark with greater certainty and precision the extent and boundaries of the warmed waters of the Pacific Ocean.

Mr. John L. Hayes differed from Mr. Dana on the question of general elevation and subsidence in the areas of coral islands; his belief was, that according to the views of the great Prussian geologist Von Buch, the districts of elevation were limited and not contemporaneous.

Prof. Rogers enquired of Mr. Dana what was the maximum rate of subsidence consistent with the growth of corals.

Mr. Dana replied that there were no definite facts bearing on the subject, but that it must of necessity be very gradual.

Prof. Rogers remarked that if accurate measurements could be made for a sufficient period of time on existing reefs, of the depth of the water from fixed points, evidence might be accumu-

* See Mr. Redfield's paper, at page 293 of this volume.

lated equal in value to that derived from similar means on the coast of Sweden, as bearing on the great dynamics of our earth.

Prof. Bailey read a paper referring to the observations he had made upon the microscopic fossils in specimens from the infusorial stratum and adjacent miocene deposits at Petersburg, Va., which had been sent to him for examination by M. Tuomey, Esq. of that place, the discoverer of the locality.

He stated that the infusorial earth, and the included casts of Crassatella, Pecten, &c. contained all the species of Coscinodiscus, Actinocyclus, Dictyoca, &c. which he had previously described as characteristic of the deposits at Richmond and Rappahannock cliffs; and that in addition he had detected many novel and interesting forms, figures of some of which were exhibited—among them a Triceratium, a large species of Zygoceros, analogous to forms found living at Boston, and a curious unknown fossil which he suspected might be a portion of a Zygoceros.

In the infusorial specimens he detected no traces of Polythalmia, but found them abundant among the contents of the shells sent by M. Tuomey from the miocene beds of Petersburg—figures of some of these were shown, among which were species of Textularia, Rotalia, Triloculina, and also of a minute crustacean, resembling a Cypris in form, but which Mr. Dana had informed him might be analogous to the marine genus Cytherina.

Prof. B. then stated that he had detected Polythalmia, in a specimen of shelly limestone in the collection of Dr. Chilton in New York, which was used in the construction of the Alamo, and which was probably quarried in the neighborhood of the fortress. The species appeared similar to those of the cretaceous group. He also stated that he had examined a specimen of lignite from Cape Sable, Md., sent by M. Tuomey, and found it decidedly coniferous. As amber occurs at the same locality, he suggested that these coniferous trees might have produced the fossil resin which it accompanies.

Prof. H. D. Rogers then read the following letter from his brother, *Prof. William B. Rogers* of Virginia, on the limits of the infusorial stratum in Virginia.

University of Virginia, April 23, 1843.

Since my first discovery of the infusorial stratum on the Rappahannock and at Richmond, as referred to in my Report for the year 1840, I have succeeded in finding a similar deposit at numerous other localities, extending from the Potomac River to near the southern boundary of the state. Among these points may be enumerated the Stratford cliffs on the Potomac, the vicinity of Westmoreland Court House, and a great number of localities between the Potomac and Rappahannock Rivers, the James River below City Point, Petersburg on the Appomattox River, and a tract about Dupre's bridge on the Nuhenen River.

Further search will, I am convinced, greatly multiply these localities, and the observations already made are quite sufficient to prove the wide horizontal extension of this interesting division of our tertiary series. Although in some of these localities, as at Richmond, the stratum reposes upon beds containing eocene impressions, and although beneath the miocene strata, at other places, as for example the Stratford cliffs and Petersburg, it is underlaid by unequivocal miocene, and hence at these places, if not generally, is to be referred to a position in the geological series within and near the bottom of the miocene division of the tertiary, I am inclined however to the opinion that these strata are not all upon exactly the same horizon, and that some of them lie in a higher part of the formation. M. Tuomey of Petersburg, who has recently observed the deposit at that place estimates its thickness at thirty feet.*

In connection with these statements it may be interesting to add, that accompanying the infusorial material I have found vegetable remains at some localities in great abundance. They are imperfectly carbonized, still preserving their form and the fibrous texture, and they seem all to be referable to creeping and apparently cryptogamous plants. From the specimens I am now collecting, I hope to be able to decide with some certainty as to their true character.

Mr. Dana remarked that he had observed the same form of microscopic shell, as No. 7 of Prof. Bailey's figures, in Oregon.

Prof. Bailey said the same form had also been found on the coast of England, probably washed from the chalk.

Mr. W. C. Redfield read a paper entitled "remarks on some new fishes and other fossil memorials from the new red sandstone of New Jersey."

In this paper Mr. R. alluded to the general absence of fossils in this formation and the enhanced geological value of the few fishes and other remains which had been brought to light, and submitted to the Association specimens of three new species of these fishes which he had obtained from near Pompton in New Jersey. He referred to the allied characters of these fishes with specimens which he submitted from the new red sandstone formation of England, and particularly to the slightly heterocercal character of their caudal structure. He showed their dissimilarity to the more heterocercal forms of the fishes of the coal formation, even when of the same genus, and their want of analogy to the

* See this Journal, Vol. XLIV, p. 339.

homocercal forms of the fishes which are found as we ascend above the lias. He referred likewise to the Ornithoidichnite which he had found at Pompton, and submitted the same to the inspection of the Association, it being the first of these footmarks which had been discovered in the red sandstone of the Middle States of the Union; and noticed in connection with these tracks, the interesting discoveries of the bones of a gigantic struthoid bird, which formerly existed in New Zealand, the *Dinornis* of Prof. Owen, a good account of which was to be found in the Penny Cyclopædia for March, 1843, Vol. xxvi, p. 518.*

In continuation Mr. R. then referred to the fossil rain-marks which are found in the same rocks, and submitted some remarkably well characterized specimens, from different parts of New Jersey and Massachusetts. He showed that an objection which had been made at the last meeting to the genuineness of these rain-marks, founded on their appearance in relief on the upper surface of the rock, in some observed instances, could not be sustained; for the fact proved to be, that in a great number of cases in which soft unburnt bricks were exposed to rain, the secondary effect of the rain was to wash and denude the first markings in such manner as to leave only protuberances or markings apparently in relief. He found that the circumstances most favorable to the preservation of distinct pitted impressions of rain-drops and true casts of these in relief, were of somewhat rare occurrence.

Prof. Rogers said that he believed it was the general opinion of our geologists that the new red sandstone of Connecticut and New Jersey belonged to the upper division of the formation of the same name in Europe. One fact which went far toward deciding this point was the occurrence of the Posodonomya keuperi in the new red sandstone of Virginia, discovered by his brother, Prof. Wm. B. Rogers.

The Secretary then read an interesting letter from Prof. Owen, on the Ornithichnites and Dinornis, [which is inserted in the present volume of this Journal, p. 185.]

Prof. John Johnston of Middletown confirmed the statements of Mr. Redfield touching the numerous impressions of shrinkage marks, &c. observed in the new red sandstone at that place.

Prof. Rogers remarked, that the cracks in the new red, were proofs of long continued dry weather, and their width might be proportionate to its continuance, while the size and depth of the rain-drops gave evidence of the strength of the shower ensuing.

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* See this Vol. p. 185.

Mr. John L. Hayes, from an extensive observation of the feet and tracks of living birds, was led to the belief that the Ornithichnites of Hitchcock were probably impressions of volant Grallæ, as these birds inhabited low and marshy ground, while the heavy Struthoid birds allied to Apteryx and Dinornis, were the frequenters of arenaceous plains and lofty hills.

Dr. Emmons showed specimens from the Potsdam sandstones, having strong impressions resembling rain-marks—proving the existence of these ancient meteorological registers, and of course of the rains producing them, much lower down in the rocks than heretofore observed.

Prof. Hitchcock then exhibited casts of nearly all the varieties of bird-tracks hitherto discovered in the Connecticut sandstone. These casts had been skillfully prepared and grouped by his friend Dr. James Deane of Greenfield, the original discoverer of the tracks. He said he could not but feel that the Apteryx character of the impressions, taken with the discovery of the Dinornis, had had a great influence on the mind of Mr. Owen, as deciding his final conclusion. He had himself been so much impressed with the mammalian massive character of the Ornithichnites giganteus when first discovered, that after an attentive consideration he rejected the specimens, in the belief that no bird could make so bold and deep an impression. He could not believe that the birds which made such impressions were volants; while on the other hand, some of the impressions were so delicate and slight as to equal the tracks of any of the volants of the present day.

Dr. Jackson read extracts from a letter of Elie de Beaumont, expressing his great interest in the specimens of O. giganteus which Dr. J. had sent him, and his belief that they were animal tracks.

In the course of this discussion Ex-Governor Seward had been introduced to the Association by Prof. Emmons. He was addressed by the President, who expressed in the name of the Association the great obligations American geology owed to him for the zeal and fidelity with which he had carried the New York survey to a successful completion.

To which Gov. Seward briefly replied.

Subsequently Gov. S. was elected a member of the Association.

Mr. John L. Hayes read a report, prepared in pursuance of a resolution of the Association, upon the probable influence of icebergs upon drift.

The information presented had been obtained from an examination of more than eighty persons, principally masters of vessels engaged in the whale and South Sea seal fisheries, in the merchant service and Labrador fisheries, all of whom had seen icebergs; also from authentic published accounts. He adverted to the intense interest with which all glacial agencies were regarded, to explain the various phenomena of drift, the transportation of earth and large fragments of rock in a southerly direction, the abrading and furrowing of the rocks in the same direction, the distortion and bending of strata of clay, the formation of bowl-shaped cavities, and of the peculiar longitudinal ridges of bowlders and gravel which occur in the drift.

To throw light upon these phenomena, he had collected information,

1. As to the mode of formation of icebergs, their original position, and the manner in which they had been detached.

2. The magnitude and form of those floating at sea.

3. The direction, rate, and nature of their movement, the limits of their transport, their grounding and dissolution.

4. The positive and negative testimony as to the transportation of fragments of rock and earth.

In the first place, it was shown by accounts given of northern and southern glaciers, that islands of ice are fragments which have been detached from those glaciers—that the fixed icebergs or glaciers of the Arctic and Antarctic shores are governed by the same laws, and exhibit the same general phenomena as the glaciers of the Alps. Like the Alpine glaciers, the fixed icebergs of the north and south polar shores are formed by the yearly accumulation of snow. Blocks of rock and earth are found on their surface and in their interior, as they are found on the Alpine glaciers. Several of the fixed icebergs of the Antarctic were particularly described. Instances were cited where these fixed icebergs or glaciers had been strewn with stones transported from a distance, which stones had been afterwards covered by new deposits of snow and ice, where large rocks were found in the perpendicular face of the glacier overhanging the sea, and where they have been covered with piles of sand and volcanic scoriæ. It was shown from the peculiar structure of these icebergs, their fissures, &c. that they must advance into the sea precisely as the glaciers of the Alps do along the valleys. Instances of the detachment of icebergs from the glaciers were cited, and the immense waves produced from their fall were described—these waves lifting up large vessels upon the shores, detaching other bergs and dashing them to pieces, and loosening from them imbedded fragments of rock.

2. The enormous mechanical power which might be exerted by moving icebergs, was inferred from their great magnitude. Many were

described from oral accounts over two hundred feet in height, and from two to fifteen miles in length; and some, which from careful admeasurement, were found to be from two to thirteen miles in length.

3. With regard to the nature of their movement, it was observed that it was very slow and perfectly steady, and in the direction of the great under currents which always tend from the poles to the equator, and that no rotary movement had ever been noticed; and indeed no remarkable movement except that produced by the overturn of the iceberg. Facts were mentioned illustrating the great depth of water in which icebergs sometimes ground, and that they thus remained grounded many years. The limits of the transport of icebergs were shown from the facts observed to be about 40° of north latitude and 36° south latitude.

4. Many original facts were stated as to the transportation of bowlders, from a small size to the diameter of many feet, and from an examination of the positive and negative testimony upon the subject, it was inferred that icebergs are rarely seen charged with foreign materials except near their source.

From the facts exhibited, the following inferences were drawn:

1. The steadiness in the movement of the icebergs in our present seas, in the direction and under the influence of great under currents in our northern hemisphere from causes which must have prevailed as well in the ancient as in our present seas, favor the theory that icebergs with gravers of rock in their lower portions, or pressing the sand and gravel by their immense weight along the surfaces of the rocks in the bottom of the ancient oceans, might have scored and grated along the rocks, grinding off their salient points, and leaving their surfaces smoothed and striated in the fixed southerly direction in which they now occur.

2. The immense magnitude of the icebergs in our present seas, and the evidence as to their present mechanical power, when moved by powerful currents, warrant the conclusion that they must have exerted a powerful influence in pushing and crowding along the sand and gravel which formed the bottoms of the ancient seas, and in thus forming accumulations analogous to the moraines of the glaciers.

3. The length of time during which icebergs may remain aground, even when swept by rapid currents which might surround them with sand and mud, or sweep away the loose materials, leaving hills or banks upon spots protected by the stranded icebergs, favors the idea that this agency had an influence in giving the present form to our drift.

4. The formation of glaciers upon the present coasts under such circumstances, that fragments of rock and detritus from the land upon which they form becomes attached to them, the constant advance and

separation of the glaciers from the land, and then floating into the sea as icebergs, with loads of earth and rocks, lead to the conclusion that icebergs, breaking off from the shores of ancient seas, were important agents in the transportation of rocks and earth from their parent beds. The existence of immense fragments of rock in situations where they could not have been carried by water alone, as on the sides of hills with valleys intervening between them and their parent beds, but where they might have been left by stranded icebergs, favors this conclusion.

5. The fact that a large part of the fragments detached from glaciers are of small size, and that these small fragments of icebergs or glaciers are dissolved and broken to pieces at no great distance from the parent glaciers, together with the fact that fragments of rock, although often seen near their source, are rarely seen at a distance, lead to the inference that the same causes limited the transportation of the bowlders and larger fragments of the drift, to within the comparatively small distance from the parent rocks at which they now occur.

Mr. Nicollet, from the committee on drift, had drawn up a short paper on the erratic deposits of the great valley of the Mississippi, which however he did not read, but made some remarks on the great importance and interest of the subject, and its bearing on the philosophy of geology and present causes. He urged the importance of an united effort on the part of all of the members of the committee on this subject, and the advantages of their making up a general report on the subject to embrace the views and observations of all.

The standing committee then handed to the chair the following resolution, which was passed, and the letters mentioned addressed.

"The standing committee has come to the conclusion that from the number of papers to be presented, it is inexpedient to accept the polite invitation of the railroad companies. Therefore resolved, that the secretary address a letter of thanks, making known the regret of the Association, and that it be sent to the secretaries of the said companies at an early hour."

Dr. C. T. Jackson was on the committee appointed to report on the subject of drift; he had, from his labors in the field and laboratory, been prevented from giving that time to the preparation of a paper on the subject which it seemed to require. He would call attention to a few subjects orally. The uniform direction of these scratches seemed to indicate a general cause acting in a direction from the north. In Lapland and Finland their course was found to be from northwest to southeast.

Dr. J. gave way to a motion that the Association now take its recess.

Afternoon.—Mr. S. B. Buckley read a paper on the *Zygodon* of Owen, (the *Basilosaurus* of Harlan,) and exhibited some of the enormous fossil vertebræ of this extinct animal.*

Mr. Redfield spoke of the importance of retaining this skeleton in the country, and of its appropriateness, when suitably erected, as an ornament to this hall, which now contained so rich a collection of palæontological remains.

On motion of *Prof. Hitchcock*, a resolution was adopted expressing the sense of the Association as to the importance of having this skeleton placed in the state museum.

Prof. Bailey said that it was interesting to see the two extremes of existence united in the same specimen; he had examined the marl in which the bones of the Zygodon were imbedded, and found it composed to a great extent of minute Foraminifera and Polythalmia, some of which were perfect, others decomposed or showing only casts of thin cells. The species he did not recognize as having seen before.

Dr. C. T. Jackson resumed his communication on the subject of drift, begun in the morning session, being one of the committee appointed last year to report on that subject.

Few subjects excite more attention at present among geologists than the phenomena of drifted rocks and soils, and the recent attempts to combine observations and form some plausible theory has invested it with a still higher interest. It was formerly supposed that the phenomena of drift were the effects of a transient deluge, which many were inclined to identify with that described in Genesis. It is now however generally conceded among geologists, that this occurrence was prior to the creation of man. There are no remains of his works or presence to be found in the tertiary shale which immediately preceded this epoch; neither arrow-heads, firebrands, or any other work, nor the vestiges of his footsteps or the fossilized bones. Hence we may conclude that man was not formed until the world was finished and prepared for his abode. The geologist sees in the diluvium not proofs of Divine vengeance, but evidences of the highest wisdom and goodness of the Creator in thus preparing and commingling the soils of the earth. The earliest exact observations on this subject were made by De Saussure, Pallas,

* See description of this skeleton by Mr. Buckley in this Journal, Vol. XLIV, p. 409.

and De Luc, on the continent of Europe, and by Sir James Hall in Scotland. More recently Von Buch has collected a vast amount of interesting facts, and lastly Messrs. Charpentier, Agassiz, Sefstrom, De Beaumont, Durocher, and many other distinguished men in Europe and America, have added their labors, both in the collection of facts and the development of theoretical views. It was natural that causes should be sought for effects observed, and not surprising that when bold and plausible hypotheses were advanced, they should find warm advocates, as well as uncompromising opponents, and thus from a thorough sifting of the facts and principles by both parties, truth would be in the end attained.

The present state of the controversy as to the phenomena of drift, aptly illustrates these remarks. A cause now in action was discovered, which was deemed sufficient to have produced the various effects which are termed diluvial, or drift phenomena. Many eminent men incautiously embraced the new theory, which within two or three years from its promulgation, has been found utterly inadequate, and is now abandoned by many of its former supporters. This was the glacial theory of the celebrated Agassiz of Switzerland. He then called attention to a report by M. Durocher on the phenomena as exhibited in Scandinavia, and a comparison of the facts there noted with what he had observed in the northern states of this country. De Beaumont objects to the use of the word *diluvium*, and prefers the terms employed by Charpentier, *terraines erratiques*, *blocs erratiques*, and *phenomene erratique*. In England and this country the term *drift* has been substituted, but to cover all the cases must be combined with some other word. Thus we say, *drift*, *striæ*, *embankments*, *ridges* or *phenomena.* Durocher uses the word *diluvium*, but by it he does not intend to express a belief in any theoretical views. He refers the phenomena to a period anterior to the existence of man, as is done in this country. In 1839, Mr. D. left the French Arctic Exploring Expedition for the purpose of investigating the *drift* phenomena in the Faroe Islands; then visited Spitzbergen, the northern coast of Lapland, St. Petersburgh, Finland, the interior of Russia, Poland, the north of Germany and Denmark, continuing his travels until June, 1840. Thus passing over an immense surface, he collected all the most important facts, and consulted the observations of the geologists residing in the various countries he visited. He found in the north of Europe, that the furrows and scratches are visible on all the rocks which are hard enough to receive and permanent enough to retain them. And also that there are two sets of striæ crossing each other at angles, never greater than 10° or 12°. This coincided exactly with the observations of Dr. J. on the drift scratches of Maine, New Hampshire, and Massachusetts. On the borders of the

Gulf of Alten, (the variation being 11° west,) the striæ on the sleat run north and south magnetically; those on the amphibole N. 15° W. and S. 15° E. by compass. On the borders of Lake Ladoga, the striæ on granite run N. 22° to 25° W. by compass. In Finland, the striæ are further from the meridian, being N. 69° W. to S. 69° E. by compass. Proceeding south, this angle diminishes to N. 20° W. and S. 20° E.—N. 30° W. and S. 30° E.; the normal direction being N. 25° W. mag.

M. Sefstrom found on the west side of the Gulf of Bothnia that the direction was N. W. and S. E. and this may be considered as the mean of all the observations; the variations being accidental, owing to elevations producing deviations in the course of the currents. Valleys of erosion follow the same direction. The direction of the *Osars*, and also drift-blocks and bowlders, correspond with this course. These *Osars* are identical with the ridges which in Maine are called *horsebacks*, and like them resemble railroad embankments, and are composed of sand and gravel.

Marine shells are found in the drift of Denmark, of which M. Beck recognizes seventy species as identical with those now living. They are often broken, but sometimes both valves occur together. Near Stockholm the shells were found whole, and were tranquilly deposited. So also at Uddevalea, originally explored by Alex. Brongniart. These are among the proofs of the submergence of this region during the diluvial epoch. From the S. E. of Finland to St. Petersburgh and Moscow, erratic blocks of granular granite, peculiar to Viborg in Finland, are found scattered—the least distance is from one hundred and forty to one hundred and fifty leagues. Blocks of sandstone at Memel came from Lake Onega, a distance of two hundred and forty five leagues. Thus the drift phenomena in the north of Europe are more remarkable than those described by Dr. J. in the Reports on the Geology of Maine, in which erratic blocks were traced to a distance of one hundred and twenty six miles in a direction S. E. by E. Examples of these were mentioned in Maine, New Hampshire, Massachusetts, and Rhode Island; and reference was made to the works of Prof. Hitchcock for other striking instances.

Messrs. De Beaumont and Durocher are equally cautious in adopting any one cause for the phenomena. Durocher thinks there is abundant proof of two separate causes, distinct as to the time of their occurrence. 1st. The breaking up of the Northern or Frozen Ocean, by which a current loaded with ice was sent over the partially submerged country, abrading the rocks, producing striæ and bowlders, and carrying them to the S. E. 2d. That afterwards icebergs formed on the coast and carried off each summer their load of rocks and earth, and deposited them to the S. E. In the meanwhile the land was gradually rising from the sea level, and brought the detritus above the surface. During the periods of tranquillity marine shells were deposited in those portions of the country which were submerged. Durocher, De Beaumont, and Bohtlink, all consider the glacier theory insufficient to produce the effects observed. And the phenomena in Scandinavia, like those observed here, all go to disprove the hypothesis of Agassiz. Even in Switzerland, the celebrated Prof. Andre De Luc regards the views of Agassiz as fanciful and imaginary. Dr. J. referred to the quotation of Peter Dobson's hypothesis by Mr. Murchison. He concluded by remarking that he had travelled over most of the Swiss glaciers, and knew the statements made were greatly exaggerated. He knew too that our drift striæ and erratic blocks did not radiate from one principal mountain group, nor were they in any case much deflected by them; nor was there any proof that glaciers had ever existed in Maine, New Hampshire, or Massachusetts.

Mr. Nicollet then rose and addressed the meeting at considerable length and with great animation on the subject of Dr. Jackson's paper just read, and in opposition to the glacial theory of M. Agassiz. He expressed his astonishment that M. Agassiz should have entirely overlooked the labors of his predecessors in the same field, and particularly of M. De Saussure, who spent forty years in investigating all their phenomena, and had nearly exhausted the subject. It was impossible to conceive how the effects ascribed by M. Agassiz to the moving glaciers could with propriety belong to them. The *mer de glace* was an immense vault of ice under which, as in a grotto, one could walk even for twenty miles, while on its bottom runs a stream of water. How could the bottom of the *mer de glace* then be supposed to score and furrow the rocks in its path? M. Agassiz had overlooked too the true effect of the expansion of the ice; he had ascribed to *it* the downward movement of the glacier, while De Saussure long ago proved that this motion was due to gravity only. The expansion did effect the fissuring and arching up of the glacier, just as it produces the same phenomena in the ice of our rivers.

One very important point in the subject of diluvial furrows had been overlooked by M. Agassiz as well as by many other observers of the same facts, not only in the Alps but in other places—this was whether the furrows on the rocks obeyed the direction of the valleys, or the general direction of diluvial furrows, irrespective of the sides of mountains and the course of valleys. Until full observation was made of the facts with this point in view, we could arrive at no valuable general conclusions.

An animated and pleasing debate then ensued between *Prof. Hitchcock*, *Mr. Nicollet*, *Mr. Redfield*, and the Chair.

Prof. Hitchcock remarked that so disastrous had been his experience in respect to the glacial theory of Agassiz, that he was almost afraid to say any thing more on the subject. His views had been so much misunderstood on both sides of the Atlantic, that he was satisfied that the fault lay in the language which he had used on former occasions. He had been supposed to be an advocate for the unmodified glacial theory. But if he could trust his own consciousness he never had been a believer in it. The views which he presented in his paper on the phenomena of drift in North America, read to this Association last year and now published in their Transactions, are essentially the same as those which he held when he gave his anniversary address before this body in Philadelphia: and those views he certainly stated in that address. Nay, he invented a new term, viz. *glacio-aqueous*, to express the final conclusions of his mind on the subject. By this term he meant to say that the phenomena of drift were the result of the joint action of ice and water, without saying which of these agents had exerted the greatest influence. But whether that glacio-aqueous action had been the result of the enormous accumulation of glaciers according to Agassiz, or from floating icebergs while northern countries were yet beneath the ocean according to Lyell and Murchison, or of the upheaving of the Arctic Ocean whereby its aqueo-glacial contents were precipitated southward according to De la Beche, he had not then made up his mind nor has he yet made it up. The *Etudes sur les Glaciers* of Agassiz did indeed throw a flood of light into his mind, by showing how (if that writer has rightly interpreted the phenomena of glaciers) moving ice could produce such effects as are connected with drift. It did seem to him to have introduced a new element into the dynamics of drift, and he expressed his strong admiration of the labors of the distinguished professor of Neuchatel, though he certainly never meant to adopt his views in full: and in saying that the fundamental principle of Agassiz's theory seemed to him to be true, he meant only that ice and water had been the agents employed in producing the phenomena of drift, for he understood the glacial theory to require both these agencies. Indeed the melting away of the vast accumulations of ice around the poles, which this theory supposes to have been done suddenly, must have produced southerly currents and transported icebergs in that direction in vast quantities.

The ground then which he (Prof. H.) took and still takes was that ice and water were the agents employed in producing the phenomena of drift, and he found that nearly all geologists of the present day admit this position. He was happy therefore to find his views in accordance with the whole geological world. Geologists do indeed differ in the proportions in which they mix ice and water for this work; but they all admit both agents to have been concerned. Even Elie de Beaumont, according to Dr. Jackson's paper just read, admits both these agents, and this certainly is an advance upon the views which have so extensively prevailed in continental Europe on this subject. Prof. H. did not feel as if we could safely go farther than to say that drift was the result of glacio-aqueous action, and he had some doubts whether we could ever go farther except hypothetically. Yet most geologists seemed not willing to stop there: and he had no objection to their indulging in conjectures in the wide field beyond, and he would always be happy to examine their ingenious hypotheses. And in regard to the glacial theory he could not agree with Dr. Jackson, that it was already dead and waiting to be buried. The late numbers of the Edinburgh Philosophical Journal, of the Geologist, and other European periodicals, loaded as some of them were with papers on this subject, certainly looked as if some vitality still remained in that theory or its advocates. He was particularly interested in the effort of Mr. Maclaren to make the glacial theory and the iceberg theory coalesce. Indeed it would not be strange if the true and ultimate theory on this subject, if that is ever reached, should be a combination of all the three leading hypotheses which have been alluded to above.

In conclusion, he begged leave to say, that he derived his first ideas of glacio-aqueous action, nearly twenty years ago, from the papers of Sir James Hall on the diluvial phenomena of Scotland. For although that writer imputes those phenomena to a deluge, he loads the waters with ice bearing fragments of rock which must have scoured and ground the surface. And it is a little curious that while the late distinguished president of the London Geological Society finds the germ of the iceberg theory in a paper by one of our countrymen, (Mr. Dobson,) some of us should have derived it from a distinguished geologist of Great Britain.

Mr. Redfield said if any savans of Europe have set down our worthy first president (Prof. Hitchcock) as an incautious geologist or likely to be led away by novelties, we all know they are much mistaken. He well recollected having been led into a discussion with him on the subject of the transportation of bowlders and drift some years since, on which occasion the agency of ice had been treated with proper scrutiny and caution. Mr. R. said that although then entertaining views similar to those of Lyell and Murchison, yet he had relied mainly on the force of the polar currents. But as to the *cause* of these currents, which had been previously referred to, he desired to wash his hands of all suspicion of attributing them to the melting of ice and evaporation in the lower latitudes. He considered them to be due to other causes of a far different nature.

Mr. R. spoke of the vast effects of the regular polar currents of the ocean in the transportation and deposit of bowlders and drift, currents which must have been in action ever since the earth had rolled on its axis with an incumbent ocean; and he thought this mighty and enduring agency as conjoined with ice had not been duly appreciated. He felt that much credit was due to Mr. Hayes for the facts which he had collected from observant voyagers, and took occasion to allude to the summary outlines of the systematic currents of the ocean which had been given by himself at a former meeting of the Association, but not furnished for the report of proceedings. He then traced on the map the natural course, as well as the deflected or forced direction of these currents as they issued from the Arctic regions; the natural course falling westerly and the deflected one easterly of a meridian line, and corresponding severally in direction with each of the two systems of striæ found on the rocks of North America. He showed on the map the coincidence of these striæ with the present courses of the great ice-fields and numerous icebergs of the north; suggesting that attention to the phenomena of single icebergs in open sea would fail to produce an adequate conviction of the efficiency of the cause in question. But those who had attentively considered the narrative of the last voyage of Capt. Back might be satisfied that the movements in mass of such vast *packs* of ice and icebergs as those in which his ship was enclosed for many months and moved slowly a great distance to the southward by the force of the great current and the agitation produced by storms, doubtless while rending and moving by means of the vast floes and the base of the bergs the incoherent portions of the shores and the subaqueous topography and grooving the faces of the coherent rocks, were causes which acting without stint of time, were sufficient to produce most of the phenomena which have been noticed in drift formations.

Mr. Hayes said his observations had proven to his satisfaction that the immense mass of these icebergs below the water caused them to be entirely influenced by the currents beneath the surface, and explained why apparently they were not affected by the winds and currents above the surface. He alluded to some facts which had been observed as illustrative of this point. The reason of these strong under-currents he did not undertake to explain. He would ask of Mr. Redfield whether in his opinion there was any reason to suppose that the currents in the ocean below the surface, always tended towards the equator.

Mr. Redfield replied at some length, explaining the geographical system of currents observed in the ocean. These were mainly independent of the atmosphere and winds. The whole of the evaporation at the equator, let it be as much as it may, would have no sensible effect in producing a current from the polar regions to supply its place. The Mississippi, through its numerous branches, received the drainage of many thousands of square miles, and some had contended that the influence of its current must materially affect the force of the Gulf Stream. He would not undertake to say but what *some* effect was produced, but he could say, that it had never yet been perceptible to observation.

In reply to a question of Mr. Horsford, *Prof. Espy* stated that the evaporation at the equator could not be the cause of the polar currents towards that point. A current of one mile per hour would fill up the entire deficiency caused by the evaporation, in one hour. It was idle therefore to look to that cause for the effect. He thought the mean temperature at the poles and equator, a sufficient cause to explain the phenomena of the currents. Mr. E. explained this point at some length. The specific gravity of the water, caused by the difference of temperature, would give a current from the poles to the equator. At the equator, the temperature of the water at the surface was about 70°, while at the depth of one thousand fathoms it was but 37° or 38°, and remained at about that temperature as you went to a greater depth. This could only be accounted for by supposing that the water at that depth was supplied by a current from the polar regions.

Mr. Hall read a short paper and presented a natural section of a portion of the shore of Lake Erie, exposing the broken and contorted strata and intermingled drift.

This section was exhibited to the Association last year, with a view to elicit similar facts and to enable us to draw some inference as to the cause producing the phenomena.

He stated that the subject of drift had occupied but a small portion of his attention, having been engaged mainly in the study of the older rocks and their contents. In the outcropping edges of the limestones and other firm strata, he had frequently noticed the separation of the layers, one being elevated at a much higher angle than the next below, and the space filled with loose materials. Sometimes large masses were thrown over upon the stratum in place to the south—at other times they had been lifted and fallen back, presenting abrupt anticlinal axes of small extent.

In the section before us similar causes seem to have operated in a more stupendous manner, and to have produced corresponding results.

He gave an explanation of the section, which consists of a stratum of loam, resting on clay and gravel, and below this the shaly strata, which are cracked throughout into short blocks as if by a violent undulatory motion ; insinuated beneath these, and between different strata, we find clay, gravel and pebbles, with smaller fragments of the shale. These materials are often folded and cortorted in such a manner as to lead to the conclusion that they could only have been produced by a tremendous force from the northward, forcing the loose materials beneath and between the strata, and moving the whole to the southward, producing the folded appearance.

The surface of the firm rock beneath is grooved and striated, precisely in the same manner as the surfaces of our present strata, so that if the broken rocks above were removed the surface would present the same appearances as those of the grooved and polished rocks.

It is here quite evident that the breaking up of the strata, the intermingling of the drift, &c., all took place during a single period, being produced by a force moving to the southward. This force, whatever it may have been, seems sufficient to have produced the breaking up of strata, the production of worn fragments, and the excavation or denuding operations every where visible.

The phenomena presented in this instance lead us to the most interesting conclusions, but whether we are warranted in assuming that similar causes may have produced the effects every where, it is perhaps impossible to decide. The subject of drift and the causes of its production are still open to discussion, and no theory yet advanced seems satisfactory to all parties. Therefore, without a desire to advance any theoretical considerations, he had presented the facts for the consideration of the Association.

After explanation of sections of the drift and river channel at Portage, *Mr. Horsford* inquired whether there was evidence of the original north and south valleys having been excavated by a force operating from south to north.

Mr. Hall replied that he had formerly embraced that opinion, but finding no positive evidence of such a force had abandoned it. If this opinion were true we should expect to have found remains of southern drift among the loose materials at the north, but so far from this being the case he had always found northern materials in the southern drift. He was led to believe from the facts in the case, that there must be valleys of two distinct epochs.

Prof. H. D. Rogers then addressed the meeting in a very eloquent manner, showing how the absence of southern materials in the northern drift might be accounted for by supposing the forces of the northern currents to have been so great as entirely to have swept all vestiges of the superficial drift into the Gulf of St. Lawrence. This vast reflux of waters was attendant on the uplift of the continent, and the great drainage which resulted from the flowing off of the oceanic waters.

Mr. Hall read a short paper in explanation of two sections at Portage, N. Y.

Under the terms diluvium, drift, &c., are included products, which, however similar they may be in general characters, are often due to different causes, and are the results of eras widely separated in time, and differing in many essential circumstances. The more ancient appear to have been the more universal, and as we descend to modern periods, the extent of the operation seems to have diminished.

In our theories we have made provision for a wide sweeping deluge, for immense excavating waves, and for hemispheres of ice, but we have overlooked the subsequent and minor, though often important operations of the bursting of lakes, or the change in river channels, which must have occurred frequently during the earlier periods after the emergence of our continent from the ocean. The existence of such lakes, would be a natural consequence of the contour of the surface.

Evidence of the outbreaking of such lakes is seen in the margins of all our great valleys, where more recent detritus is spread over the older deposits of that kind.

Bones, shells and fragments of wood, are frequently found in these deposits, which are referred to the drift period—though we are not prepared to say that the drift is destitute of such remains, yet those which Mr. Hall had seen were clearly in positions to be referred to a subsequent period.

In the excavation of the Genesee Valley canal, at Portage, along the side of a hill which consists of alternating layers of fine sand and clay, at a point about two hundred feet above the base of this deposit, some fragments of fine-grained wood, highly impregnated with iron pyrites, were found. This was in a layer covered by a mass of gravel and sand eighty feet thick, which, from the nature of its materials, was a deposit subsequent to the drift, and of southern origin.*

* The existence of these two deposits was pointed out in the Annual Geological Report for 1839.

Upon the surface of this sloping hill is about ten feet of a deposit of sand, clay and gravel, saturated with water, and constantly moving down the side of the hill. This is evident not only from facts ascertained in excavating the canal, but also in the fact that the oak trees which grew upon the high ground have slidden down and become intermingled, standing in all directions among the hemlocks which skirt the margin of the river below.

The deposit in which the fossil wood is found, was probably made in a lake which was afterwards filled with an accumulation of gravel and sand, derived from the outbursting of some reservoir farther south. This deposit being pervious to water allows this liquid to pass through it, and meeting with an impervious mass below, flows out in the form of springs, undermining and carrying down the surface matter.

In a transverse section extending across the river valley and channel, Mr. H. illustrated the changes which had taken place in the direction of the Genesee, during a comparatively recent period.

The river to the south of Portage flows in the bottom of a broad valley, extending toward the north. At Portageville the stream bends around to the left, and after flowing a short distance nearly south, turns to the north and northeast, cutting its channel through the rocky slate in some places to the depth of three hundred and fifty feet, and forming in its passage three falls of sixty six, one hundred and ten, and ninety six feet respectively. This channel is narrow with mural banks; but a short distance below the lower fall it emerges into a broader valley, in a line with the channel to the south of Portage, before it is deflected from its course.

The space between these two points, as shown in the section, is a deep broad gorge filled to a great height with clay, sand and gravel.

This is evidently the ancient channel of the river, and yet after it had become filled with this drift the stream found an easier passage by excavating the solid rock for three miles, than by removing these loose materials.

Still below this point the river leaves the broad channel and excavates a gorge through the shales emerging into the broad valley at Mount Morris.

Several other lakes and streams in Western New York exhibit the same phenomena, and although there are northern channels filled with drift, the streams often turn at right angles and excavate their course through rocky strata.

The Chair mentioned to the meeting that Prof. Hitchcock would favor the Association with a public lecture in the evening, at 7½ o'clock. Adjourned.

Monday, May 1, 1843.—The minutes of Saturday were accepted, and the Chair presented from the committee on publication the completed volume of transactions, comprising the proceedings of the Association from its organization to the present time, together with all the papers which have been read at former meetings of the Association. It was ordered that the present committee on publication be continued for the year to come, and that Dr. Amos Binney be the treasurer of that committee. It was also

Resolved, That the thanks of the Association be presented to the committee of publication for the very acceptable manner in which they have performed the laborious and responsible duty of publishing the first volume of the Transactions of the Association.

Resolved, That the thanks of the Association be presented to Prof. E. Hitchcock, for his very interesting address before the Association on Saturday evening, and a copy be requested for publication.

Prof. Hitchcock read a paper on native copper found in drift in Massachusetts, and also on the occurrence of yttro-cerite in the same state. He considered the copper as having originated in the primary to the north of the place where it was found; there was no drift in that direction, having any other than a northern origin; he considered this fact might have an important commercial bearing.

Dr. Jackson stated he had found the yttro-cerite in Bolton, Ct.

B. Silliman, Jr. reminded Prof. Hitchcock that copper was found at Bristol, Conn., in the primary, beyond but near the junction of the new red, and that Prof. Shepard had expressed the opinion in his report, that all deposits of copper in the secondary were limited in extent, and were originally derived from the primary.

Dr. L. C. Beck stated that native copper had been found near Somerville, N. J., under circumstances similar to those in Massachusetts and Connecticut; one mass in his hands weighed when found, one hundred and twelve pounds.

Prof. H. D. Rogers enquired of Dr. D. Houghton if he considered the native copper of Lake Superior, as belonging to the older secondary, and whether the trap and sandstone of the peninsula of Michigan, were of the same geological age as the similar formations of New Jersey and Connecticut.

Dr. Houghton said he could not speak definitely as to the contemporaneousness of the two formations, but he was sure of the *similarity* of their structure. The copper in Michigan was frequently accompanied by zeolites and prehnite.

B. Silliman, Jr. reminded Dr. H. of the discussion on this subject, at the meeting in Philadelphia in 1841, and stated his analysis of the Lake Superior copper in reference to its being an alloy of silver and copper, and also the silver on the copper to ascertain if it were an alloy of copper and silver; but he had found both the metals distinct and quite pure, although fused into perfect union at their two surfaces. He would enquire of Dr. H. whether he had found silver under similar circumstances, and whether the silver was not segregated by the action of the dyke, and found in separate masses, and if it promised to be of any economical value.

Dr. Houghton said the great mass in Yale College cabinet, referred to by Mr. S., was the only loose mass, where he had seen the silver and copper united; he had been at the same vein that afforded this specimen, and had found silver distinct in branching masses, but in very small quantity, and he had in one case found antimonial sulphuret of silver; the silver and copper were both very nearly pure.

Dr. Beck said the facts in N. Jersey were of a similar character.

On motion of the Chair, Dr. L. C. Beck, B. Silliman, Jr., and Dr. D. Houghton, were appointed a committee to draw up a report for the next annual meeting, embracing all the known facts, bearing on the occurrence of native copper, in the trappean regions of the United States.

Mr. James Hall showed a specimen of the cherty limestone from near Niagara, as proving in a very conclusive way, the direction of the diluvial agency by which the surface of the rocks *in situ* had been ground down; in this specimen the nodules of chert had resisted the diluvial action and stood out *in relievo* above the surface of the stone, having before them a ridge of limestone which had been protected by the chert nodule.

The Chair said he adverted to this fact last year, and considered it as conclusive evidence against the hypothesis of simple aqueo-glacial action, as the cause of the smoothed and polished rocks; he considered that such an effect could result only from the sweeping over the rocks of a vast accumulation of angular and *loose* fragments of hard rocks, mixed with smaller gravel and sharp sand.

B. Silliman, Jr. stated that much of the loose material which covered the red sandstone of Connecticut, consisted of fragments of rocks, many of which could be referred to the trap mountains to the north, which presented one surface worn down quite flat, like the rubbing stone of a stone-cutter, and as if they had been carried evenly and for a long time, over the surface of the rocks, by some force competent to keep them in one position. He left it for gentlemen to decide as best suited their own views, whether they were thus held by being set in ice, or by a superincumbent and adjacent mass of loose materials and waters. The large pebbles of quartz and other hard rocks, of which the coarser beds of conglomerate in the Connecticut valley were composed, were worn down without dislocation, and had, measurably, served to protect a *lee* of adjacent rock from degradation in a manner similar to the cherty nodules shown by Mr. Hall. The striations in these valleys were about S. 20° E. and were on the whole irrespective of the directions of the valley, frequently scouring the sides of hills, in a line oblique to the axis of the valley.

The Chair urged that the diluvial currents had extended farther to the south in the long parallel valleys of Pennsylvania, and had been much influenced by the existing topography of the country; and the scourings on the rocks there were resultant lines between the general direction of the onward current, and the direction of the mountain slopes. It was uninfluenced by the topography of the country so long as the waters stood above the summits of the mountain ridges, but when the inundation was nearly exhausted, the subdivided current conformed itself almost entirely to the configuration of the surface.

Mr. Redfield said that the diluvial markings on our American rocks, might be viewed as constituting two distinct systems, in one of which the striæ have a southwesterly direction, in the other a southeasterly one; the latter system greatly predominating in the country lying east of the Hudson.

The discussion was continued for some length by Messrs. Houghton, Jackson, Espy, Rogers, Hitchcock and Hall, and it was generally admitted that we must find, in the conjoint action of water, ice and loose detritus, a cause sufficient to account for all the phenomena known by the various names of glacial, diluvial and aqueo-glacial.

B. Silliman, Jr. then made a few remarks on the configuration of the valleys of the secondary, as influenced by and connected with the intrusion of trap rocks.

The subject of appointing a committee on drift, to report at our next meeting, having come up for discussion, it was

Resolved, That the following gentlemen be instructed to report on the present state of our knowledge on the subject of drift. New England and New York, Prof. E. Emmons; the West and far West, J. N. Nicollet; the South, W. B. Rogers.

The chairman explained to the meeting, a view which had occurred to him to account for the crescent-formed dykes of trap in the new red sandstone of New Jersey and Connecticut.

That the crescent form of the trappean dykes of the New Red sandstone regions of New Jersey and Connecticut is in some manner connected with the dip of the stratified rocks which they traverse, is plainly indicated by the constant dependence between the direction of these crescents and the direction of the dip. Thus in New Jersey, where the dip of the Red Sandstone is towards the northwest, the horns of the crescents point towards the same quarter, while in Connecticut, where the strata possess an easterly dip, the points of the crescents are directed eastward.

May we not explain this curious relationship by conceiving the fissure through which the melted trap has pushed to the surface, to conform itself, where it traverses the upper part of the inclined sandstone, to the plane of the dip. The sandstone being disrupted in a plane parallel to the dip, the beds on the upper side of the sloping dyke will be lifted off from those upon which they reposed, and in this tilting of the beds, there will arise towards the extremities of the fissure seams or transverse cracks, extending in the direction of the dip. Now when we view the outline of the principal or central portion of the fissure continued into these transverse cracks at its extremities, we readily perceive that it must constitute a curve or crescent concave in the direction of the dip.

Mr. B. Silliman, Jr. remarked that there was an almost perfect identity between the views just explained by Prof. Rogers and those arrived at more than two years since by his friend Dr. James D. Whelpley and himself in the Connecticut valley. These views had been laid before Mr. Lyell by Dr. Whelpley, and illustrated to Mr. L. by visits to several localities in the vicinity of New Haven. It had been their intention to lay a paper on the subject before the present meeting, but they would postpone it to next year. It was then

Resolved, That Dr. J. D. Whelpley, Prof. H. D. Rogers and B. Silliman, Jr., be a committee to report on the intrusive trap of New Jersey and Connecticut.

Mr. Dana exhibited a few drawings by himself, illustrating the metamorphosis of the Anatifa. The young of the common barnacle was first noticed and figured by Thomson, who remarked its close resemblance to the species of the Cypris family among Crustacea. These drawings show the same with regard to the Anatifa, and prove also that the pedicle in the Anatifa corresponds with the anterior legs, (properly a pair of antennæ,) of the young Cypris-shaped animal. In the young state it swims free in the ocean and has a pair of compound eyes. The eyes disappear when changing to the adult form; in this state the Anatifa is a fixed animal, like the barnacle, and has no further use for eyes. One of the drawings represents the young free animal; a *second*, the same attached by its anterior legs, which terminate in a disk to the sea-weed, and a *third*, the full developed Anatifa, with the valves of the shell of the young, (the exuviæ,) loosely adhering to the foot of the pedicle.

The propriety of uniting these animals with the order Crustacea, has been often suggested. The structure of the mouth and legs, and the fact that they change their skin from time to time, like the species of this order, would alone seem sufficient to authorize this union; but now after the discovery by Thomson and others respecting the young, there is no reason for farther hesitation.

Dr. L. C. Beck read a paper on the occurrence of bituminous or organic matter in several of the New York limestones and sandstones.

In this paper the author stated that in almost all the New York water limestones which he had analyzed, the residuum left, after the action of dilute muriatic acid, when subjected to heat, gave out a bituminous or peaty odor. In some cases the proportion of this matter could be determined by first carefully drying the whole insoluble residuum, ascertaining its weight, and then subjecting it to a red heat and noting the loss. In other instances, however, while the bituminous odor was sufficiently evident, the loss of weight was scarcely appreciable. The same fact was observed in the limestone from Rochester, and generally in those limestones termed *fetid*. The sandstones at Laona and elsewhere in Chatauque County are often so highly impregnated with this or a like substance that specimens, even though kept for some time, burn with flame. This bituminous matter has been observed in a limestone from Saratoga County, which, from the circumstance of its being made up of rounded grains, has been called *oolitic* limestone by mineralogists.

Dr. B. remarked that the odor of the fetid limestone had sometimes been referred to the presence of iron pyrites, giving rise to sulphuretted hydrogen. If it was due to iron pyrites, the sulphuretted hydrogen would have been evolved during the process of solution; which was not the case.

The author adverted to the statements made by Dr. Daubeny at the last meeting of the British Association (1842) concerning the occurrence of organic matters in various secondary limestones, and was inclined to consider the whole class of facts as proofs that races of organic beings had existed at the period of the formation of these rocks, of which not a single representative now remains.

Dr. Emmons remarked that this bituminous matter was of general occurrence in the New York rocks, even the older arenaceous deposits.

In some limestones noticed by Mr. Hall, the bitumen was so abundant as to run out when lime was burnt and render the specimen offensive to handle. The limestone at Montreal was so charged with bituminous matter as to smut the hands, while a case had been cited by the late Mr. Eaton, of a limestone having when heated the smell of horn, indicating the presence of nitrogenized matter.

Dr. Owen read a paper on fossil Palm Trees, found in Posey County, Indiana.

They were discovered about twelve miles from New Harmony, in excavating in a slaty clay on the banks of Big Creek, a tributary of the Wabash, for the purpose of laying the foundation of a saw and grist mill, and forming a rag dam. The stratum in which they are imbedded is one of the upper members of the Illinois coal-field.

From the first commencement of the excavation from twenty to twenty five fossil stumps have been seen. Dr. Owen has disinterred only three himself. These were found standing erect, with from five to seven *main* roots attached, and ramifying in the surrounding material. There is every reason to believe that if pains had been taken to expose the others, all would have been found provided with roots.

Besides the three trees which were transported to Dr. O.'s laboratory, several segments of other trees, previously dug out, were found amongst the rubbish. Some of these had the scars of the stems well preserved, and presented besides the structure of the bark, which resembled minute wrinkles, something like the impression left on soft clay by pressing a file on it. No medullary rays or growths could be discovered on the transverse sections of the trees.

All the specimens observed had part of the bark converted into a black carbonaceous substance. Judging from the disposition of the scars on several of the specimens found, there must have been at least three species. This was shown on a drawing; and Dr. O. exhibited a model, quarter the natural size of the smallest stump excavated. A portion of one of the roots of the largest one was also laid before the Association. The general character of these interesting specimens was determined by the form of the scars, being longer horizontally than vertically, and the absence of flutings. In the bed of Big Creek, fifteen feet beneath the roots of the trees, is a seam of coal supposed to be from three and a half feet to four feet thick. Almost immediately over them is a layer of sandstone, and over that an imperfect seam of coal. The top of the fossil tree is about fifteen feet beneath the level of the bank of Big Creek.

Dr. Owen supposed from the present position of these trees, that they have been quietly submerged and now occupy the spot where they originally grew.

A more detailed description of this locality of fossil palm trees will probably appear hereafter in this Journal.

Dr. C. T. Jackson read a report on the organic matters of soils, and exhibited specimens of them and their characteristic salts and compounds.

He demonstrated the complex nature of mould or humus, and proved that only refined and exact analyses would show the causes of fertility and barrenness in soils. Several cases in point were cited, and among others the analysis of three soils; one of which was almost barren; the second was moderately fertile; while the third was remarkably productive and had been cultivated for nearly one hundred years. In these three soils the relative proportions of organic and mineral matters were precisely the same, insomuch that it was supposed at first that they were all taken from the same spot; but a more refined analysis showed a very marked difference in the condition of the organic matters, and to this difference, imperceptible to an ordinary or crude analysis, the difference in fertility was owing.

The organic matters formerly confounded under the names of ulmin, geine and humus, are numerous and very different in their chemical nature, varying in different soils and producing various degrees of fertility.

Berzelius who first described and named geine, has utterly abandoned that name* as improper, there being no such proximate principle, it having been proved to be a very complex compound of new and peculiar acids. Those which we know at present are the following: crenic acid, apocrenic acid, humic acid, humin, extract of humus, glairin, glucic acid, apoglucic acid and coal of humus.

The five first of these substances are generally present in fertile soils. Crenic acid forms two salts with lime, one of which, the crenate, is very soluble in water and in alcohol, while the subcrenate is more difficultly soluble.

Apocrenic acid is highly charged with nitrogen, and is a very valuable ingredient in soils, furnishing a portion of the nitrogenous matter of plants. It is probably formed by the gradual change of vegetable matters through the influence of the air and ammoniacal salts derived from decomposing animal manures and from rain. It forms from the crenates and humates, when they are exposed to atmospheric influence, by breaking up the soil during cultivation. Hence we see that a yellow soil turns black by two or three years' exposure to the atmosphere, and from an unfertile state becomes fertile. We can readily produce the same result in a few hours, when we operate on a small quantity of soil in the laboratory. Apocrenic acid forms salts with lime and with peroxide of iron, which are nearly insoluble in water, but which are readily decomposed by the action of carbonate of ammonia, or by potash or soda; so that we may readily conceive of the fertilizing influence of these alkaline matters, since they render the organic manures, which were before insoluble, perfectly soluble in water, so that they may be absorbed by the rootlets of plants. If these matters are absorbed, as they infallibly must be, it is evident that they must undergo a series of modifications in the sap vessels, so that they are no longer found in the juices of plants in the state which they were when they were first absorbed. Now, by analysis of the sap of plants before the putting forth of their foliage, we find certain extractive matters and sugar. In some, apoglucic and glucic acid have been discovered.

Let us then consider the composition of these substances, and see whether it is possible for humic acid, for instance, to pass by chemical changes into sugar.

	Carbon.	Hydrogen.	Oxygen.
Sugar is composed of - -	6	10	5
Glucic acid of - - -	8	10	5
Humic acid of - - -	40	24	12
Ulmin from sugar - - -	40	32	14

* The erroneous statement in the Amer. Jour. of Science, was owing to the reviewer having only a spurious edition of Berzelius' Chemistry, which was printed in French at Bruxelles.

Now 11 atoms of sugar consists of 66 carbon, 110 hydrogen, 55 oxygen, and may form, by decomposition according to Mulden,

	Carbon.	Hydrogen.	Oxygen.
3 atoms of glucic acid, - -	24	30	15
1 atom of ulmin, - - -	40	32	14
1 atom of formic acid, - -	2	2	3
23 atoms of water, - - -	0	46	23
	66	110	55

Then by the opposite action, which is the regeneration of vegetable matter, we may have ulmin or humic acid converted into glucic acid, formic acid or sugar.

By the action of the atmosphere, glucic acid is converted into apoglucic acid, and by the action of nitrogenized bodies it is further converted into crenic acid and apocrenic acid. Thus we see how these important organic matters may be formed from ordinary vegetable matter, by exposure to the influence of moisture, atmospheric air, and the soil.

Dumas regards vegetables as true organs of reduction or deoxidation, while animals are regarded as organs of combustion or oxidation. Thus while plants absorb carbonic acid and give out oxygen, animals absorb oxygen and give out carbonic acid gas. Plants directly or indirectly furnish us with our aliment, and after consuming it, animals render the same amount of matter to the vegetable kingdom in another form, which is better adapted to their use. Thus we see the great and mysterious circle of the revolution of organic elements is complete.

Besides the organic matters derived from vegetable mould, we must consider the action of saline matters which constitute the necessary components of plants.

We have already seen by the beautiful researches of Prof. Bailey, that nearly all kinds of forest trees contain in their bark, and enclosed in their sap-vessels, crystals of oxalate of lime, more than a million of which have been discovered in a square inch of the bark of the birch tree. This discovery is truly interesting, for it shows us another step in the process of changes of organic matter; for it is one of the easiest things for the chemist to convert sugar into oxalic acid. We must not fail to remark also, that while oxalic acid is a deadly poison, oxalate of lime is perfectly harmless; so that here we have this powerful acid in chains. What function this salt performs in vegetation is yet unknown.

When we examine any kind of grain or any seed which is not charged with oil so as to prevent an examination by the test we shall now describe, it is discovered that the seeds contain phosphates of certain bases. During the spring of 1840, Dr. J. discovered the presence of phosphoric acid in Indian corn, wheat, oats, beans, peas and chestnuts. This was done by burning away the seeds by heat and nitric acid, when in several cases, glacial phosphoric acid and phosphates of lime and magnesia were discovered. In his first operations, the whole seed was burnt in order to obtain the ashes for analysis, but having been shown by Mr. A. A. Hayes, specimens of Indian corn which were soaked in a weak solution of sulphate of copper, whereby the precise limits of the phosphates were shown by the formation of phosphates of copper, he was induced to examine the situation of these matters by this test; and then by dissecting out the organs of the plant which contained the phosphates, he was enabled to analyze them in a more thorough manner, so as to arrive at interesting and important results.

Specimens of various seeds were exhibited to the Association, which demonstrated the presence of phosphates in the cotyledons only. This was stated to be a general fact in every case where the experiment had been tried. The presence of the salts above noticed is a most important discovery. It explains the origin of the bones of animals.

Around the cotyledon of Indian corn, Mr. Hayes discovered a layer of a salt of peroxide of iron. This was also demonstrated by examples shown to the Association. This iron shield around the cotyledon of corn is not to be overlooked, for it is the source of the oxide of iron which enters into the composition of the red globules of the blood of animals.

Indian corn also contains a fat oil which exists in the transparent hard portion of the corn, combined with starch and a peculiar nitrogenized body called zeine. This serves to form the fat of animals, and the starch and zeine form the carbonaceous compounds of the muscles and tissues.

Dr. L. C. Beck then read a paper from J. N. Nicollet, on the mineral region of the state of Missouri.

The mineral region embraced in this paper, comprises the Valleé Mines; Mines La Motte; the Pilot Knob; and the Iron Mountain. At Valleé's mines the principal ore is the common sulphuret, (galena,) which is invariably found in ferruginous clay containing concretions of argillaceous oxide of iron, and lumps of the radiated and crystalline varieties of sulphuret of iron. The *lead-bearing clay* is at irregular depths from the surface, and is of variable thickness; under it is a limestone. The main shaft of this mine is sunk about two hundred feet, and the lode is computed to be about four hundred feet above the Mississippi, the level of the country being six hundred feet above the same.

The following section furnished by the superintendent of the mines, shows the descending order and thickness of the several beds.

1.	Ferruginous clay mixed with rubble, - -	30	feet.
2.	Red clay mixed with large masses of rock requiring blasting, - - - - - - -	130	"
3.	Red clay similar to No. 1, in the seams of lead,	70	"
4.	Rock masses of limestone, - - - -	4	"
5.	Lode, in red clay, of indifferent thickness, say	40	"
		274	"

Pursuing a general direction, at the bottom of the shaft, the ore is found to swell out laterally into pouches, or branches off so as to admit of lateral openings; so that the whole extent of the galleries is estimated at fifteen miles under ground. Over the surface, the lead region may be distinctly traced.

Tuesday morning, *May* 2.—The secretary read a paper by Mr. Nicollet, on the earthquake of New Madrid.

Prof. H. D. Rogers communicated to the Association the results of his researches in relation to the recent earthquakes, and gave an outline of a theory of earthquake action, by which he and his brother Prof. W. B. Rogers propose to explain the forces concerned in the formation of anticlinal flexures, and to account for several other dynamic phenomena in geology.

The characteristic features of earthquake motion, were shown to consist, as originally stated by Michell, in a peculiar rapid undulation or wave-like rocking of the ground, and a short vibratory jar or tremor, the tremulous shaking seldom extending to as great a distance from the source of the earthquake as the undulation. Details of several earthquakes were quoted, to prove that the rocking motion is a true billowy undulation. Thus, during the severe shock so destructive at Hayti in May, 1842, the public square at Porto Plata was seen to undulate like the waves of the sea, the houses rocking to and fro like vessels in a storm. In the great earthquake which shook the Windward Islands on the 8th of February last, the earth in Antigua reeled to and fro for more than two minutes, and the same is mentioned in relation to the motion at Guadaloupe and every other locality, where the phenomena were carefully noted.

The captain of the British steamer Tay reports, that being off Antigua and looking towards the shore, *the hills appeared to be in motion.* During the earthquake of Conception in 1835, the motion of the ground at Valdavia, according to the interesting description of Capt. Fitzroy, was undulating and regular, like waves rolling, from west to east, but strong; and it lasted nearly ten minutes. The houses waved and cracked. The same earthquake was felt at the island of Chiloe by Mr. Douglas, who describes the motion, we are told by Mr. Darwin, as horizontal and slow, similar to that of a ship at sea, going before a high regular swell, with three to five shocks in a minute.

Prof. Rogers proceeded to show the manner in which these earthquake undulations advance, and adduced some facts from which even the amplitude of the individual waves or pulsations may be approximately computed. As a confirmation of the truth of the generalization, long ago arrived at by Michell, that the disturbance is not simultaneous over the whole region shaken, but is transmitted with a high velocity, he presented the results of an analysis of the earthquake which happened in the United States, on the 4th of January of the present year, respecting which he had collected some instructive information.

This earthquake was felt from the sea-coast of Georgia and South Carolina, to beyond the western frontier military posts, and from the latitude of Natchez to that of Iowa, a space in each direction of about eight hundred miles; and there are reasons for believing that its actual extent was considerably greater. A comparison of the dates of the shock, as felt at the numerous localities heard from, seems to settle with satisfactory accuracy, the *direction*, *velocity* and *mode of progress* of this earthquake. The facts collected from more than twenty five stations, and embodied in a tabular form, make it obvious that the shock was simultaneous throughout an elongated and narrow belt or line, ranging in a N. N. E. direction from the western edge of Alabama, nearly through Nashville and Cincinnati, being also simultaneous along every other line having with this a parallel direction; whereas no such synchronism existed, where the localities compared were situated in any other than a N. N. E. and S. S. W. position. Places lying to the W. N. W. of others invariably encountered the convulsion soonest, and by an interval of time, in this case, *strictly* proportionate to the distance.

These general facts justify, it is conceived, two important conclusions; first, that the area in agitation at any given instant was *linear*, and secondly, that the earthquake moved from W. N. W. to E. S. E., keeping parallel to itself in the manner of an advancing wave. The data brought together in the table, indicate with considerable precision the *velocity* with which the shock was propagated from W. N. W. to E. S. E. Ascertaining the moment when the belt of synchronal disturbance reached St. Louis, and that again when it coincided with a line passing through Tuscaloosa, Nashville and Cincinnati, the earthquake is found to have occupied about eight minutes and twenty four seconds in the transit, the distance passed over being about two hundred and seventy miles. From this it would appear that the velocity was about thirty two miles per minute. If in like manner the times be compared when the earthquake was at Nashville, and again in a mean position between Columbia and Charleston, it will be found to have occupied in this part of its rapid march, about eleven minutes and eighteen seconds, but the distance being three hundred and eighty miles, the velocity indicated was nearly thirty three and a half miles per minute. So close an agreement of the two sets of results, bespeaks the accuracy of the data upon which these computations are based.

In support of the above inferences respecting the direction of transmission of the earthquake, is the observed direction of the oscillating motion, which is described as having been at various localities from west to east.

The phenomena of the recent earthquake of Guadaloupe were next alluded to, as confirming in a remarkable manner the accuracy of the general laws of earthquake motion, deduced from the above described earthquake of the United States. In the West India convulsion, the velocity of transmission was about twenty seven miles per minute, the pulsation being propagated laterally from an immensely elongated axis of disturbance, extending in a N. and S. direction, through the Windward Islands, to Bermuda on the N., and to the coast of Guiana on the S.

Respecting the origin of the wave-like motion of the ground in earthquakes, some able writers have been disposed to consider it as the result of a mere tremulous jar radiated from some deep-seated focus, or line of sudden fracture, and reaching the surface at points more and more remote from the source of disturbance. All the circumstantial descriptions of the phenomena disclose, however, an essential and characteristic difference in the two motions, and plainly indicate the wave-like motion to be an actual billowy oscillation of the earth's crust. That the vibratory jar is not the cause, but itself the necessary consequence of the undulation, is apparent from the following considerations. A mere tremulous vibration transmitted along a given column through the earth's crust, would not sensibly elevate or depress the surface, since the waves of compression and dilatation among the particles of the column, would neutralize each other in their effect on the dimensions of the mass. It is difficult to conceive, moreover, how the broad, ample and comparatively slow pulsations of the earthquake can by any cumulative process, be the result of those almost infinitely more minute and rapid waves which constitute vibration in solids, and which in earthquakes are the tremulous jar, and the cause of the characteristic rumbling sound. But if it were even practicable to account for the enormous magnitude of the low, broad waves into which the crust is thrown, how shall we derive from a vibration radiated from a deep focal spot or line, their remarkable *number* and *isochronism?* A simple vibratory jar sent through the crust, if competent to produce a great wave at all, could on the hypothesis produce no more than *one*, as the result of a single concussive force, so that when these waves follow each other, at regular intervals of a quarter of a minute or more, for several minutes, we must admit that they are generated in some other manner, since there is no conceivable cause for a strictly isochronous repetition of the subterranean force.

But an objection of another kind suggests itself, in the excessively fissured and crushed condition of the strata in many regions, and their extremely heterogeneous composition, which must inevitably lead to a rapid dispersion or breaking up of all regular waves of vibration within the rocky mass.

An eminent British geologist has suggested that the undulatory motion in earthquakes, may be of the nature of the vibration in a stretched cord when it is struck; but Prof. Rogers and his brother find it difficult to imagine, if we deny the theory of a pulsating fluid under the crust, how in a mass so little homogeneous and so unelastic, nodal vibrations could take place in the solid fabric of the globe, causing waves of the height and amplitude of the earthquake undulations.

Rejecting the opinion that the vibratory jar is the cause of the undulatory motion, they deem it more in accordance with known phenomena, to recognize it as the *effect*, and to attribute the tremor to an extensive, minute fissuring and grinding together of the strata under the alternate dilatation and compression going on in every part of the rocky mass, during the undulation.

The dimensions of the individual inundations would appear to be susceptible in certain cases of direct calculation. Though the waves or temporary flexures must be of various magnitudes in different earthquakes, their amplitude in the more violent convulsions is manifestly very great. Thus taking the data furnished by Darwin in his account of the earthquake of Conception, it may be shown that the probable width of each pulsation in that instance, was at least ten geographical miles, while there is reason to conclude that in the great Lisbon earthquake, each wave of the crust had an amplitude amounting to twenty five miles.

That the wave-like motion of the earth is of the character of an actual billowy pulsation, is shown not only by the visible heaving of the ground, but by the sensations produced, and by the alternate opening and closing of enormous parallel chasms of great depth, and the direction of these, which is perpendicular to the course of the undulation. Of the manner in which the wave-like movement in earthquakes may be supposed to originate, Michell suggests, that large tracts of country may be supposed to rest on fluid lava, which when disturbed may transmit its motion through the overlying crust. Comparing the process to the waves caused in a carpet, when one end of it is lifted from the floor and suddenly brought down again, he conceives, that "a large quantity of vapor may raise the earth in a wave, as it passes along between the strata, which it may readily separate in a horizontal direction, there being little or no cohesion between one stratum and another. The part of the earth that is first raised, being bent from its natural form, will endeavor to restore itself by its elasticity, and the parts next to it having their weight supported by the vapor which will insinuate itself under them, will be raised in their turn, till it either finds some vent or is again condensed by the cold into water, and by that means prevented from proceeding any further."

Prof. Rogers and his brother propose an explanation of the origin of the pulsation which they deem more in harmony with sound dynamic considerations, and with the observed phenomena of earthquakes. Instead of supposing it possible for a body of vapor to pass horizontally between the strata, or even between the crust and the fluid lava, which at their contact must be closely entangled, they attribute the movement to an *actual pulsation* in the molten matter itself, engendered by a linear disruption of the crust from enormous tension, and the sudden or explosive escape of highly compressed steam and gaseous matter. Upon this doctrine the course of the subterranean vapors would be towards and not from the line of disruption, and the undulation of the crust would arise from the instantaneous and excessive change in the pressure on the surface of the lava mass, the operation of which would be as effectual as a sudden downward stroke in creating in the fluid a system of great oscillatory waves. The billows excited on the surface of the sea of molten lava, by the rupturing and immediate collapsing of the crust, must, it is conceived, be of the nature of progressive waves of oscillation. Generated in a group on each side of the axis of disturbance, these waves will move off in parallel order, the two belts coalescing at their extremities to form a rapidly dilating *elliptic* zone, the outline of which will mainly depend on the form and elongation of the rent. Around the extremities of the fissure, the pulsation will be feeble from the rapid *radiant* progress, in this position of the waves, and this perhaps may explain the absence of a sensible shock during the Guadaloupe earthquake, in the region N. and N. W. of Bermuda, while it was distinctly felt to a great distance in a due west direction. If the earth's crust be ruptured along a very short line, or the rent be by the orifice of a volcano, the pulsation will be approximately *circular*. Such seems to have been nearly the form of the celebrated Lisbon earthquake.

Should the line of disruption, on the other hand, be greatly elongated, and the pulsations on *one side* of it only be studied, the belt of advancing waves may seem *straight*, the apparent form of the lines of synchronous shock in the recent Mississippi earthquake.

The views here suggested of the nature and cause of the wave-like motion in earthquakes, rest upon a generalization which the authors of this communication regard as one of the soundest deductions in geology, that fluid lava underlies large regions of the earth's crust, and that this crust is of very moderate thickness. If it be conceded that the earthquake undulation and its attendant phenomena imply actual pulsation in a subjacent fluid, the whole tendency of geological fact is to demonstrate that this fluid can be only intensely heated rock or lava. And conversely, the frequent recurrence of earthquakes in every known district of the globe, and the vast distances over which these pulsations are transmitted,—in some instances more than three thousand miles,—are facts which lend strong support to the doctrine of central heat, since they indicate that the internal igneous fluid is absolutely universal.

Prof. Rogers next gave a concise description of the structural features of the great Appalachian chain of the United States, illustrating the nature of the flexures in the strata, their remarkable parallelism and great length, their distribution in groups, and the law of their successively diminishing curvation, crossing the region from southeast to northwest. Referring to the published volume of the Transactions of the Association, for a full exposition of the views of his brother and himself in explanation of the phenomena, he confined himself to shewing that the bending and elevation of strata in regular flexures or axes, is the necessary consequence of a wave-like oscillation of the crust, acting simultaneously with a horizontal or tangential pressure. The identity of the ancient undulations, thus causing permanent flexures, with modern earthquakes, was then maintained, and facts appealed to in proof that these convulsions sometimes produce permanent anticlinal arches of gentle curvature, at the present day.

Other applications of the theory of the paroxysmal undulation of the earth's crust, were then adverted to; particularly the ready explanation it affords of the remarkably wide and uniform distribution of the coarse materials in some of our rocks of mechanical origin. It was argued, that to no aqueous action less extensive than that of an inundation as broad and diffused as an earthquake, can we attribute the strewing of the great sheets of matter now forming certain conglomerates and sandstones. Repeated oscillations of the crust, if very vehement, and accompanied as they would be by some permanent elevation of the surface, might send the ocean upon the dry land, and form of the fragmentary detritus a superficial layer as broad as the area inundated.

The amazing momentum imparted by the pulsating crust to the sea above it, and the huge magnitude of the surges into which this would be thrown, would contribute greatly to the dispersion of the erratic matter. In this manner Prof. Rogers conceives we may explain, by a series of tremendous earthquakes proceeding from a high arctic latitude, the rush of mingled water, ice, and fragmentary rock, at the formation of the so called "drift" which now overspreads all the northern parts of North America and Europe. Upon this view of the origin of that interesting stratum, a competent cause is suggested for the almost *universal* scratching and polishing of the floor upon which the drift reposes, since the enormous velocity that would be imparted by the vast sea-waves to successive portions of the angular rocky matter, would naturally give the blocks and fragments great cutting power. It seems unnecessary therefore to appeal to the stranding of icebergs, for the force which scored and grooved the surface of the strata throughout the northern latitudes.

Dr. Jackson exhibited to the members several specimens of sulphuret of antimony, silver ore, blende, and the tin reduced from the ore, exhibited at the last meeting, all from the state of New Hampshire.

The attention of the meeting was then called to several points of business from the standing committee.

The following nominations for membership, were proposed and confirmed. Prof. Albert Hopkins, Schenectady, N. Y., Mr. W. J. Lettsom, British legation, Washington, D. C., Dr. Norwood, Madison, N. Y., Dr. A. Clapp, New Albany, Ind., Dr. Charles Pickering, Washington, D. C., Hon. Judge B. Tappan, Steubenville, Ohio, Mr. M. Tuomey, Petersburg, Va., Dr. J. T. Plummer, Richmond, Indiana, Erastus Smith, Esq., Hartford, Ct., Dr. Jeffries Wyman, Boston, Rev. E. H. Newton, Cambridge, N. Y., Mr. Geo. Gibbs, Mr. Wolcott Gibbs, and Dr. J. R. Chilton, New York, Prof. D. Olmsted, New Haven, Ct., Dr. Wm. M. Carpenter, and Dr. J. L. Riddell, New Orleans, Mr. Root, Syracuse, N. Y., Dr. Charles Page and Hon. H. L. Ellsworth, Washington, D. C., Lt. Ruggles and Col. H. Whiting, Detroit, Mich., Mr. J. G. Anthony, Cincinnati, Ohio, Dr. William Darlington, Westchester, Pa., Mr. R. Buchanan, Cincinnati, Ohio, Dr. J. P. Kirtland, Geo. H. Cooke, Dr. John Wright, Troy, N. Y.

Resolved, That all papers read at the present session of this Association, be handed as far as practicable to the Secretary, before the rising of the Association.

Resolved, That at all future sessions of this Association, the various papers read, and reports submitted to the session, shall be considered, from the time of their presentation, as the property of the Association, and shall be delivered by the author to the secretary at the time.

Resolved, That the several committees appointed at the last session of the Association, which have not reported or have not been remodeled, be continued and their several members be requested to report at the next session.

Resolved, That this Association hold its next annual meeting at the city of Washington, on the second Wednesday of May, 1844.

The following were the officers elected for the next meeting of the Association.

Chairman.—Dr. John Locke, Cincinnati, Ohio.

Secretary.—Dr. David Dale Owen, New Harmony, Indiana.

Treasurer.—Dr. Douglass Houghton, Detroit, Michigan.

Local Committee.—Hon. H. L. Ellsworth, Prof. F. Hall,* Francis Markoe, Jr., Dr. Chas. Pickering, J. D. Dana.

Standing Committee.—Same as last year, viz. Prof. E. Hitchcock, Dr. Ducatel, Dr. C. T. Jackson, Dr. L. C. Beck, L. Vanuxem, Dr. J. B. Rogers, Prof. J. W. Bailey, Prof. B. Silliman, John L. Hayes, Esq.

Resolved, That hereafter the chairman of each meeting be elected at the meeting over which he is to preside.

Resolved, That the following gentlemen be requested to prepare reports on the present state of our knowledge on the various subjects affixed to their several names, and as far as practicable, to present them to the Association at its next session.

On Fossil Corals.—James D. Dana, Dr. A. Clapp, and John Gebhard, Jr.

On Fossil Foot-marks.—Prof. E. Hitchcock.

On Fossil Crustacea and Crinoidea.—James Hall.

On American Fossil Botany.—H. D. Rogers, J. E. Teschemacher, and J. W. Bailey on the microscopic portion.

On American Forest Trees and their distribution.—Geo. B. Emerson.

On American Cryptogamia.—Rev. Mr. Russel, Mr. Edward Tuckerman, Jr.

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* Since deceased.

On Entozoa.—Dr. J. Wyman.

On the Fossil Osteology of North America.—Dr. J. Wyman.

On the Geological distribution of Minerals.—J. D. Dana.

On the Chemical relations of the American Coals.—W. B. and R. E. Rogers.

On the Chemical and Economical Relations of the Green Sand of the United States.—J. B. Rogers and C. T. Jackson.

On the Native Compounds of Lime, Magnesia, Iron and Manganese.—Martin H. Boyé and James C. Booth.

On the Evaporating Power of various Coals.—W. R. Johnson.

On the Comparative Ichthyology of the Coast of North America and Europe.—D. H. Storer.

On the Fossil Fishes of the United States.—John H. Redfield.

On Volcanic Phenomena and the Distribution of Volcanoes.—J. L. Hayes.

On Drift Phenomena.—Prof. E. Emmons, for the New England states and New York; J. N. Nicollet,* for the West and far West; W. B. Rogers, for the Southern boundary.

Afternoon.—*Mr. Hall* read a paper upon the Crinoidea of the rocks of New York, their geological and geographical distribution.

These may be regarded as the most singular and beautiful fossils of our older rocks. When found in all their gorgeous perfection, they remind one of the fanciful creations of some fairy tale; and the glowing descriptions of the coral groves of our tropical seas can be in some degree appreciated.

If we find their external and general characters beautiful, their more minute and intricate structure is often still more curious and interesting.

Mr. H. referred to the ingenious work of Miller, published in England in 1821, as the groundwork on which we are enabled to found generic distinctions. Mr. Say's descriptions of the Caryocrinus ornatus and C. loricatus are almost the only scientific descriptions of Crinoidea with which I have met. Some notices and figures of other species from different parts of the country, serve to show how rich are our rocks in these remains.

Here follows a short description of the general structure and habits of these animals. In some species the mouth, composed of small triangular plates, shows a close analogy with the Echinidea. The delicate structure of this class of animals, rendered them peculiarly liable to destruction upon slight causes; and their immense numbers are only

* Since deceased, Sept. 11, 1843.

attested by their comminuted remains, which in some places constitute thick masses of limestone. They are likewise scattered throughout the calcareous shales, and commenced their existence at a very early period in the earth's history, and their remains are found in greater or less abundance, from the calciferous sandrock through all the formations of the New York system.

In the lower rocks, these remains, with one or two exceptions, are so far obliterated as not to be referable to their appropriate genera. It is not until we reach the rocks of the Niagara group, that we find them in any degree of perfection. In the finely comminuted homogeneous mud deposit forming the lower member of this group, these animals flourished in great numbers and equal perfection. There are in this group more ascertained species than in all the rocks of New York besides, no less than nine being already established. Several of these are of genera unknown elsewhere, and are therefore interesting from their unique character.

Here followed a description with figures and specimens illustrating the structure of the Caryocrinus ornatus, the Hypanthocrinites decorus and H. cœlatus, the Cyathocrinites pyriformis, and several other species. Some of these were exceedingly beautiful and delicate in their structure. Several other forms from the Hamilton, Portage and Chemung groups were also exhibited, some of them of great beauty and perfection.

In their geological range, these fossils appear to be more limited than any other forms; and so far as my observation extends, not a single form is known to extend beyond the rock or group of which it is typical. Their fragile nature appears to have been such, that they were unable to withstand any great changes in the condition of the ocean bed, and when a quiet deposit is succeeded by one made in a more disturbed sea, they seem all to have perished. This was shown in the Niagara group, in the transition from the calcareous mud to limestone.

In all the distinct masses forming the Helderberg series of limestones, we find these remains to change with the commencement of a new deposit; and though often only fragments occur, they are yet sufficiently characteristic. When the shales of the Hamilton group succeed these limestones, we find a new and distinct creation of these animals, differing from those of the rocks below. Among these are many beautiful forms, though they are for the most part imperfect. In this group commences the Pentremite, and an allied genus, the Nucleocrinus.

In the Portage and Chemung groups of rocks, these remains are likewise entirely different from any of those below, and different from each other. Though the materials of the two groups are similar, there are no remains continued from the lower to the higher.

Geographical distribution.—From the extensive destruction of these animals, and the transportation of their broken remains over large areas, it is impossible to determine what has been their original extent when living. The fragments have doubtless been often drifted to great distances from their original places of growth. Wherever they are known to exist in any degree of perfection, their geographical extent is very limited. Of the nine or ten species known in the Niagara group, not more than one is known to range beyond a distance of forty or fifty miles, and this one not more than sixty or seventy. Nearly all of them are confined to the space of a few miles. Certain situations appear to have been favorable to their growth, and though the nature of the deposit may appear equally uniform for a much greater distance, they do not occur.

In the limestones of the Helderberg series, their remains are widely distributed, but from their being usually in fragments, the fact furnishes no argument that they were thus widely distributed when living. In most cases in this series and in the Hamilton group, the perfect specimens are scarcely known beyond the single locality.

In the Portage group, the only perfect species known does not extend horizontally more than ten or twelve feet, and the place appears as if a forest of these beautiful forms had been swept down and covered with the soft mud above.

The well ascertained species of the Chemung group, are almost equally limited in their geographical range.

From all the facts collected it appears, that certain species, though preëminently typical of certain formations, cannot be relied upon over any wide area of country. Their presence may be relied upon as identifying certain formations, and their absence is not by any means to interfere with conclusions regarding identity drawn from other sources.

The natural history of this class of animals, in connection with their geological distribution, is exceedingly curious and interesting, and these few facts may serve to enlist observation upon their situation and condition in other parts of the country.

Dr. Owen read a paper on a universal system of geological coloring and symbols.

It was proposed that the three primitive colors should be adopted to represent the three great palæontological periods, viz. blue (indigo) for the primary fossiliferous; red (light red, or a carmine tint) for the secondary; yellow (gamboge) for the tertiary,—that the principal groups of these periods, when compatible with distinctness, be indicated by a variation in the tint, the intensity increasing in the descending order, and, if necessary, imparting at the same time to the limestones a bluish cast, and to the argillaceous deposits a greyish tint.

Pink (lake) was proposed for granitic rocks—greens for serpentine, greenstone and trap; the first being the light green, the last the darkest; or if particular greens for each be thought preferable, *terre verte* may represent serpentine; *sap green*, greenstone; *Prussian green*, trap; *reddish grey*, for trachyte; *light grey*, for modern lavas; *reds* with *yellow spots*, for porphyry; *grey* with *white spots*, amygdaloid. A *purple* color composed of the lake of the granite and the indigo of the lowest fossiliferous, was thought to be the most appropriate color for the metamorphic rocks. If it should be thought desirable to distinguish the different members of the metamorphic group, *neutral tint* might represent gneiss; *native ochrous purple*, mica slate; *archal*, hornblende schist; and the mixture of *indigo* and *lake* of the period clay slate and killas. *Ultramarine*, or *ultramarine* and *lake*, might represent metamorphic limestone. For the carboniferous rocks, considered by some an independent formation, *burnt sienna* was recommended, which having a decidedly red hue, would give to this formation the tint of the period to which it belongs.

In detailed sections and charts the same general system should be adhered to. If a variation of the tint should not be found sufficiently distinct in such cases, then sandstones and siliceous deposits of the first period may uniformly be indicated by *chrome yellow;* those of the second period, by *yellow ochre* or the red color of the period; those of the third period, by *gamboge yellow;* limestones of the first period may be represented by *indigo;* those of the second period, by *Prussian blue;* those of the third period, by *cobalt blue.* If possible, the demi-tints should not predominate, but rather the primitive color of the period.

All the primitive tints being transparent colors, they can be more easily laid on in a neat, delicate and uniform manner, than the opaque mineral pigments; and, if desirable, they may be glazed on the top of the lithological tints, and the general hue of the period thus imparted.

Thus, according to the plan proposed for detailed geological sections, blue will always indicate limestones or calcareous deposits; greys, argillaceous strata; greens, argillo-calcareous deposits; dark grey or black, carbonaceous beds.

In addition to this system of coloring, still further to facilitate the recognition of formations, a system of symbols was also proposed, on a similar plan to chemical symbols, taking the first letter of the name of a formation to represent it, and, when that was appropriated, the first and second, or when two or three words are employed, the first letter of each word, thus:

Granite, G.
Metamorphic rocks, M.
Gneiss, Gn.
Mica slate, M. S.

Hornblende schist, H. S.	Green sand, G. S.
Metamorphic limestone, M. L.	Chalk, Ch.
Chlorite schist, Ch. S.	Lower cretaceous, L. Cr.
Clay slate, C. S.	Middle cretaceous, M. Cr.
Cambrian rocks, C.	Upper cretaceous, U. Cr.
Silurian, S.	Eocene, E.
Old red sandstone, O. R. S.	Miocene, Mi.
Mountain limestone, Mn. L.	Older pliocene, O. P.
Millstone grit, M. G.	New pliocene, N. P.
Carboniferous rocks, Ca.	Post pliocene, P. P.
Red conglomerate, R. C.	Serpentine, Se.
Magnesian limestone, Ma. L.	Greenstone, Gr.
Zechstein, Z.	Trap, T.
New red sandstone, N. R. S.	Basalt, B.
Lias, L.	Trachyte, Tr.
Inferior oolite, I. O.	Porphyry, P.
Middle oolite, M. O.	Amygdaloid, Am.
Upper oolite, U. O.	Lava, La.
Weald, W.	

A paper was then read by Mr. Nicollet, on the mineral resources of St. Louis and its vicinity. It was then

Resolved, That this Association close its present session and adjourn te meet on the second Wednesday of May, 1844, at 10 o'clock, A. M., at Washington, D. C.

H. D. Rogers, *Chairman.*

B. Silliman, Jr. *Secretary.*

Art. XI.—*On the upright Fossil Trees found at different levels in the Coal Strata of Cumberland, Nova Scotia;* by Charles Lyell, Esq., F. G. S., F. R. S., &c.

[Communicated to this Journal by the author.]

The first notice of these fossil trees was published in 1829 by Mr. Richard Brown, in Haliburton's Nova Scotia, at which time the erect trunks are described as extending through one bed of sandstone twelve feet thick. Their fossilization was attributed by Mr. Brown to the inundation of the ground on which the forest stood. Mr. Lyell in 1842 saw similar upright trees at more than ten different levels, all placed at right angles to the planes of stratification, which are inclined at an angle of 24° to the S. S. W.

The fossil trees extend over a space of from two to three miles from N. to S., and according to Dr. Gesner, to more than twice that distance from E. to W. The containing strata resemble lithologically the English coal-measures, being composed of white and brown sandstones, bituminous shales, and clay with ironstone. There are about nineteen seams of coal, the most considerable being four feet thick. The place where these are best seen is called the South Joggins, where the cliffs are from one hundred and fifty to two hundred feet high, forming the southern shore of a branch of the Bay of Fundy, called Chignecto Bay. The action of tides, which rise sixty feet, exposes continually a fresh section, and every year different sets of trees are seen in the face of the cliffs.

The beds with which the coal and erect trees are associated are not interrupted by faults. They are more than two thousand feet thick and range for nearly two miles along the coast. Immediately below them are blue grits, used for grindstones, after which there is a break in the section for three miles, when there appear, near Minudie, beds of gypsum and limestone, and at that village a deep red sandstone, the whole having the same southerly dip as the coal at the Joggins, and being considered by Mr. Lyell as the older member of the carboniferous series.

Above the coal-bearing beds and stretching southward for many miles continuously along the shore, are grits and shales of prodigious thickness, with coal plants, but without vertical trees.

Mr. Lyell next describes in detail the position and structure of the upright trees at the South Joggins. He states that no part of the original tree is preserved except the bark, which is marked externally with irregular longitudinal ridges and furrows without any leaf-scars, precisely resembling in this respect the vertical trees found at Dixonfold, on the Bolton Railway, described by Messrs. Hawkshaw and Bowman. No traces of structure could be detected in the internal cylinder of the fossil trunks, which are now filled with sandstone and shale, through which fern leaves and other plants are occasionally scattered. Mr. Lyell saw seventeen vertical trees, varying in height from six to twenty feet, and from fourteen inches to four feet in diameter. The beds which inclose the fossil trees are usually separated from each other by masses of shale and sandstone, many yards in thickness.

The trunks of the trees, which are all broken off abruptly at the top, extend through different strata, but were never seen to penetrate a seam of coal however thin. They all end downwards, either in beds of coal or shale, no instance occurring of their termination in sandstone. Sometimes the strata of shale, sandstone and clay with which the fossil trunks have been filled are much more numerous than the beds which they traverse. In one case nine distinct deposits were seen in the interior of a tree, while only three occurred on the outside in the same vertical height.

Immediately above the uppermost coal-seams and vertical trees are two strata, probably of fresh-water origin, of black calcareo-bituminous shale, chiefly made up of compressed shells of two species of Modiola and two kinds of Cypris.

Stigmariæ are abundant in the clays and argillaceous sandstones, often with their leaves attached, and spreading regularly in all directions from the stem. The other plants dispersed through the shales and sandstones bear a striking resemblance to those of the European coal-fields. Among these are Pecopteris Conchitica, Neuropteris flexuosa? Calamites cannæformis, C. approximatus, C. Steinhaueri, and C. Nodosus, Sigillaria undulata and another species. The genera Lepidodendron and Sternbergia are also present. The same plants occur at Pictou, and at Sydney in Cape Breton, accompanied with Trigonocarpum, Asterophyllites, Sphænophyllum, and other well known coal-fossils.

The author then gives a brief description of a bed of erect Calamites, first discovered by Mr. J. Dawson, in the Pictou coal-field, about one hundred miles eastward of the Cumberland coal-measures before described. They occur at Dickson's mills, one mile and a quarter west of Pictou, in a bed of sandstone about ten feet thick. They all terminate downwards at the same level, where the sandstone rests on subjacent limestone, but the tops are broken off at different heights, and Mr. Dawson observed in the same bed a prostrate Lepidodendron with leaves and Lepidostrobi attached to its branches.

From the facts above enumerated Mr. Lyell draws the following conclusions:—

1. That the erect position of the trees, and their perpendicularity to the places of stratification, imply that a thickness of several thousand feet of coal-strata, now uniformly inclined at an angle of 24°, were deposited originally in a horizontal position.

2. There must have been repeated sinkings of the dry land to allow of the growth of more than ten forests of fossil trees one above the other, an inference which is borne out by the independent evidence afforded by the Stigmaria found in the under-clays beneath coal-seams in Nova Scotia, as first noticed in South Wales by Mr. Logan.

3. The correspondence in general characters of the erect trees of Nova Scotia with those found near Manchester, leads to the opinion that this tribe of plants may have been enabled by the strength of its large roots to withstand the power of waves and currents much more effectually than the Lepidodendra and Sigillariæ, which are more rarely found to retain a perpendicular position.

Lastly, it has been objected that if seams of pure coal were formed on the ground where the vegetables grew, they would not bear so precise a resemblance to ordinary subaqueous strata, but ought to undulate like the present surface of the dry land. In answer to this Mr. Lyell points to what were undoubtedly terrestrial surfaces at the South Joggins, now represented by coal-seams or layers of shale supporting erect trees, and yet these surfaces conform as correctly to the general planes of stratification as those of any other strata.

He also shows that such an absence of superficial inequalities and such a parallelism of successive surfaces of dry land, ought to be expected according to the theory of repeated subsidence, because sedimentary deposition would continually exert its levelling action on the district submerged.

Art. XII.—*On the Coal Formation of Nova Scotia, and on the Age and Relative Position of the Gypsum and accompanying Marine Limestones;* by C. Lyell, Esq. F. G. S. &c. &c.

[Communicated to this Journal by the author.]

The stratified rocks of Nova Scotia more ancient than the carboniferous consist chiefly of metamorphic clay-slate and quartzite, their strike being nearly east and west. Towards their northern limits these strata become less crystalline and contain fossils, some of which Mr. Lyell identifies with species of the upper Silurian group, or with the Hamilton group of the New York geologists.

The remaining fossiliferous rocks so far as they are yet known belong to the carboniferous group, and occupy extensive tracts in the northern part of the peninsula, resting unconformably on the preceding series. They may be divided into two principal formations, one of which comprises the productive coal-measures, agreeing precisely with those of Europe in lithological and palæontological character. The other consists chiefly of Red Sandstone and red marl, with subordinate beds of gypsum and marine limestone, but this series is also occasionally associated with coal-grits, shales, and thin seams of coal. A variety of opinions have been entertained respecting the true age of the last mentioned or gypsiferous formation, and it is the purport of this paper to show, first, that it belongs to the carboniferous group; secondly, that it occupies a lower position than the productive coal-measures. These last are of vast thickness in Nova Scotia, being largely developed in Cumberland County, and near Pictou, and recurring again at Sydney in Cape Breton. In all these places they contain shales, probably deposited in a fresh-water estuary, in which several species of Cypris and Modiola abound. The plants of these coal-measures belong to the genera Calamites, Sigillaria, Stigmaria, Lepidodendron, Pecopteris, Neuropteris, Sphenopteris, Nœggerathia, Palmacites, Sternbergia, Sphenophyllum, Asterophyllites, Trigonocarpum, with which are the trunks and wood of coniferous and other trees. Upon the whole nearly fifty species of plants have been detected, more than two-thirds of which are not distinguishable from European species, while the rest agree generically with fossils of the coal formation in Europe.

The internal cylindrical axis of petrified wood in the Stigmaria of Nova Scotia exhibits the same vascular structure, and the same scalariform vessels as the English specimens.

Mr. Lyell next describes the gypsiferous formation, especially the marine limestones of Windsor, Horton, the cliffs bounding the estuary of the Schubenacadie River, the district of Brookfield, and the cliffs at the bridge crossing the Debert River near Truro. Several species of corals and shells are common to all these localities, and recur in similar limestones in Cape Breton. In this assemblage of organic remains we find a Crustacean intermediate between the Trilobite and Limulus, Orthoceras, (two species,) Nautilus, Conularia, Encrinus and Cyathophyllum, besides some species of the carboniferous limestone of Europe, such as Euomphalus lævis, Pileopsis vetustus? Pecten plicatus, Isocardia unioniformis, Producta Martini, P. Scotica? Terebratula elongata, Fenestella membranacea? Ceriopora spongites, Goldf. For assistance in determining these the author has been chiefly indebted to M. De Verneuil.

The plants associated with these limestones consist of several species of Lepidodendron, Calamites, and others agreeing with carboniferous forms. With these Mr. Lyell found in Horton Bluff scales of a Ganoid fish, and in the ripple-marked sandstones of the same place Mr. Logan discovered footsteps which appear to Mr. Owen to belong to some unknown species of reptile, constituting the first indications of the reptilian class known in the carboniferous rocks. Several of the shells and corals of this group have been recognized by Messrs. Murchison and De Verneuil as identical with fossils of the gypsiferous deposit of Perm in Russia, and it had been successively proposed, (see Proceedings of the Geological Society, Vol. III, p. 712, and Mr. Murchison's Anniversary Address, Feb. 1843, Vol. IV, p. 125,) to refer these gypsiferous beds of Nova Scotia to the Trias, and to the period of the magnesian limestone. That they are more ancient than both these formations Mr. Lyell infers, not only from their fossils, but also from their occupying a lower position than the productive coal-measures of Nova Scotia and Cape Breton. In proof of this inferiority of position three sections are referred to; first, that of the coast of Cumberland, near Minudie, where beds of red sandstone, gypsum and limestone are seen dipping southwards, or in a direction which would carry them under the productive coal-measures of the South Joggins, which attain a thickness of several miles. Secondly, the section on the East River of Pictou, where the productive coal-measures of the Albion mines repose on a formation of red sandstone including beds of limestone, in which Mr. J. Dawson and the author found Producta Martini and other fossils common to the gypsiferous rocks of Windsor, &c. Some of these limestones are oolitic like those of Windsor, and gypsum occurs near the East River, fourteen miles south of Pictou, so situated as to lead to the presumption that it is an integral part of the inferior red sandstone group. Thirdly, in Cape Breton according to information supplied by Mr. Richard Brown, the gypsiferous formation occupies a considerable tract, consisting of red marl with gypsum and limestone. In specimens of the latter Mr. Lyell finds the same fossils as those of Windsor, &c. before mentioned. Near Sydney these gypsiferous strata pass beneath a formation of sandstone more than two thousand feet thick, upon which rest conformably the coal-measures of Sydney, dipping to the northeast or seaward, and having a thickness of two thousand feet.

To illustrate the gypsiferous formation the author gives a particular description of the cliffs bordering the Schubenacadie for a distance of fourteen miles from its mouth to Fort Ellis, which he examined in company with Mr. J. W. Dawson and Mr. Duncan. The rocks here consist in great part of soft red marls, with subordinate masses of crystalline gypsum and marine limestones; also three large masses of red sandstone, coal-grits, and shales. The strike of the beds, like that at Windsor, is nearly east and west, and there are numerous faults and flexures. The principal masses of gypsum do not appear to fill rents, but form regular parts of the stratified series, sometimes alternating with limestone and shale.

The author concludes by describing a newer and unconformable red sandstone, without fossils, which is seen to rest on the edges of the carboniferous strata on the Salmon River, six miles above Truro.

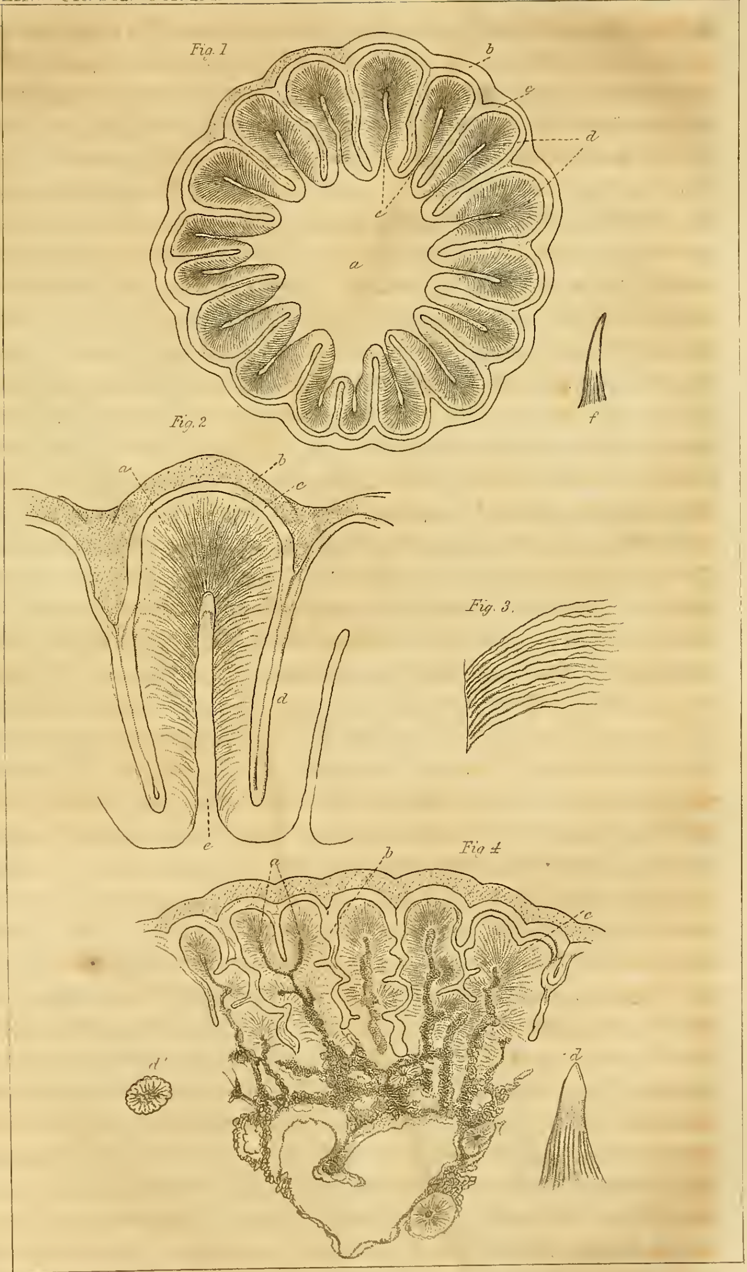
Art. XIII.—*On the Microscopic Structure of the Teeth of the Lepidostei, and their Analogies with those of the Labyrinthodonts;* by Jeffries Wyman, M. D.—(with a plate.)

[Read before the Boston Society of Natural History, August, 1843.]

The Lepidostei, like other Sauroid fishes, are provided with large conical teeth, intermixed with more numerous teeth of a smaller size. The larger teeth are found on the upper and lower maxillaries, and the intermaxillaries; the smaller ones are found on the same bones, and also on the vomer and palatines. On the two last they are arranged "en carde," except on the anterior portion of the vomer in the *Lepidosteus oxyurus*, where they are arranged in a linear series. The larger teeth, of which the microscopic structure is here more particularly described, are a little recurved, have a conical form, and sharp and slightly trenchant points; externally the surface is smooth near the apex, but more or less striated or fluted near the basal portion. The base is implanted in a cavity or alveolus, with which it is anchylosed; but from which, when shed, the teeth are detached by the absorption of their substance at the points of union.

The teeth of the *Lepidosteus platyrhinus*, Raf. and *L. oxyurus*, Raf. when cut at right angles to their axes, present a surface which is subdivided into numerous segments, by lines extending from the circumference towards the centre, the whole resembling somewhat a section of a porcupine quill. Under the microscope this appearance is seen to be the result of a peculiarity of organization hitherto undescribed in the class of fishes, and to which there is an approximation in the teeth of the Ichthyosaurus, and a still closer resemblance in those of the Labyrinthodonts. With regard to the teeth of the *Lepidosteus ferox*, Raf. I am unable to give any information, since as yet I have not been able to submit them to microscopic examination.

In the *Lepidosteus oxyurus*, where the teeth are the least complicated, (though constructed upon the same plan as in the L. platyrhinus,) the basal portion is fluted, while that near the apex is perfectly smooth, which differences correspond to others in the substance of the tooth still more striking. (See fig. 1, *f*.) Externally the tooth is covered with a layer of investing substance or "cæmentum," (figs. 1 and 2, *b*,) which follows the outline of the more internal fluted portions; and within this is a second more transparent layer, (figs. 1 and 2, *c*,) which at the space between two adjoining convolutions extends in a straight or but slightly curved line towards the centre, (fig. 2, *d*,) and being folded on itself returns again towards the circumference; and this is repeated between all the different segments. The length of the involved portion is equal to a little more than one third of the diameter of the tooth. The pulp cavity, which in the mature tooth is quite small, is characterized by the existence of radiations from its circumference towards the exterior of the tooth, terminating at equal distances from its centre, and occupying the middle of the spaces between the involutions of the cæmentum, (figs. 1 and 2, *e*.) The space comprised between the involutions of cæmentum and the radiations from the pulp cavity, is filled with "dentine," which is characterized by the existence of calcigerous tubes, of which there are highly magnified representations in fig. 2, *a*, and 3; these last in nearly all cases radiate from the



J. Wyman del.

TEETH OF LEPIDOSTEI.

[illegible]

prolongations of the pulp cavity, but very few of them coming from that cavity itself.

In the apical portion, which is perfectly smooth externally, the internal structure is much more simple than that of the basal portion which has just been described, there existing no involutions of cæmentum, and no radiations of the pulp cavity. The calcigerous tubes in the apex radiate at once from the central cavity, extending towards the circumference.

The teeth of the *Lepidosteus platyrhinus*, Raf. (fig. 4,) commonly known as the "duck-bill gar," are constructed upon the same general plan, though more complex in the details. The involuted cæmentum extends in straight lines towards the centre for a very short distance only, and then becomes more or less irregular in its course, sometimes being undulated, and at others changing its direction suddenly so as to form angles, (fig. 4, *b*.) The pulp cavity and its prolongations are also more or less irregular, according to the condition of the cæmentum, generally terminating in a simple dilatation, or, as is sometimes the case, bifurcating, as at fig. 4, *a*. In the central portion of the tooth exist also numerous pulp canals, which send off calcigerous tubes from their circumference, a conformation similar to that met with in the Rhizodus, among the extinct Sauroids.

Remarks.—In considering the structure of the teeth above described, no one can fail to recognize the analogy which exists between them and those of the Labyrinthodonts, described by Prof. Owen in his Odontography; and had we nothing but the teeth of the respective animals to which they belong with which to institute comparisons, they would both be referred at once, without doubt, to one and the same natural family. Prof. Owen does not appear to have been aware of the existence of the Labyrinthodontic structure in the Lepidostei, since in speaking of the teeth of this genus no reference whatever is made to it, and in describing the involuted cæmentum of the Labyrinthodonts he says, "such a disposition of the external substance may be traced at the base of the tooth in a few fishes, but is *more conspicuous* in the fang of the *Ichthyosaurus*."* If any one will make a comparison between the accompanying figure of the tooth of the

* Odontography, or a Treatise on the Comparative Anatomy of the Teeth, &c., by Richard Owen, F. R. S. &c. Vol. I, p. 201.

Lepidosteus oxyurus, and that of the tooth of the *Ichthyosaurus* given by Prof. Owen in his Odontography, Pl. 64 B, fig. 3, it will be at once seen that in the former there is a much closer resemblance to the Labyrinthodonts than in the latter; that is, in the *Lepidosteus oxyurus* the involutions of the cæmentum are much more extensive—although the Ichthyosaurus is described by Prof. O. as having teeth much more complicated in this respect than any existing animal.

In comparing the teeth of the Lepidostei with those of the Labyrinthodonts, those of the former will be found much less complex than in a greater portion of the latter; though quite as much and even more so, than in the *Labyrinthodon leptognathus*, figured in the Odontography, Pl. 63 B, fig. 1. Between the tooth of this last and that of the *Lepidosteus oxyurus*, there is no material difference except in the size of the pulp cavity; the radiations of this last and the involutions of the cæmentum having precisely the same relative position in both.

Thus we have the teeth of the Lepidostei, and some of the species of the Labyrinthodonts at least, reduced to the same type or plan of organization. In both the pulp cavity sends out its radiations, and in both the cæmentum is more or less prolonged inwards, at regular intervals subdividing the tooth into numerous sections. The calcigerous tubes in both cases are directed from the rays of the pulp cavity towards the investing cæmentum and its involutions.

The question very naturally presents itself, whether some of the fossil teeth from the Warwick sandstone in England and the Keuper in Germany may not be referred to some extinct Sauroid fish, rather than the Labyrinthodonts, according to the views of Prof. Owen. The former existence of gigantic Batrachian reptiles it is presumed will not be doubted, since it is based upon osteological evidence which it is impossible to controvert. But Prof. Owen informs us that in many instances the teeth from both the formations above mentioned, which were submitted to him for examination, were either mere fragments, or teeth detached from the jaws on which they grew. These he very naturally referred to the Labyrinthodonts, no such peculiarities of structure having been shown to exist in the other Vertebrata, excepting in a rudimentary form in the Ichthyosaurus, and the bases of the teeth of a few fishes. Since however the Lepidostei present peculiarities of structure which vary only in degree from those of the Labyrinthodonts, the existence in a fossil tooth of the Labyrinthodontic structure alone, would seem to be an insufficient character for determining to which of the two natural families, Sauroid fishes or Labyrinthodonts, it might belong. These remarks involve questions which we have no means at present of deciding, and would therefore submit them for the consideration of those favorably situated for instituting such comparisons as are necessary for arriving at correct conclusions.

EXPLANATION OF THE PLATE.

Fig. 1, transverse section of the tooth of *Lepidosteus oxyurus*, Raf.; *a*, pulp cavity; *b* and *c*, cæmentum; *d*, dentine; *e*, radiations from the pulp cavity; *f*, tooth of natural size.

Fig. 2, highly magnified segment of preceding section; *a*, dentine; *b* and *c*, cæmentum; *d*, one of the involutions of the cæmentum; *e*, pulp cavity.

Fig. 3, calcigerous tubes highly magnified.

Fig. 4, portion of transverse section of the tooth of the *Lepidosteus platyrhinus*, Raf.; *a*, one of the radiations from the pulp cavity, bifurcated at its termination; *b*, one of the processes from the cæmentum, having an undulated instead of a straight course as in the Lepidosteus oxyurus; *c*, dentine; *d* and *d'*, tooth and transverse section of the natural size.

Art. XIV.—*On Vibrating Dams;* by Elias Loomis, Professor of Mathematics and Natural Philosophy in Western Reserve College.

Sometime in the winter of 1841–2, my opinion was asked respecting a remarkable phenomenon noticed at Cuyahoga Falls, a village on the Cuyahoga River about eight miles from Hudson. The phenomenon consisted in the vibrations of the doors and windows, and other movable objects belonging to the buildings in the village. They were noticed at certain stages of the water, and at times ceased entirely. They were generally ascribed to a certain dam in the river, and various conjectures were formed as to the mode of their production. The subject was new to me, and I did not at first form any distinct idea of the phenomenon itself or its cause. Some weeks afterwards, I visited the locality, and although the water was at too low a stage to exhibit the phenomenon in question, I succeeded in obtaining a pretty good description of the facts and formed my opinion as to its cause. I published a notice of it in the Ohio Observer which led to some discussion, and brought to light several similar cases elsewhere. As I have not succeeded in finding any notice of this subject in such books as I have had the opportunity of consulting, I have thought it desirable that the facts should be placed on record. I propose therefore to communicate such information as I have been able to collect, and shall conclude with some speculations as to the cause of the phenomena.

I. *Dam at Cuyahoga Falls, Ohio.*

This dam is a portion of an arc of a circle, the convexity of course being turned up stream. It is formed of hewn oak timbers one foot square, piled upon each other in tiers, all morticed firmly together, so as to form as it were one huge plank two feet thick; twelve and a half feet in breadth, and ninety feet in length, measured not between the banks, but between the points of support. Its curvature is described with a radius of a hundred and twenty feet; that is, the arc is about one eighth of the circumference of a circle. There is an embankment of earth upon the upper side, which until recently was left in an unfinished state. The bank did not rise to the top of the dam, and sloped off very abruptly. This dam was erected in the summer of 1840. During the winter of 1840–1 there was noticed considerable rattling of the windows of the neighboring houses; a phenomenon different from what had ever been noticed before; but during the winter of 1841–2, which was a very open and wet winter, the vibrations were more remarkable and became a matter of general complaint. The doors and windows of most of the houses in the village would shake for days together violently as with the ague, and to such a degree as seriously to disturb sleep. This phenomenon was apparently somewhat capricious. After continuing for a time, perhaps an hour or a day or longer, the vibrations would suddenly cease, and after some interruption might be as suddenly resumed. The rattling of vibrating objects would frequently cease, while the vibrations could still be *felt*. A window, when *apparently* at rest, if put in motion by the hand, would continue to rattle. The buildings themselves (stone as well as wood) would vibrate with the doors and windows. This might be felt and also seen; as for example, a slender branch of a grape vine trained up against the side of a stone building, was seen to vibrate in exact time with the doors and windows.

The vibrations ceased entirely when water ceased to pour over the dam; they were also inconsiderable when the depth of water was eighteen or twenty four inches. A depth of five or six inches produced the greatest effect. The number of vibrations per second was thought to be about constant, but no accurate experiments on this point were ever made. From the best estimate I could obtain, they amounted to twelve or fifteen per second. A heavy log resting against the dam materially impaired the effect. The vibrations were seldom noticed in summer, as the water rarely run six inches over the dam; I however formed a plan during the summer of 1842, for a series of experiments during the ensuing winter and spring, to determine particularly the number of vibrations per second. I had several methods in mind to be tried if others should fail, but the one with which I expected most success was with a monochord; being simply a cat-gut, one end of which passed over a pulley and was stretched by a variable weight. The number of vibrations was too small for a musical sound, but by holding a small slip of paper near the string when vibrated, a succession of rattles is produced, which I hoped might be tuned to unison with the rattling of the windows. The vibrations of the string could be easily determined by the principles of acoustics. I was however never allowed the opportunity of testing my methods. In my first communication to the Ohio Observer, I had stated as a test of my theory, "if this dam were filled up to the top with earth it would probably cease vibrating." To my great regret the experiment was immediately tried. During the season of 1842 a large amount of rock, estimated at two hundred and fifty tons, was deposited upon the embankment. This raised the bank fully up to the level of the dam for a depth of six feet or more. Since that time the river has passed through every stage of elevation, known in ten years; from that in which you might walk with dry shoes over the entire length of the dam, to a depth of six feet on the break of the dam, which happened June 5, 1843, the greatest rise known since 1832. The result is that *the vibrations have entirely ceased.*

II. *Dam in East Windsor, Conn.*

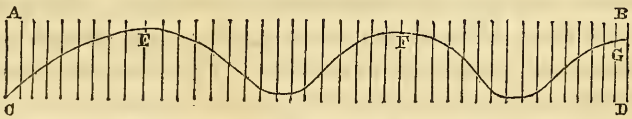
The following information is derived from a letter from Mr. M. W. Osborn, dated Florence, Erie Co. Ohio; and another from Mr. N. S. Osborn, dated Scantic, Conn. This dam is on the Scantic River, in the township of East Windsor. It was formerly eighty feet in length; but is now one hundred, and is perfectly straight. It is based on a sandstone rock, raised from six to seven feet from the rock, and about five feet from the surface of the water below the dam. The dam is a flat one, the rafters being raised not more than twenty five degrees. It was built by raising two tiers of logs, one in front and the second much lower, some ten feet back of the front one. These are bound together, and secured by ties running from one to the other. On these tiers of logs each rafter rests, with its foot on the solid rock at the bottom of the dam. The base is covered with gravel about three fifths of the distance towards the top. The vibrations are most remarkable when the sheet of water is about four or five inches deep. A gentle breeze up the stream is said to be most favorable to the vibrations, but a high wind, disturbing the falling sheet of water, in a measure destroys the effect. An unbroken sheet the whole length of the dam is necessary to get the greatest effect, and an unbroken sheet to a considerable extent to get any effect. Any article, whether light or heavy, resting on the edge or top of the dam, thereby separating the sheet of water, impairs the effect, and hence arises the practice of nailing strips of board every twelve or twenty feet to destroy the vibrations. This is an infallible remedy. Mr. Osborn has resorted to it yearly for fifty years. It has been his uniform custom to cause strips of board from six to twelve inches in width to be nailed with their ends projecting beyond the dam sufficiently far to divide the sheet of water. The width of the boards must be taken into account. It is found from experience that the narrowest ones must be placed nearest together, while the wider ones will bear to be separated a greater distance. These pieces usually become torn off during the winter and spring freshets, but the vibrations are not felt at all while the water is high, or while the sheet that pours over the dam much exceeds four or five inches in depth. A sheet of water of much more than this depth falls with a smooth unbroken surface, and without vibratory motion.

Some effect is felt when the depth of water is less than four or five inches. When the water commences pouring over the dam in a thin sheet, the tremulous motion is perceived in the falling water. This motion increases as the water descends, and a very thin sheet is soon broken and falls irregularly. As the depth of water increases, it is less and less scattered in its fall, until at length it forms an unbroken sheet. It is not until this takes place that much effect is produced on the surrounding atmosphere. The strength of the vibrations increases with the depth of water up to four or five inches; beyond this, the vibrations diminish, until at length they cease to be felt in the atmosphere, and cannot be perceived in the sheet of falling water. The tremulous motion of the water, which is greatest at the bottom of the sheet, is not confined to the falling water, but may be seen for a short distance back of the edge of the dam.

The vibrations are not at all times very perceptible to the ear. They give a fluttering sensation, like that produced by a partridge while "drumming." The number of vibrations is about five per second. A window will commence vibrating, and increase in force for five, ten or fifteen minutes, or perhaps longer, when it will gradually cease, and sometimes remain at rest for a short time. Owing, as is supposed, to the unequal thickness of the sheet of water, caused by the settling of some portions of the dam, and the breaking of some planks, the vibrations have not been experienced for the last two years, until since some recent repairs. They occur now only occasionally, and the effect is but slight in comparison with what has formerly been witnessed. The same cause has prevented them before, within the memory of Mr. Osborn. The dam has constantly been built on the same plan for fifty years or more. The most powerful vibrations ever witnessed occurred when about twenty feet of one end of the dam was about two inches higher than the rest of it. They apparently began at that end of the dam, from which they extended the whole length, when the rest of the sheet was too thick to vibrate of itself. At that time they would continue to grow more powerful until the motion was communicated to the water in the dam near to the edge, when it would cease for a few minutes. The vibrations have been known to affect windows in a house nearly one fourth of a mile distant in a northwest direction, and another a little more than one fourth of a mile in a southeast direction, the foundations of both of these buildings resting on sand. It is not recollected that any vibrations have ever been noticed in the windows on that side of Mr. Osborn's house which is turned directly from the dam.

III. *Dam at Springfield, Mass.*

The following is the substance of a letter from Mr. Amasa Holcomb, well known as the manufacturer of Herschelian telescopes. The dam crosses Westfield River about two miles west of Springfield. It is four hundred and fifty feet long and seventeen feet high, and stands on solid rock the whole length. It runs nearly north and south, and is perfectly straight and level on the top. The water is taken from the dam in a canal about eighty rods to a paper mill; and where the dam joins the canal there is a strong breastwork of stone laid without mortar. Where the dam joins the bank at the south end, there is also a breastwork of stone, with an apron of plank for a few feet, on which the sheet of water falls and turns it to the north. All the rest of the sheet falls into water of six feet or more in depth, it having torn away the rock below the dam. The dam is of wood, framed. A row of posts about three feet apart are framed into the highest plate of the dam its whole length. The upper side of the dam has a slope apparently of 40° or 50°; but on the lower side, the water falls without obstruction the whole height of the dam. The dam is built in the best manner, and was very expensive. The rafters are covered entirely with plank carefully jointed, and tight without gravel except at the bottom. There may be some gravel washed on, but there is none to be seen. The water is now (Aug. 10, 1842) running over the dam to the depth of about eight inches. The most favorable time for vibrations is when the water is a little lower, but I am told that it requires nearly the present amount to produce them. I will now describe the vibrations as intelligibly as I can. There is no wind and the surface of the pond is very smooth, perfectly so apparently, but the sheet of water is in waves, and the inequalities appear to commence immediately after the water leaves the dam. Standing on the stone breastwork at the north end of the dam, and looking south in the range of the dam, the top of it appears like a long stick of timber under water with the ends fast, but vibrating horizontally four or five inches in the middle. This apparent vibration is no doubt caused by the shape of the surface of the water. Standing on this breastwork, my clothes shook and kept exact time with the vibrations. Standing on the canal bank of earth a few rods below, the effect on my clothes was perceptible, but much less. I was told that the vibrations were about two per second, but I found them very different. Others had not accurately observed the time, or the time is variable. There are three distinct vibrations that appear to be equidistant in time, and then a space nearly equal to two, and this in constant succession as observed from my station at the north end of the dam. The three vibrations with the space, occupied a second as near as I could measure. But then there appeared to be a finer set of vibrations between these, but how many is past human skill to ascertain with precision. But there is another very curious phenomenon that I observed. Standing twenty or thirty rods below (east of) the dam, where you look at the sheet of water nearly at a right angle, it seems to flash like lightning in dry clouds, or northern lights when there is a brilliant display of electrical flashes. This is evidently owing to the sheet of water partially breaking, and consequently looking white, while some part at the same time remained unbroken. I cannot describe this without a figure.



A B represents the top of the sheet of water; C D the bottom; the curved line above E, a kind of arch; that above F, one somewhat shorter, and at G, a part of an arch. Standing down the river and looking at the sheet, that part at G turns white or partially breaks, and the change begins at the bottom and runs up nearly to the top of the sheet. Then the part at F changes in the same manner, running rapidly from the bottom to the top, and then the part at E in the same manner. Then commencing again at G, the same order is observed without the least apparent variation. There is some change observed in that part above the curved line, but small, especially in that part near the middle of the dam, or between E and F. The changes at G, F, and E, correspond in time precisely with the vibrations. I mean by this, that the water breaks or changes white once in a second at

G, and the same at each of the other places, making three distinct vibrations in a little more than half a second, and then a longer space as before observed. The doors and windows shake in Springfield, two and a half miles from the dam.

IV, V. *Two dams in Northampton, Mass.*

The following information was furnished by Professor Hitchcock. The vibrations and jarring have occurred at two dams in Northampton, about a hundred rods apart on the same stream, running along the south side of the village. Both of them are built of wood; that is, of timber with planks pinned to them, and the top is straight. The upper one is two hundred feet long, and twelve feet high; the lower one about the same length, and six or seven feet high. Both of them rest on solid strata of red sandstone, which have an easterly dip of 15° or 20°. The vibrations occur only at a particular height of the water. If the sheet is not continuous they do not take place; nor if it be quite thick at high water. As the water was not at the proper pitch either time when I visited the spot, I have not witnessed the effects.

I was assured that the jarring of houses was sensible through the whole village, which must be over a distance of half a mile. Some of the near neighbors found the effect so annoying from the lower fall, that they threatened prosecution. The proprietors knew not what to do; when happily a log floated against the dam and stopped the vibrations. The hint was improved, and a piece of plank fastened to the top of the dam, so as to break the sheet, and since that time they have not been noticed. They are still common at the upper dam, and I was told by an intelligent man who lived upon the bank, that the vibrations could be distinctly seen, not only on the curve of the descending sheet, but extending as waves a considerable way up stream. He informed me also that he first noticed these vibrations in a mill-dam of a similar kind, built upon a rock in Brattleborough, Vt. Your inquiries have also quickened my own memory, so that I distinctly recollect having frequently noticed a singular vibration in the descending sheet of water over a dam, so that I can hardly doubt but it is common. I regret that my time does not permit me to hunt up other facts of a similar character.

VI. *Dam at Gardiner, Maine.*

The following is a letter from Mr. R. H. Gardiner. The Cobbossee Contee river, a fine mill stream, empties into the Kennebeck at this village. In the last mile of its course it falls a hundred and twenty seven feet. There are now six dams across the stream, all of them built of stone, and make a fall of from twelve to twenty two feet each. This however is not the height that the water falls perpendicularly, which varies from eight to sixteen feet. All the dams are of the same construction, that is, two walls of split stone are laid and filled in between with small stones and coarse gravel, and covered on the top with flat stones seven or eight feet long, making that the width of the dam at the top, which varies at the bottom from ten to fourteen feet according to the height. The upper side is filled with earth and gravel to within two feet of the top. The wall on the lower side is nearly perpendicular, and the water falls from seven to fifteen feet on a wooden apron of timber. These dams have been erecting during the last twenty years; but what is remarkable, the vibrations were never observed until last year, although the oldest dam, and the one which has the highest perpendicular fall, is in the midst of the village. In all these dams, the portion over which the water falls is a perfectly straight line, and varies from eighty to a hundred and sixty feet in length, and the water where it runs over the dam in freshets is from three to six feet deep.

We have had no freshet this year and no vibrations. I regretted that I did not give more attention to them last year; but living more than a mile from the village, and being particularly occupied at the time, I omitted the opportunity. The vibrations were seldom heard in the day time, but regularly in the night.

The only circumstances that I am aware of that were peculiar to the last season were, 1st, the dam immediately above the lowest was built the summer preceding. The roll of this dam, over which the water falls on the apron, is the shortest in the river. Of course the water falls here with more violence. 2nd. The freshet occurred when the frost was coming out of the ground, and the earth was full of water. I could not learn that the vibrations were felt when there was much wind. The strongest vibrations were felt not close to the dams, but in some buildings (and one of them a brick hotel) erected on boggy ground near the river, but thirty rods below the lower dam, and ninety below the one most recently built. I intended to have made careful observations this season had the opportunity been afforded.

VII. *Dam at Hartford, Conn.*

The following is from Mr. J. P. Brace. About nine years ago, I lived near the mill-dam called Imlay's dam, and was much annoyed by the rattling of the windows of my house, when any quantity of water was passing over the dam. It was some time before I ascertained the cause, but after some investigation was fully satisfied it was produced by the passage of the water. The reason of my hesitation at first was the variation in the noise when the state of the dam was the same; but I soon discovered that the direction of the wind would account for this variation. The noise was produced by a rapid vibratory motion of the window sash, if slightly loose. I have stood by the dam and perceived that the impulse of the falling water gave the same motion to the contiguous air, and that the noise of the waterfall was not continuous. I never ascertained how much water was necessary to produce the effect, but I have the impression left upon my mind, that in a great freshet the vibratory motion ceased. Those who lived nearer the dam than I did were often very much annoyed. Within a few years, the dam has been taken down and built anew from its foundations. Whether the same effect now takes place, I cannot inform you.

Besides the seven cases here described, and an eighth alluded to at Brattleborough, I have heard of a ninth at Putnam in Ohio, and have also received vague intimations of several others.

Remarks.

In all these cases it is sufficiently obvious that the running water is the prime cause of the vibrations, for the vibrations invariably cease when the water ceases running. But how is the effect produced? By friction upon the dam? by collision with the air in its fall? by impulse upon the rock beneath? or in some other way?

1. *The dam itself vibrates.*—The experiment tried at Cuyahoga Falls admits I apprehend of no other explanation. Last year when the dam was comparatively free, the rattling of windows was

very annoying. But after the dam was loaded with two hundred and fifty additional tons of stone, all other circumstances remaining the same, the vibrations ceased. Moreover, direct evidence of the vibration of the dam is afforded at Scantic, (p. 367,) and at Northampton, (p. 370,) by the tremulous motion of the water extending a short distance back of the edge of the dam. The fact mentioned at Springfield, (p. 368,) of the apparent vibrations of the dam, is probably to be explained in the same way. Although refraction through the undulating surface of the water would give an apparent motion to the timbers of the dam, yet this undulating surface itself indicates real vibrations in the dam.

2. *These vibrations are excited in the dam by the friction of the running water.*—Vibrations of the dam must be communicated to surrounding objects, the air, water, earth, etc. In which of these are the vibrations first excited? Does the air communicate vibrations to the dam, or the dam to the air? This question is answered by the experiment at Cuyahoga Falls. When the dam is so loaded that it cannot vibrate, the phenomenon ceases entirely. Hence it is clear that the dam is the original vibrating body; and those who have observed the vibrations excited in elastic rods and plates by a bow, will probably have little hesitation in admitting that the running water performs the office of a bow. We may easily estimate the velocity requisite to produce the greatest effect. The depth of water at the time of the greatest vibrations is estimated at five or six inches for Cuyahoga Falls; four or five inches at Scantic; a little less than eight inches at Springfield; and at Gardiner, for a stone dam, about six feet. That is, a wooden dam requires a velocity of five or six feet per second, and a stone dam about twenty feet. The ordinary velocity of the bow upon a bass viol is perhaps one foot per second.

3. *The time of a vibration may be computed when we know the dimensions of the dam.*—From some experiments made with the largest beams I have been able to command, it is inferred that a single beam of white oak two feet thick and ninety feet long, vibrating as a whole, would make 1.5 single vibrations per second. Vibrating in two segments, it would make 6.0 vibrations, and in three segments 13.8 vibrations per second. The time of vibration is independent of the width when the beam is free; but if one edge of a long plank be confined, its number of vibrations is increased, while by loading it the number is diminished. I am un-

able to compute these effects rigorously for the Cuyahoga Falls dam, but presume that they nearly balance each other. The number of vibrations thus computed, on the supposition of three vibrating segments, corresponds very closely with the estimate on p. 365; it is hence inferred that this was the common, perhaps the only mode of vibration of this dam. I much regret that I had not the opportunity of measuring accurately the number of vibrations, and of obtaining some direct evidence of the number of nodes. At Springfield it appears pretty clear that there are two and a half vibrating segments, the half segment at the north end vibrating as if that extremity were entirely free. With a rod of uniform size and elasticity the first two segments would be of one hundred and eighty feet each, and the remaining half segment ninety feet. In a dam, these conditions of perfectly uniform thickness and elasticity could not be expected, and the segments may be considerably unequal. A prismatic beam of oak one hundred and eighty feet long and twenty two inches thick, would make one vibration per second. It is remarkable that the vibrations of the three segments appear not to be synchronous, but succeed each other at intervals of about a quarter of a second. If this observation was made while standing at the north end of the dam, a part of the retardation might be ascribed to the velocity of sound, which travels one hundred and eighty feet in about the sixth part of a second. It would seem that the intervals should be different when observed from one end of the dam, and from a station thirty rods below, though no mention is made of any such difference. This point deserves further examination.

The dam at Scantic is one hundred feet long, and makes five vibrations per second. It may hence be conjectured to vibrate in two segments. No direct evidence of this fact is furnished by the preceding statement. It is not improbable however that peculiarities may be detected in the falling sheet, similar to those at Springfield, which would indicate the position of the nodes.

4. *Why is the effect of the vibrations impaired by dividing the sheet of water?*—The facts stated on pp. 366, 370, may seem inconsistent with the idea that the dam is itself the original vibrating body. These facts do indeed indicate that the descending sheet of water has an important office to perform. This office I conceive to be that of a membrane vibrating in unison with the dam and reinforcing the sound. Behind this sheet, is a confined

body of air. The dam vibrating before this air is like a tuning fork before the open mouth of a tube or cubical box, the opposite end of which is closed by a stretched membrane. The box does not originate the vibrations, but reinforces them, so that effects are produced upon distant objects to which the dam itself would be entirely incompetent. This effect is impaired by every division of the sheet of water. It is a rent in the membrane. It is not to be supposed that the vibrations of this confined column of air correspond to the fundamental note of a tube of the same length. A segment of the dam at Springfield makes but one vibration per second, which would require an open tube eleven hundred feet in length to vibrate in unison with it.

5. *Are the vibrations transmitted to distant objects by the earth or atmosphere?*—Air is the more common vehicle of sound, yet as most of the dams here mentioned rest on solid rock, which is a good conductor, it has been supposed that this might be the principal conductor. There are some facts which seem to indicate that these vibrations are transmitted chiefly by the atmosphere. (1.) The two buildings mentioned p. 367, at Scantic, as being near the limit of this influence, both rest on a bed of sand, which is a very poor conductor of musical vibrations. (2.) The vibrations are chiefly heard on the side of buildings next the dam. This is asserted of Scantic, p. 368, and has also been asserted of Cuyahoga Falls. (3.) It is probable that the vibrations are chiefly confined to the upper edge of the dam, where the friction is applied and where the dam is more free. (4.) The facts mentioned at Springfield, pp. 368, 369, show that the stone breastwork vibrates, but the canal bank of earth a few rods below hardly at all. This shows how poor a conductor is sand, and renders it improbable that it should be the vehicle for transmitting the vibrations to the buildings mentioned p. 367. (5.) The fact mentioned at Gardiner, p. 371, respecting the place where the strongest vibrations were felt, may seem inconsistent with my position; but it is presumed that this statement was derived not from Mr. Gardiner's own observation but from the testimony of others, and it is possible that a general conclusion may have been drawn too hastily.

6. *Why is not the rattling of windows continuous, instead of being subject to frequent interruption?*—I do not understand that the dam ceases to vibrate whenever the windows cease to rattle.

Thus it is mentioned, p. 364, that a window when apparently at rest if put in motion by the hand, will continue to rattle; showing that there was a power previously acting upon the window, but not quite sufficient to overcome all the resistances; yet with a little foreign assistance at starting, the vibrations are maintained. The seemingly capricious motions of the window indicate to me only slight changes of *intensity* in the moving force, or in the amount of the resistances. Slight changes in the moving force may arise from a change in the depth of water upon the dam, due to the drain from the mills dependent upon it. Hence when the water is low, the phenomenon would more frequently happen by night than by day. Or it may be due to a real increase of water in the river, owing to showers at a distance. A change in the amount of the resistances may be due to the moisture of the atmosphere, temperature of the room, etc. Or finally, a slight change in the direction or force of the wind may occasion an appreciable difference in the strength of the transmitted vibrations; and common impression would seem to ascribe nearly the whole effect to this circumstance.

Hints to observers.

As this phenomenon is not known to have been particularly studied hitherto, and as it seems intimately connected with an important branch of science, it is hoped that it may receive some attention. I therefore propose to direct observers to some points which seem to merit particular examination.

1. The number of vibrations per second. This should be determined with the utmost accuracy, and frequently verified to see if the number is invariable. When the number does not much exceed four or five vibrations per second, they may be directly counted. With a seconds watch in your hand, count the number for one minute. Repeat the process several times, and take the mean result. Do the same every day for a long period, and preserve the separate results. See if the discordances exceed the unavoidable errors of observation. If they do not, it may be presumed the time is constant. When the number of vibrations amounts to ten or fifteen per second, some artifice must be resorted to. Compare the rattle of the windows with that produced by some known movement; e. g. a wheel moving with a known velocity, and having projecting teeth which strike upon some

light object, as a piece of paper held in the hand. The method described on p. 365 might perhaps be available.

2. A minute description of the dam is important. Its precise length, breadth, and height; material; form of construction; straight or curved; amount of embankment on the upper side, perpendicular or not on the lower side.

3. Peculiarities in the descending sheet of water, which indicate the vibrating segments. It seems improbable that a long dam should vibrate in one segment. The depth of water on the dam should be frequently measured, both during the occurrence and cessation of the phenomenon. It should be preserved in a register, stating for the same dates whether the vibrations were perceived and to what degree.

4. The direction and force of the wind should be noted whenever the vibrations are suddenly interrupted or resumed. It is also important to know what is the greatest distance at which the vibrations are ever felt? Are they perceived on all sides of the building; and upon what foundation does the building rest, rock, sand, clay or gravel.

5. It is desirable to get direct evidence of the vibrations of the dam. If several rods of an inch diameter were inserted vertically into the top of the dam, it is not improbable that by taking careful range with fixed objects the vibrations might be sensible. This method might detect not only the existence of vibrations, but the place of the nodes. In general, we might expect the middle of a vibrating segment where the current is swiftest, which is usually towards the middle of the dam. If any one should try this experiment expecting to see an oscillation of the rods through several inches, he would probably be disappointed. I should not however despair of being able to render the motion sensible. The rods should obviously be sufficiently rigid not to have a vibratory motion of their own independent of the dam.

In the speculations in which I have here indulged, I have been guided entirely by my own reflections, except so far as the general principles of musical vibrations are concerned. It is not improbable therefore that this article may contain various statements which I shall hereafter see occasion to correct. If however it should be the means of exciting the curiosity of scientific men, my main object will be accomplished.

Art. XV.—*Reply of* J. P. Couthouy, *to the accusations of J. D. Dana, Geologist to the Exploring Expedition, contained on pp.* 130 *and* 145 *of this Volume.*

Messrs. Editors—I have only this day been made aware, that in the July number of your Journal, p. 130, are the following remarks, forming part of an article "on the temperature limiting the distribution of corals, by J. D. Dana, geologist to the U. S. Exploring Expedition." "I have before stated to the Association, that the temperature limiting the distribution of corals in the ocean is not far from 66° Fahr. On ascertaining the influence of temperature on the growth of corals, I was at once enabled to explain the singular fact, that no coral occurs at the Gallapagos although under the equator, while growing reefs have formed the Bermudas in latitude 33°, four or five degrees beyond the usual coral limits. In justice to myself I may state here, that this explanation, which was published some two years since by another, was originally derived from my manuscripts, which were laid open most confidingly for his perusal, while at the Sandwich Islands in 1840. The anomalies which the Gallapagos and Bermudas seemed to present, were dwelt upon at some length in the manuscript, and attributed in the *latter* case to the influence of the warm waters of the Gulf Stream; in the *former* to the southern current up the South American coast, whose cold waters reduce the ocean temperature about the Gallapagos to 60° F. during some seasons, although twenty degrees to the west, the waters stand at 84° F." To this passage, is appended the ensuing editorial note of explanation. "The publication here alluded to we understand refers to an article by Mr. J. P. Couthouy, which appeared last year in the Boston Journal of Natural History."

I am at a loss for words that shall express the mingled feelings of astonishment, indignation and sorrow, excited in me by a perusal of the above most serious accusation, against which I claim the privilege of defending myself through your pages. In doing this I shall endeavor to be as calm and dispassionate as the case will permit. From the wording of the passage above cited, I am somewhat in doubt whether the idea intended to be conveyed by Mr. Dana, is that I am indebted to him for my views upon temperature as connected with the growth of corals, taken as a whole, or merely for the application of those views to the anomalies presented by the Gallapagos and Bermudas. To avoid any possibility of evasion however, I shall consider his charge as embracing both these points, since in another place he certainly affirms the former of them, as will be shown before I conclude.

The broad meaning of the charge is, that I am guilty of THEFT, literary larceny; of a treacherous and most dishonorable ABUSE OF CONFIDENCE, in having appropriated the ideas developed by Mr. Dana in his MSS. "confidingly laid open for my perusal." The imputations it contains are so gross, that if substantiated, they would richly warrant my ignominious expulsion, not only from the Association before which they were made, but from every other scientific body of which I have the honor to be a member. It stands Mr. Dana in need to be very sure that he can make good his assertion, since if he fails to do so, he must appear before the public in no enviable position, as guilty of having cast a foul blot upon the escutcheon of another on insufficient grounds. It must be borne in mind, that in the distribution of the various departments of natural history among the naturalists attached to the expedition, the corals were specially assigned to me. Their habits, growth, distribution and all else connected with their history, were consequently the objects of my particular attention. Traversing the same ground with Mr. Dana, possessed of equal facilities for observing the phenomena presented by corals, with the same facts presented to my notice, (and I believe all my associates, not excepting Mr. D. himself, will do me the justice to acknowledge that I neglected no occasion for investigating either,) it must I think be admitted that something more than Mr. D.'s unsupported assertion is requisite, to prove that I could only arrive at similar conclusions with himself, by meanly purloining his MS. statements. Until it could be made manifest that Mr. D. enjoyed an exclusive monopoly of the ability to deduce the views under discussion, from a study of phenomena simultaneously observed by both; I might content myself with claiming that my simple denial should be taken as an equivalent for his bare assertion, and casting (as is my undoubted right) the *onus probandi* upon his shoulders, challenge him to produce the proof of his charge. It will not suffice for him to show that the views referred to, were contained in his MSS., and that these latter were perused by or read to me at Oahu in 1840. He must prove beyond a question, that I then and there, from that source and no other, derived as he affirms, the views set forth by me in the article to which your note alludes. I have no intention however of resting satisfied with this alone. In answer then, to the accusation of Mr. Dana, I solemnly declare on my faith and honor as a man, that it is utterly and unqualifiedly destitute of the slightest foundation in truth, with the exception of the mere fact of his having laid open his MSS. for my perusal, in relation to which I cannot speak positively. At the next meeting of the Association, and in the presence, I trust, of every one who heard the remarks of Mr. Dana, this denial shall be substantiated by the most unequivocal testimony, personally should I live, and should I not, the proof shall be entrusted to others who will vindicate my memory. I will simply state at present, that so far from my having derived the opinions in question, as Mr. D. alleges, from his MSS. at the Sandwich Islands in 1840, they had at that period been several months in the possession of my friends in the United States, having been communicated from Sydney, New South Wales, in December, 1839, in substantially the same form as to facts, so far as the influence of temperature on corals is concerned, as that of their publication in January, 1842. At the time this was done, I was confined to my room by severe illness, the result of exposure, from which the physicians had pronounced recovery more than doubtful. The squadron was about sailing on a cruise whence it might never return. In the event of an unfavorable termination to either, my manuscripts might be lost, and with them whatever of new or important they contained, either of facts or suggestions. To guard against this contingency, I transmitted, by sure hand, to some friends in Boston, duplicate minutes of the most important of my observations from the time of our leaving the United States, to our arrival at Upolu in the Samoan group. These minutes were accompanied by a rigid injunction to allow no portion of them to be made public or to be perused, excepting by a few intimate friends of those to whom they were addressed, which injunction I may here add was faithfully observed. Events have proved that I acted wisely in adopting this course, since when at the trial by court martial of Lieut. Wilkes, a year ago, my journals and notes (deposited with him on my separation from the squadron at Oahu in 1840) were called for in evidence, they could no where be discovered among the other archives of the expedition, deposited at the Navy Department, and Mr. Wilkes professed entire ignorance of their fate. Neither am I aware that up to the present hour, they have been found or even sought for. But for the documents referred to, I might therefore, at this moment, stand comparatively powerless to repel the accusations of Mr. Dana.

These documents are not at present in my possession, but I pledge myself to obtain and produce them in evidence at the next meeting of the American Association of Geologists and Naturalists, before whom it appears the charge of Mr. D. was first made.

The publication of my article on coral formations in the Boston Journal of Natural History, for January, 1842, was first induced by certain statements of Mr. Lyell, in one of his lectures before the Lowell Institute, which caused considerable misapprehension as to the features of some of the Polynesian islands visited by me.

Looking upon the suggestions which had presented themselves to my mind, upon the influence of temperature on the growth of corals, as of some importance, and having learned from Mr. Lyell that Mr. Darwin was about publishing an elaborate work on the subject of their distribution, &c., I concluded, after consultation with my friends, to embrace this opportunity for a brief expression of my views, and thereby avoid being forestalled by him, in case his observations had led him to similar conclusions. Unlike Mr. Dana, I deemed it highly probable that another person, observing the same facts as myself, might draw precisely the same inferences. This was my sole motive for publication.

I will now pass to the fact of my instancing the Gallapagos and Bermudas as deviations from the general limit of coral formations. From the manner of Mr. Dana's mention of *his* remarks on these groups, one would naturally infer that they were the only anomalous cases cited by *me*, as well as by himself, and therefore to be viewed in the light of collateral evidence of my having purloined his notes. But this is far from a fair statement of the case. The former are spoken of, simply as an instance occurring in the equatorial Pacific of the same singular destitution of corals characterizing a number of the intertropical islands of the Atlantic, such as Trinidad, Martin Vas, Fernando Noronha, the Cape Verds, and Canaries, and Mr. Dana will not, I presume, include these also, among those whose anomalies "were dwelt upon at some length in the manuscript." The simple truth is, my information that the Gallapagos, and the three first named Atlantic islands, were destitute of coral, was derived on my passage from Sydney to Tahiti, in the spring of 1840, from Dr. Brown, the surgeon of the vessel, and Capt. Rugg, the commander, the latter of whom, especially had spent many months among the Gallapagos, and though utterly unacquainted with natural history, had been struck, nevertheless, with this peculiar feature, as something curious, and different from any of the other tropical islands of the Pacific, among which he had been cruising for several years. He attributed it to the sulphurous salts with which the earth at these islands is every where impregnated, affecting the water to such a degree, that corals could not live in it. I was at first inclined to coincide with him, but reflecting that this explanation could not apply to the like absence of corals in the Atlantic islands, was led to suspect that it would be found owing to the low temperature of the ocean.

This suspicion, however, I only verified while the sheets of my article were passing through the press, by an examination of the meteorological tables in the appendix to King and Fitzroy's voyage, for a knowledge of which I was indebted to my friend, Dr. A. A. Gould. From the same work, and at the same time, were derived all the local temperatures of the Pacific, specified in my article.* On the other hand, the fact of coral reefs existing at Bermuda, so far beyond their general limits, is a fact in itself so remarkable, that the most casual observer would scarcely fail to have his attention drawn to it, in connection with this subject of temperature; and I think it will be conceded, that to have passed over them in silence would have been far more extraordinary, than that I should have remarked concerning them, "though unable to speak positively from having no data; as to the Bermudas, I have no doubt from their proximity to the Gulf Stream, that they are washed by an equally warm sea."† This is the extent of my observations upon "anomalies," which were "dwelt upon at some length in the manuscript" of Mr. D., "laid open most confidingly" for my perusal at the Sandwich Islands, and thus much, I trust, it has been shown I had other means of arriving at, without abusing the confidence of Mr. D. But I

* Vide Boston Journal Nat. Hist., Vol. IV, p. 160. † Op. cit. p. 160.

must now proceed to notice another, and very important mis-statement of Mr. Dana's. On page 145 of your last number, I find the following among the "proceedings of the American Association of Geologists and Naturalists," at Albany, in April last. "Mr. Dana alluded to a statement made by Mr. Couthouy, at the meeting of the Association at Boston, that the limiting temperature of corals was 76° Fahrenheit, and took occasion to remark, that Mr. Couthouy was indebted to him (Mr. D.) for the views there advanced by him, with regard to the temperature limiting corals; and added, that the temperature 76° Fahr. was a mistake by Mr. Couthouy for 70°, the limit fixed upon by Mr. Dana when the views were communicated by him to Mr. Couthouy."

It must be admitted that the language of this accusation is sufficiently clear and explicit, as to time, place and circumstance. There seems to have been especial care taken to prevent any possibility of misapprehension as to the precise nature of the charge, and also to fix it distinctly upon me, by the frequent iteration of my name. It is open but to the solitary objection that there is not one syllable of truth in the passage from beginning to end, so far as Mr. Dana is concerned. Both the statement alluded to, and the views he represents me as having expressed in it, are entirely the creation of Mr. D.'s singularly imaginative brain. The facts set forth in the indictment on which I am thus arraigned at the bar of public opinion, are altogether fictitious. The opinions which Mr. Dana therein alleges were derived by me from his manuscript, I have never expressed either orally or in print! Incredible as this may seem, I shall now proceed to place it beyond the shadow of a doubt.

First, then, I deny that I made any statement, advanced any views or expressed any opinions whatever to the Association, upon the subject of temperature limiting corals, or upon their growth, their distribution, or in short, upon any topic connected with corals directly or indirectly.* For the evidence of this assertion, I refer to the proceedings of the Association, as published in your Journal, and in the first volume of its Transactions.

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* Perhaps I ought in so sweeping a denial, to except the remarks alluded to on page 153, Am. Jour. for July, 1842, (p. 48, Transactions of the Association,) on the evidences of successive paroxysmal elevations, presented by one of the uplifted coral islands visited by me in 1839; though these had not the slightest bearing on the question here at issue.

Omitting for the present farther comment on his assertion that the opinions here falsely quoted as mine were derived from him, I affirm in the next place, that Mr. Dana is guilty of gross and inexcusable misrepresentation of my actual views in regard to temperature limiting the growth of corals. I have never named, either directly, or otherwise, any particular standard of temperature as limiting such growth, but on the contrary, have declared that we were not yet possessed of sufficient data to establish that point, and Mr. Dana betrays that he feels the weakness of his cause, by thus ascribing to me opinions I have never entertained. I leave others to pass judgment on his motives for doing this, merely remarking, that could it be made to appear that I had named, or intended to name, as a limiting temperature, that designated by himself, it would give a coloring of probability to his charge.

Moreover, so far from specifying 76° Fahrenheit as such limit, by mistake, as he asserts, for 70° Fahrenheit, the limit assigned by him, I have in my published views expressly stated my conviction, that wherever this temperature of 76° exists, there corals will be found to flourish in their utmost profusion. In proof of this, I adduce the following extracts from my article on coral formations in the Pacific. Speaking of a reef near Tutuila, one of the Samoan group, on which were thirteen fathoms water, I remark, "This ledge, distant about two and a half miles from the coast, which was very steep, *was profusely covered with coral.* The *surface temperature* was here 81°, and that of the *bottom* 76° Fahrenheit. Throughout the Coral Archipelago to the eastward of Tahiti, the *surface temperature* ranges from 78° to 81°. The same may be said of that in the neighborhood of the detached islets, between Tahiti and Samoa, to the west. *Throughout this region, I observed all kinds of corals flourishing in perfection on the outer plateau of the reefs, at a depth of seven, eight, and in some cases, as that just cited, twelve or thirteen fathoms.*"* That I here intended to prove, that as throughout the Archipelago, where corals flourish in such perfection, the *surface* temperature is the same as at the reef off Tutuila; so also is the temperature of the *bottom*, i. e. 76° Fahrenheit,—is surely obvious, even without what here follows. "It is my belief that

* Boston Jour. Nat. Hist. Vol. IV, pp. 74, 75.

to a certain extent, the corals are limited in their range of growth by temperature rather than depth, and that wherever this is not below 76° Fahrenheit, *there, cæteris paribus, they will be found to flourish as in the Polynesian seas;*"* and again, "among the Paumotus, *the field of their most lavish display*, the temperature varies from 77° to 83°. At Tahiti, from 77° to 80°, and about the same at the large groups to the west of it. At the Hawaiian Islands, laying between 19° and 22° north latitude, it is sometimes as high as 81°. In our own hemisphere, among the Antilles, Bahamas, and southern coasts of Florida, I have found the temperature of the water near the shore, at different seasons, from 78° to 82°, *and in all these regions coral reefs abound.*"† I have italicised in the preceding quotations, the passages proving most clearly the falsity of Mr. Dana's representation of my opinions in regard to a limiting temperature, and I appeal with confidence to every candid and honorable mind, whether they do not completely disprove his assertion, that I named 76° Fahrenheit as the temperature limiting the growth of corals? whether, on the contrary, I have not specially designated it as the temperature suited for their utmost development?

Certainly, no unprejudiced person will attempt to deny, that there is a wide difference between affirming, that wherever the temperature is not below 76° Fahrenheit, *there corals will be found to flourish in perfection*—that where that exists, is "*the field of their most lavish display*," and stating that when it *is* below that, they will not grow at all. To deny this, would involve the utter absurdity of assuming that there were no intermediate grades of temperature, between the one suited to their most lavish growth, and that in which they become extinct. In this matter I claim only to be allowed to mean what I have said, in regard to temperature as limiting the growth of corals, and protest against its being assumed that I mean any thing more. I ask that the language of my article on coral formations be taken in its strict literal import, and I challenge Mr. Dana to point to a single pas-

* Boston Jour. Nat. Hist. Vol. IV, p. 76.

† Ibid. p. 160. Although it is perhaps unnecessary, I will here remark, that the temperatures here given are unless the contrary is expressly mentioned, those of the *surface*, as shown by a common thermometer placed in a bucket of freshly drawn water, or held in the sea by hand, while sailing or pulling along the reefs in a boat.

sage therein, which by any construction can be made to imply that I considered 76° Fahrenheit as the limiting temperature of corals. Unless he can do this, I maintain his affirmation that I named 76° by mistake for 70°, the limit named by him, to be wholly unfounded.

There can be no excuse or palliation offered for the conduct of Mr. Dana on this point. He was bound in honor, and by every principle of justice, to possess himself fully of my real opinions, before bringing against me an accusation of so serious import as that contained in the record. It matters not whence my views were derived, I had a right to claim at his hands a fair presentation of them to the public. It was especially incumbent on Mr. Dana while accusing *me* of behavior the most dishonorable, not to show any thing like an approach to unfairness *himself:* how much more then, to avoid attributing to me in support of his charges, in an imaginary statement, opinions which I have never expressed.

The records of the Association were upon the table before him at the time his remarks were offered; he had but to open them and ascertain that I had made no such statement as he alluded to. My published views were within his reach. A slight examination would have sufficed to convince him that I had never advanced those he attributed to me, and accused me of borrowing from his MSS. Between the time of his making the charge against me before the Association, and that of its publication in your Journal, nearly if not quite three months elapsed, and yet he made no attempt to correct his misrepresentations. How far these will strengthen belief in, or cast discredit upon the similar charge against me in the article first alluded to in this reply, it is not my province to determine.

A few words as to the mere fact of Mr. Dana's having shown me his MSS. at Oahu, in 1840. Although I have not the slightest recollection of the fact, I am perfectly willing to concede that it is very possible he did so.

That he exhibited his portfolio of *drawings* I distinctly remember, and how much I was struck in a cursory examination of them, with their wonderful beauty of coloring and minuteness of detail. During the few days we passed in company, we were both very much occupied by other matters, and what conversation or comparison of observations took place between us, was of necessity very brief and hurried. My own personal relations with the commander of the Expedition were in a painfully unsettled state, and engrossed almost my entire thoughts and attention.

If I really saw Mr. Dana's notes, it will not be considered strange, that the circumstance should have escaped my memory amid the excitement and distractions of the occurrences which led to my separation from the squadron; especially when it is understood that saving my official correspondence, I am without note or memorandum of any description, relative to the Expedition, or aught that transpired at the island, subsequent to the arrival of the first of the squadron.

Had I remembered any such expression of Mr. Dana's views as he refers to, I should gladly and unhesitatingly have cited a witness so competent, in support of my own, whose priority I had abundant means of establishing. We certainly conversed freely together on all subjects so far as we had opportunity, as was natural for persons engaged in kindred pursuits, just meeting after a year of separation. If Mr. Dana, as he alleges, (and I have no objection to admit,) submitted his MSS. to my perusal, assuredly he had as free access to mine.

I may hereafter take occasion to show that he has availed himself of them in a manner that leaves him, *to say the least*, equally open with myself to the charge of having misused confidence. My first duty will be to fully vindicate myself from the accusations he has brought against me. When this shall be accomplished, it may then be Mr. D.'s turn to act upon the defensive.

I trust that I may be pardoned for here observing, that during the whole period of my connection with the Expedition, I neglected no opportunity for noting facts, and making collections in Mr. Dana's departments, both of which were always freely turned over to him, without other return being made or sought than the satisfaction it afforded me to add my contribution to the general stock. In no solitary instance did I return from an excursion without some addition to his collections. The additions thus made by me, numbered many hundreds of specimens in both Mr. D.'s departments—a large proportion of them from localities unvisited by him. I received in exchange, at the extent, some three or four dozen specimens in my departments, and the unmerited charge of having abused his confidence. At the very time he accuses me of having done this, besides freely submitting to him all my notes on the geology of Hawaii, made during a residence of nearly six months, I placed in his hands a set of over four hundred specimens, forming a complete suite of all the formations in the group, from its northwestern to its southeastern extremity, illustrative of the facts noted, and collected along the whole course of journies over more than three hundred and fifty miles, with great care and labor, and in some instances at no slight peril to life and limb.

I have hastily drawn up this preliminary defence with feelings akin to those excited by a first perusal of the charge, not more in anger than in sorrow. I confess that after the peculiar intimacy which subsisted between us during the whole of our connection in the Expedition, after the warm expressions of regard and indebtedness on his part at the time of our separation, I was wholly unprepared for his adopting so violent a course, without a word of remonstrance or request for explanation. Had he proffered either, in lieu of assailing me unawares, while I was not present to defend myself, and leaving me to arrive at a knowledge of the fact by mere accident, I could readily have proved to him that he was under an erroneous impression, have spared much pain to one, and I cannot but hope to both of us, and prevented an act of great injustice, which he will hereafter regret no less than myself.

That he saw fit to proceed as he has done, must ever be to me a source of deep regret—but this cannot be recalled. In self defence, I am compelled to prove, that Mr. Dana is virtually a traducer, or become myself an object of scorn to all true men. I am content to abide the issue. The grounds of my defence are now before your readers. It only remains for me to assure those friends to whom the charges not herein fully disproved, have given pain, that if at the next meeting of the Association before which they were originally preferred, I fail to prove their entire groundlessness, I will consent not only to forfeit that esteem which is dearer than life, but to be branded with the full ignominy which should justly attach to conduct so unworthy as they attribute to me.

Statement of Mr. Couthouy in relation to Prof. H. D. Rogers.

Permit me before concluding, to correct an erroneous statement in the proceedings of the Association in Boston, published in your Journal for July, 1842, which by implication conflicts with the claims of another to originality in conceiving a great theory. On page 183, (page 75 also of Transactions,) it is stated, that "Mr. Couthouy read some extracts from his journal, 'on the wave-like undulations of the earth's crust, at all periods of disturbance from the most ancient date to the present time.'" I am ignorant how this came to be so worded, but it conveys an entirely erroneous idea. The extract read by me referred *exclusively* to results produced by recent volcanic action in Hawaii, strikingly illustrative on a minor scale of those grand undulations of the earth's crust, so eloquently accounted for by Prof. H. D. Rogers, on the principle of a tremendous billowy movement of the ignifluous mass beneath, at some remote period. I read nothing referring to undulations of this description as observed by myself. My remarks were introduced at the request of Prof. R. on account of their bearing on his theory, as proving that effects similar to those described by him, were produced on a diminutive scale, by a less activity of the same agent, and also to prove the singular coincidence of expression between two observers of like phenomena, placed thousands of miles apart, occurring in his notes and my own; the same comparison of the undulations to the march of ocean waves, having been made by both in terms almost *verbatim* the same.

I request you to make this correction, that it may not be hereafter surmised from the record as it now stands, that any thing advanced by me, conflicted with the claim of Prof. Rogers to entire originality in the views then presented by him. Allow me to add, that the coincidence above referred to, is not without value for its bearings on the question raised by Mr. Dana, between whom and myself the similarity of expression is far less striking than this, which was assuredly the result of mere accident.

Respectfully, your obedient servant,

JOSEPH P. COUTHOUY,

Late Mem. Scien. Corps U. S. Expl. Exped.

341 Broadway, New York, August 28, 1843.

Art. XVI.—*Experiments made with one hundred pairs of Grove's Battery, passing through one hundred and sixty miles of insulated wire*;—in a letter from Prof. S. F. B. Morse, to the Editors, dated New York, Sept. 4th, 1843.

Dear Sirs,—On the 8th of August having completed my preparations of one hundred and sixty miles of copper wire for the electro-magnetic telegraph which I am constructing for the government, I invited several scientific friends to witness some experiments in verification of the law of Lenz, of the action of galvanic electricity through wires of great lengths.

I put in action a cup battery of one hundred pairs, which I had constructed, based on the excellent plan of Prof. Grove, but with some modifications of my own, economizing the platinum.

The wire was reeled upon eighty reels, containing two miles upon each reel, so that any length from two to one hundred and sixty miles could be made at pleasure to constitute the circuit.

My first trial of the battery was through the entire length of one hundred and sixty miles, making of course a circuit of eighty miles, and the magnetism induced in my electro-magnet, which formed a part of the circuit, was sufficient to move with great strength my telegraphic lever. Even forty-eight cups produced action in the lever, but not so promptly or surely.

We then commenced a series of experiments upon decomposition at various distances. The battery alone (one hundred pairs) gave in the measuring guage in one minute, 5.20 inches of gas. When four miles of wire were interposed, the result was 1.20 inches—ten miles of wire, .57 inch—twenty miles, .30 inch—fifty miles, .094.

The results obtained from a battery of one hundred pairs are projected in the following curve.

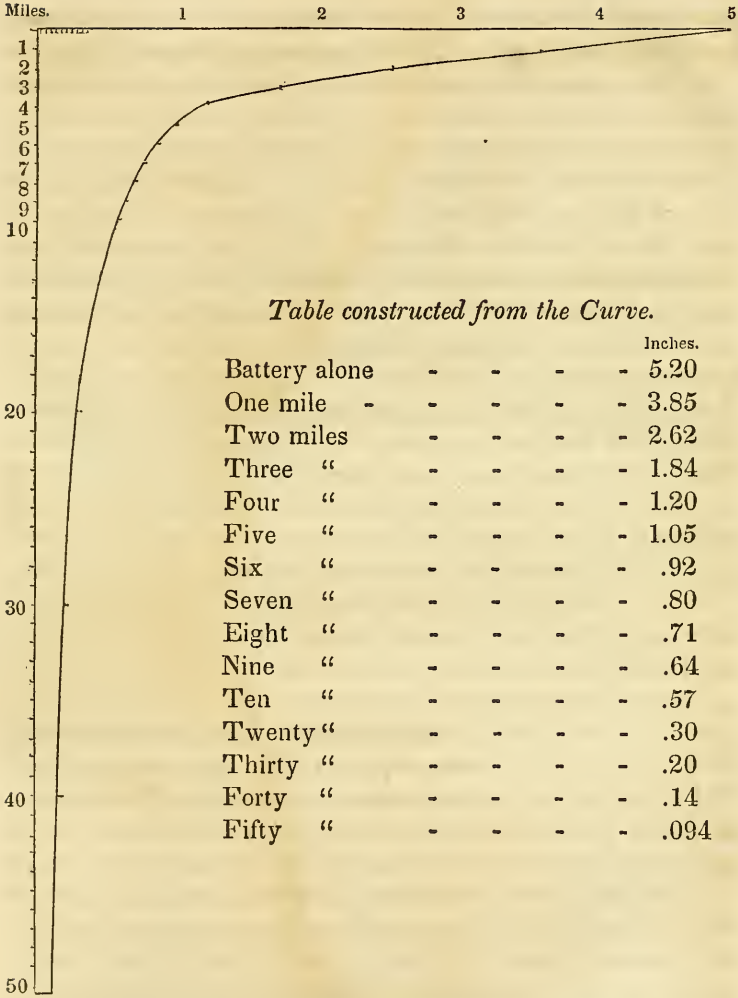


Table constructed from the Curve.

	Inches.
Battery alone - - - -	5.20
One mile - - - - -	3.85
Two miles - - - -	2.62
Three " - - - -	1.84
Four " - - - -	1.20
Five " - - - -	1.05
Six " - - - -	.92
Seven " - - - -	.80
Eight " - - - -	.71
Nine " - - - -	.64
Ten " - - - -	.57
Twenty " - - - -	.30
Thirty " - - - -	.20
Forty " - - - -	.14
Fifty " - - - -	.094

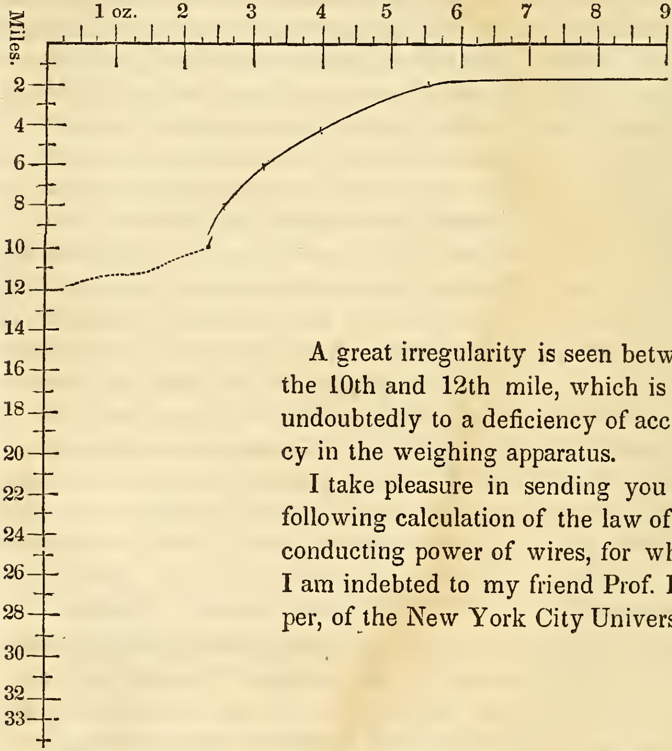
During the previous summer I made the following experiments upon a line of thirty-three miles, of number 17 copper wire, with a battery of fifty pairs. In this case, I used a small steelyard with weights, with which I was enabled to weigh with a good degree of accuracy the greater magnetic forces, but not the lesser, yet sufficiently approximating the recent results to confirm the law in question.

Table of Results.

Fifty pairs	through	2	miles	attracted	and raised	9 ozs.
"	"	4	"	"	"	4 "
"	"	6	"	"	"	3 "
"	"	8	"	"	"	$2\frac{1}{2}$ "
"	"	10	"	"	"	$2\frac{1}{4}$ "
"	"	12	"	"	"	$\frac{1}{8}$ "
"	"	14	"	"	"	$\frac{1}{8}$ "

and each successive addition of two miles up to thirty-three, still gave an attractive and lifting power of one-eighth of an ounce.

Curve from the Results.



A great irregularity is seen between the 10th and 12th mile, which is due undoubtedly to a deficiency of accuracy in the weighing apparatus.

I take pleasure in sending you the following calculation of the law of the conducting power of wires, for which I am indebted to my friend Prof. Draper, of the New York City University.

On the law of the conducting power of wires; by John W. Draper, M. D., &c. &c.

It has often been objected, that if the conducting power of wires for electricity was inversely as their length, and directly as as their section, the transmission of telegraphic signals through long wires, could not be carried into effect, and even the galvanic multiplier, which consists essentially of a wire making several convolutions round a needle, could have no existence.

This last objection was first brought forward by Prof. Ritchie, of the University of London, as an absolute proof that the law referred to is incorrect. There is, however, an exceedingly simple method of proving that signals may be despatched through very long wires, and that the galvanic multiplier, so far from controverting the law in question, depends for its very existence upon it.

Assuming the truth of the law of Lenz, the *quantities* of electricity which can be urged by a constant electromotoric source through a series of wires, the lengths of which constitute an arithmetical ratio, will always be in a geometrical ratio. Now the curve whose ordinates and abscissas bear this relation to each other is the logarithmic curve whose equation is $a^y = x$.

1st. If we suppose the base of the system which the curve under discussion represents be greater than unity, the values of y taken between $x=0$ and $x=1$, must be all negative.

2nd. By taking $y=0$ we find that the curve will intersect the axis of the x's at a distance from the origin equal to unity.

3rd. By making $x=0$ we find y to be infinite and negative.

Now these are the properties of the logarithmic curve which furnish an explanation of the case in hand. Assuming that the x's represent the quantities of electricity, and the y's the lengths of the wires, we perceive at once that those parts of the curve which we have to consider lie wholly in the fourth quadrant, where the abscissas are positive and the ordinates negative.

When, therefore, the battery current passes without the intervention of any obstructing wire, its value is equal to unity.

But as successive lengths of wire are continually added, the quantities of electricity passing, undergo a diminution at first rapid and then more and more slow. And it is not until the wire becomes infinitely long that it ceases to conduct at all; for the ordinate $-y$, when $x=0$, is an asymptote to the curve.

In point of practice, therefore, when a certain limit is reached the diminution of the intensity of the forces becomes *very small*, whilst the increase in the lengths of the wire is vastly great. It is, therefore, possible to conceive a wire to be a million times as long as another, and yet the two shall transmit quantities of electricity not perceptibly different, when measured by a delicate galvanometer.

But under these circumstances if the long wire be coiled so as to act as a multiplier, its influence on the needle will be inexpressibly greater than the one so much shorter than it.

Further, from this we gather that for telegraphic despatches, with a battery of given electromotoric power, when a certain distance is reached the diminution of effect for an increased distance becomes inappreciable.

ART. XVII.—*On the Fossil Foot-prints of Birds and Impressions of Rain-drops in the Valley of the Connecticut;* by CHARLES LYELL, Esq., V. P. G. S.*

THE deposit in which these impressions, long known on account of the researches of Prof. Hitchcock, occur, is situated in a trough of hypogene rocks, about five miles broad, the strata, which consist of sandstone, shale and conglomerate, dipping uniformly to the east at angles that vary from 5° to 30°. Mr. Lyell first examined the red sandstone at Rocky Hill, three miles south of Hartford, in Connecticut, where it is associated with red shale and capped by twenty feet of greenstone. Many of the beds are rippled, and cracks in the shale are filled by the materials of the superincumbent sandy layer, showing, the author observes, a drying and shrinking of the mud while the accumulation of the strata was in progress. The next quarries he examined were at Newark, in New Jersey, about ten miles west from New York city. The excavations are extensive, and the strata dip, as is usual in New Jersey, to the northwest, or in an opposite direction to the inclination in the valley of Connecticut, a ridge of hypogene rocks intervening. The angle is about 35° near Newark. The beds exhibited ripple-marks and casts of cracks, also impressions of rain-drops on the upper surface of the fine red shales. Mr. Lyell states, that he felt some hesitation respecting the impressions first assigned to the action of rain by Mr. Cunningham of Liverpool, but he is now convinced of the justness of the inference, having observed similar markings produced on very soft mud by rain at Brooklyn, in Long Island, N. Y. On the same mud were the foot-prints of fowls, some of which had been made before the rain and some after it.

* Communicated to the Geological Society of London, and extracted from Vol. III, No. 91, of their Proceedings. We had not seen this abstract of Mr. Lyell's paper when the article on the Ornithichnites appeared in our last number, or we should have united it with the other matter of that paper.—*Eds.*

Mr. Lyell next visited the red and green shales of Cabotville, north of Springfield in Massachusetts, where some of the best Ornithichnites have been procured, chiefly in the green shale. The dip of the beds is 20° to the east, a higher inclination, the author says, than could have belonged to a sea-beach. He observed in the same quarries ripple-marks as well as casts of cracks, and he was informed that the impressions of rain-drops have likewise been found.

In company with Prof. Hitchcock, Mr. Lyell afterwards examined a natural section near Smith's Ferry, on the right bank of the Connecticut, about eleven miles north of Springfield. The rock consists of thin-bedded sandstone with red-colored shale. Some of the flags are distinctly ripple-marked, and the dip of the layers on which the Ornithichnites are imprinted, in great abundance, varies from eleven to fifteen degrees. Many superimposed beds must have been successively trodden upon, as different sets of tracks are traced through a thickness of sandstone exceeding ten feet; and Prof. Hitchcock pointed out to the author that some of the beds exposed several yards farther down the river, and containing Ornithichnites, would, if prolonged, pass under those of the principal locality, and make the entire thickness throughout which the impressions prevail, at intervals, perhaps twenty or thirty feet. Mr. Lyell, therefore, conceives that a continued subsidence of the ground took place during the deposition of the layers on which the birds walked.

It has been suggested, but the opinion has not been adopted by Prof. Hitchcock, that the eastward slope of the beds represents that of the original beach. With a view to this question, Mr. Lyell examined the direction of the ripple-marks, and found that it agreed with the dip, or was at right angles to the supposed line of beach; but he adds, though this agreement presents a formidable objection to the suggestion above alluded to, if the ripples were produced by waves, yet it does not disprove the opinion, as the ripples do not exceed in dimensions those which are produced by sand blown over a muddy beach, and often distributed at right angles to the coast-line. Instances of this effect of the wind Mr. Lyell has remarked along the shores of Massachusetts. Nevertheless he is of opinion that the rippled layer of sandstone in question contains too much clay to have resulted from blown sand, and he is disposed to think that in most of these localities the strata have been tilted, instances of such disturbance having been pointed out to him by Prof. Hitchcock in the state of Massachusetts, and by Dr. Percival near New Haven, in Connecticut. In reference to this subject, he says, that a few miles from Smith's Ferry, a conglomerate, several hundred feet thick, containing angular and rounded fragments of trap and red sandstone, the base being sometimes a vesicular trap and trap tuff, passes upwards into the very flags on which Ornithichnites occur; and from this he infers, that there were eruptions of trap, accompanied by upheaval and partial denudation, during the deposition of the red sandstone.

With respect to the impressions having been made by birds, Mr. Lyell states, that until he examined the whole of the evidence he entertained some scepticism, notwithstanding the luminous account given by Prof. Hitchcock. In proof of their being the foot-prints of some creature walking on mud or sand, he mentions, 1st, the fact of Prof. Hitchcock's having seen two thousand impressions, all, like those he had himself examined, indented in the upper surface of the layer, the casts in relief being always on the lower surface; and 2dly, that where there is a single line of impressions, the marks are uniform in size, and nearly uniform in distance from each other, the toes in the successive steps turning alternately right and left. Such single lines, Mr. Lyell says, indicate that the animal was a biped, and the trifid marks resemble those which a bird leaves, there being generally a deviation from a straight line in any three successive prints; and his attention having been called to indications of joints in the different toes, he afterwards clearly recognized similar markings in the recent steps of coots and other birds on the sands of the shores of Massachusetts. Prof. Hitchcock has shown, that the same impression extends through several laminæ, decreasing in distinctness in proportion as the layer recedes from that in which it is most strongly marked, or in proportion as the sediment filled up the hollows and restored the surface to a level; and Mr. Lyell states, that he has observed a great number of instances of this fact.

He also says, that he can scarcely doubt that some of the impressions on the red sandstone of Connecticut are not referable to birds, but he believes that the gigantic ones described by Prof. Hitchcock are Ornithichnites. At Smith's Ferry they are so numerous that a bed of shale many yards square is trodden into a most irregular and jagged surface, so that there is not a trace of a distinct footstep; but on withdrawing from this area to spots where the same tracks are fewer, the observer, Mr. Lyell says, is forced to admit that the effect in each case has been produced by this cause.

On examining the shores on some small islands about fifteen miles southeast from Savannah, the author was struck with the number as well as the clearness of the tracks of raccoons and opossums imprinted in the mud during the four preceding hours, or after the tide had begun to ebb. At one spot, where the raccoons had been attracted by the oysters, the impressions were as confused as when a flock of sheep has passed over a muddy road; and in consequence of a gentle breeze blowing parallel to the line of cliffs composed of quartzose sand, the tracks had in many places already become half filled with blown sand,

and in others were entirely obliterated; so that if the coast should subside, the consolidation of this sand would afford casts analogous to those of Storeton Hill in Cheshire, yet the impressions had been made and filled in a few hours.

When considering the broad question whether the fossil foot-prints were made by creatures walking on mud or sand after the ebbing of the tide, Mr. Lyell reminds his readers of the fact that in the United States, as in Saxony and Cheshire, the tracks in sandstone and shale are accompanied by littoral appearances, as ripple-marks, the casts of cracks in the clay, and often by the marks of rain.

In regard to the age of the red sandstone of the valley of the Connecticut and New Jersey, the author states he has nothing to add to what had been previously advanced, by which its position had been shown to be between the carboniferous and cretaceous series. In the neighborhood of Durham, Connecticut, he had collected in the sandstone, fishes of the genera Palæoniscus and Catopterus, but no other organic remains, except fossil wood.

In conclusion, Mr. Lyell remarks, 1st, that the Ornithichnites of Connecticut should teach extreme caution in inferring the non-existence of land animals from the absence of their remains in contemporaneous marine strata; 2dly, that when this red sandstone of Connecticut was deposited, there was land in the immediate vicinity of the places where the Ornithichnites occur; and that but for them it might naturally be inferred that the nearest land was several miles distant, namely, that of the hypogene rocks which bound the basin of the Connecticut. Now, the land that caused the sea-beach, Mr. Lyell says, must have been formed of the same sandstone which was then in the act of accumulating, in the same manner as where deltas are advancing upon the sea.

In a postscript, Mr. Lyell states, that subsequently to writing the paper, he had read the luminous report of Mr. Vanuxem* on the Ornithichnites described by Prof. Hitchcock, and though it agrees in substance with his own account in some particulars, yet that he has left his notice as it stood.

Art. XVIII.—*Bibliographical Notices.*

1. *Zoology of New York, or the New York Fauna, comprising detailed descriptions of all the Animals hitherto observed within the State of New York, with brief notices of those occasionally found near its borders, and accompanied by appropriate illustrations;* by James E. De Kay. Part I, Mammalia. pp. 146, 4to, plates.—The mammiferous

* See this Journal, Vol. xli, p. 165.

animals found within the limits of the state of New York, as well as of the other eastern and middle states, had been already, for the most part, described previous to the commencement of the survey of which the present volume forms the first of a series of reports, published under the direction and at the expense of the state government. Many of the descriptions were drawn up by foreign naturalists, and in consequence of having been frequently made from stuffed skins and the incorrect information of travelers, were oftentimes erroneous. Within a few years, our own naturalists have turned their attention to the subject, and although but comparatively few additions have been made to the number of species, yet our knowledge is based upon a much more certain foundation. In the present work, the whole subject as regards the Mammalia has been gone over anew, the descriptions re-examined or rewritten, and such information added as the author, from his extended observations and long familiarity with the subject, was enabled to do. A copious synonymy has been made out, and many interesting observations brought together on the habits and geographical distribution of the different species.

The state of New York covers a large tract of territory, extending over eight degrees of longitude, and from 40° 30′ to 45° north latitude, and having an area of about 46,000 square miles. Of Mammalia inhabiting the State and indigenous to it, there are, according to Dr. De Kay, about seventy-four species, belonging to the following orders and natural families:

Order Marsupiata.—Didelphidæ, 1 species.

Order Carnivora.—Vespertilionidæ, 5 species; Sorecidæ, 8; Ursidæ, 3; Mustelidæ, 7; Lutridæ, 1; Canidæ, 4; Felidæ, 3; Phocidæ, 2. Total, 33.

Order Rodentia.—Sciuridæ, 6; Arctomidæ, 1; Gerbillidæ, 1; Castoridæ, 2; Hystricidæ, 1; Muridæ, 11; Leporidæ, 2. Total, 24.

Order Ungulata.—Elephantidæ, (fossil,) 3; Cervidæ, 5. Total, 8.

Order Cetacea.—Balænidæ, 4; Delphinidæ, 4. Total, 8.

As regards the geographical distribution, the following instances are interesting, as showing the great extent of territory over which some of the species inhabiting the State are found. The Vespertilio Noveboracensis is found throughout the territory lying between Massachusetts and the Rocky Mountains, and between the twenty-third and forty-second parallels of north latitude. Vespertilio pruinosus, Say, is found in nearly every state of the Union, on the Columbia River, and as far north as the fifty-third degree. Scalops aquaticus, or shrew mole, extends from the Atlantic to the Pacific, and from Carolina to 50° north latitude. The Procyon lotor, or raccoon, is found throughout nearly the whole of North America, its highest range as yet known being 60° north. Mephitis Americana, or skunk, is found in both Americas, extending from the frozen regions of the northern, to Chili and Paraguay in the southern hemisphere. Lutra Canadensis and Braziliensis, which Dr. De Kay regards as identical species, there being no essential differences, extends over the immense tract of country lying between the Arctic seas and Brazil.

The number of extinct mammifers whose remains have as yet been discovered within the limits of the state, is very small, and even these have been detected more abundantly elsewhere. The fossil elephant, E. primigenius, has been found in one locality only. The remains of the Mastodon giganteum, Cuvier, have been detected in several localities; but since they have not as yet been detected in Massachusetts or other eastern states, excepting Connecticut, New York may be regarded as enclosing a portion of the eastern limit.

The only other fossil mammiferous remains indicated by Dr. De Kay, are those of the fossil stag, Elaphus Americanus, which have not been detected except in one or two instances. J. W.

2. *Monographies D'Echinodermes Vivans et Fossiles;* par Louis Agassiz. Neuchatel, Suisse, 1841 and '42. 4to, planches.—Our knowledge of the Echinodermata has within a few years been much increased, especially by the labors of Muller, Tiedemann and others in Germany, and by Mr. Forbes in England. The "History of British Starfishes," by the last named naturalist, although confined to such species as are met with on the coast of England, yet may be considered as the first work in which an attempt has been made to unite in a single monograph all the different orders of the Echinodermata. In the magnificent monographs now in the course of publication by M. Agassiz and his collaborators, are included all the fossil as well as existing species hitherto known, illustrated by numerous and well executed plates. No effort has been spared to render the descriptions as complete as possible, including what has been for the most part overlooked by previous naturalists, viz. the internal organization. The importance of such a plan must be at once apparent, since it should be no less the province of the zoologist to ascertain what animals existed on the earth's surface during the early history of the world, than those which are found at the present day, and it is evident that the study of organization will be hereafter inseparable from the correct methods of zoological research.

The first monograph contains descriptions of *Salenies*, by M. Agassiz; the second, of the *Scutellæ*, by M. Agassiz; the third, *Galerites* and *Dysasters*, by Desor; and the fourth, the anatomy of the *Echini*, by G. Valentin. Of the *Scutellæ* there are described thirteen genera and seventy-four species, illustrated by twenty-five quarto plates, including several hundred figures. These descriptions are preceded by a notice of the external and internal organization, mode of growth, relations to other Clypeastroides, and their geological position and distribution. On comparing the fossil and existing species, well marked differences have been found to exist, and of the genera Mellita, Rotula and Encope, all the species belong to the actual epoch. It is also interesting to notice the fact, that among the Scutellæ, as well as some of the other Echinodermata, the species beome larger and larger as we approach the present period, precisely the reverse of what is true with regard to some of the Vertebrata, Mollusca, &c.

Previous to the labors of Valentin, Tiedemann, Meckel and Delle Chiaje had already investigated the general anatomy of the Echini, but the microscopical examinations of the former into the minute structure of the different organs, are almost entirely new. In many of the soft parts, such as the ambulacral tubes, buccal membrane, external branchiæ, &c. Valentin has discovered small calcareous bodies, assuming various shapes, resembling somewhat the spicula described by many recent microscopists, as existing in the Sponges, Alcyonias, Actinias, &c.* "Why," asks M. Valentin, "may not these minute parts be preserved in a fossil state, as well as the shell, the lantern, the teeth, and other organs? I am convinced that the microscopic palæontology of the Echinodermata will become a vast field for research." The general organization of the shell, its microscopic structure, its mechanism, its appendages, and mode of increase, are all treated of in full detail, as are also the digestive, respiratory, circulating, nervous, and generative systems. Scarcely any thing is as yet known with regard to the mode of copulation, although the duality of the sexes has long since been determined. Nearly every thing relating to the embryology of the Echini, yet remains a desideratum.

The series of monographs of which those just noticed form a part, constitute one of the most important additions which have been made to modern zoology, no less in consequence of the completeness of the plan upon which they have been conceived, than the fidelity with which they have been executed. J. W.

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* Prof. Bailey has suggested the possibility of determining the existence of the Actiniæ in the tertiary formations, by means of the minute spicula, which are the only solid parts. Quite recently I have detected spicula in the dorsal appendages of the Eolis, which resemble somewhat those met with in the Actiniæ; they are found in a small sack, recently described and figured by M. Quatrefages, though no mention is made of its contents, situated at the extremity of the appendage, opening and discharging its ~~contents~~ externally.

3. *Description of New Fresh-water and Land Shells;* by Isaac Lea.*—In the present paper Mr. Lea describes fifty four species of Unio, two of Margaritana, nine of Anadonta, one of Caracolla, one of Cyclostoma, and sixty of Melania, in all one hundred and twenty seven new species of land and fresh-water shells, described and figured by this indefatigable naturalist since the appearance of his last extended memoir on this subject, on the appearance of which it was supposed that this prolific subject was exhausted; as least so far, as that few new species of Naiades were to be looked for hereafter, and that future researches must bear mainly to the investigation of the anatomy and habits of the species already described. Some valuable light is thrown on this department of the subject in the present memoir, by the observations of Mr. Thomas G. Lea of Cincinnati, brother to our author, carried on during the years 1838, '9, and '40, on some of the species in the Ohio River, particularly in reference to the times of their parturition, which he finds to differ very much in the different species. Mr. Lea has tabulated his observations made on at least twenty five different species during each of the four years, but it will require careful and long extended observations to arrive at valuable results.

4. *Graham's Chemistry.*†—Prof. Graham's work is one of the best, if not *the* best, of all English text-books, on the difficult science of chemistry, and is of such recent date as to embrace the latest discoveries. The appearance of a correct and amended American edition under the care of Dr. Bridges, will prove an acceptable thing to both teachers and students of chemistry in this country.

MISCELLANIES.

FOREIGN AND DOMESTIC.

1. *Fossil Fruits described by Dr. Gideon Algernon Mantell.*—Dr. Mantell has recently read to the Geological Society of London, a memoir on three undescribed fossil fruits, from the chalk formation in the southeast of England.

(1.) *Zamia Sussexiensis*—a cone belonging to a plant allied to the Zamia, and found associated with coniferous wood at Selmestown in Sus-

* Transactions of the American Philosophical Society held at Philadelphia for promoting useful knowledge, Vol. VIII, new series, Part II, 1842. pp. 163–250, 4to.

† Elements of Chemistry, including the applications of the science to the arts, with numerous illustrations; by Thos. Graham, F. R. S. Lond. & Ed., Professor of Chemistry, University College, London, &c. &c. With notes and additions, by Robert Bridges, M. D. Philadelphia, Lea & Blanchard, 1843. pp. 749, 8vo.

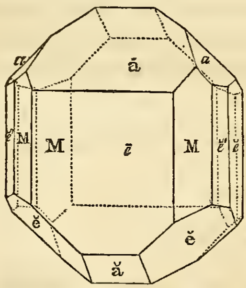
sex, in the bed described in Dr. Mantell's Fossils of the South Downs; this fruit is 5½ inches long.

(2.) *Abies Benstedi*—a fir cone found with coniferous wood in the Iguanodon quarry near Maidstone, and described in Dr. Mantell's memoir on the Molluskite, (see page 243 of the present volume.) This fossil contains numerous seeds in a fine state of preservation.

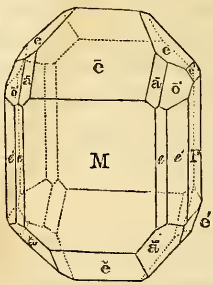
(3.) *Carpolithes Smithiæ*—a most remarkable fruit; it is the same that is described in Dr. Mantell's Fossils of the South Downs, from the white chalk near Lewes, as resembling a compressed nut of Areca. It was evidently a spurious compound berry, like the mulberry, the seeds imbedded in a pulpy substance. It was found by Mr. Smith, of Tunbridge Wells, in the white chalk of Kent.—(Letter from Dr. Mantell to the senior editor, dated Clapham Common, near London, March 30, 1843.)

2. *Eremite.*—A comparison of the angles of Eremite and Monazite, appears to indicate that these species are identical. They agree also in hardness, color, and lustre; the discrepancy in specific gravity may arise from imperfect determination, as error is scarcely avoidable in crystals so minute. The following are a few of the angles of Eremite. (See Am. Jour., Vol. XXXIII, p. 71.)

Monazite.



Eremite.



M : *e*=136° 35′, M : ē=140° 40′, M : ĕ=126° 8′, P : e=131° 52″. For the corresponding inclinations, Monazite gives (Am. Jour. XXXIII, p. 203) 136° 30′ (*ē* : M), 140° 10′ (*ē* : ā), 126° 25′ (*ē* : ă), 131° 22′ (*ĕ* : *a*). In a late article on the foreign Monazite by Delvoiseaux, in the Annales des Mines, t. II, 1842, p. 362, these angles are given as follows: 136° 30′, 141° 5′, 126°, 131°. By calculation, M : T in Eremite gave the writer, 103° 46′, and in Monazite, 103° 42′. A specimen of Eremite in the hands of Mr. Thomas Dutton, shows a cleavage similar to that of Monazite. When first described, only three or four very minute crystals had been seen, and in these no cleavage was detected. J. D. Dana.

3. *Meeting of the British Association at Dublin.*—This meeting of the Association was held during the month of August. It was a small meeting,—we do not learn that any thing very important came before it, and we are informed that party politics interfered somewhat with its success. One fact of great interest was announced. An upright trunk of a large Sigillaria has been discovered in the coal-field near Liverpool, with roots eight or nine feet long, spreading in every direction and with the radicles radiating from the main roots, and these roots and radicles are the *Stigmaria ficoides* and its leaves. This discovery must modify some existing theories on coal.*

4. *Animal of the Belemnite.*—Lord Northampton has recently obtained from the oolite of Chippenham, (Eng.) a specimen of a *Belemnite* with the impression of the soft parts of the animal on the surrounding clay! Even the little hooks with which the creature was furnished remain! Dr. Buckland's figure from D'Orbigny must therefore be modified; Prof. Owen, in the admirable volume of lectures on the Invertebrata, (the Hunterian lectures for this year, just published,) has given a restored outline of the animal of the Belemnite, and which must be correct.—(Extract from a letter to Prof. Silliman from Dr. Mantell, dated Aug. 28th.)

5. *Meteoric Epoch of August.*—In consequence of cloudy weather at this place for several days about the 10th of August, 1843, it was impracticable here to determine whether the meteoric sprinkle of August recurred the present year.

6. *Death of Mr. Bakewell.*—Robert Bakewell, Esq. died at his residence at Hampstead, near London, on the 15th of August, at the age of 75. He had long been an invalid, and his death was the result of gradual decline, rather than acute disease. Mr. Bakewell was one of the oldest of the present school of English geologists, and was the author of the first good treatise on geology in the English language, which went through five editions in England and three in this country, before the author's death; and it still holds a place among the best elementary works on the subject. Mr. Bakewell also published in 1823 two interesting and valuable volumes of travels among the Alps in Switzerland.

Mr. Bakewell's mind was distinguished for vigor, acuteness, and independence; his Geology was indeed much in advance of the science at the time he wrote, but his sagacious views have been fully confirmed. In the course of an epistolary correspondence of many years, we

* We hope to begin our usual abstract in our next number.

have found his letters rich in thought, and vivid and attractive in style, while a warm and true philanthropy imparted a living moral interest to his epistles.

A great degree of modest retirement characterized Mr. Bakewell's intercourse with society, and he carried it to such an extent as rarely to visit the sessions of any of the scientific bodies in London. This may serve to explain the fact that his treatise on geology was at first received with more favor in this country than in England.

7. *Death of Prof. Hall.*—Professor F. Hall, whose name has often appeared in our pages as a contributor of valuable matter, died during a journey at the west, in the month of August last. We have no information of the exact time of his death, nor his age, which however was not far from sixty. Prof. Hall was a zealous cultivator of mineralogy; he collected a large and valuable cabinet, which a few years since he generously gave to Dartmouth College, at Hanover, N. H., and at the same time he placed the chair of mineralogy in that institution on a permanent foundation, by the contribution of five thousand dollars in money.

8. *Death of Mr. J. N. Nicollet.*—It is also our painful task to record the decease of Mr. Nicollet, who died at Washington, D. C., on Monday morning, the 11th of September, a little after six o'clock, aged it is supposed about forty eight. Mr. Nicollet's labors in the departments of physical astronomy and geography are well known. He was the favorite pupil and friend of La Place; and the frequent occurrence of his name in the Mécanique Celeste, shows in what estimation he was held by his teacher.

Mr. Nicollet came to this country about ten years since, and has been engaged principally in carrying out a survey—geographical, topographical, astronomical, and geological—of the vast region embraced by the sources of the Mississippi and Missouri Rivers.

His map of this important labor was completed before his death, and was shown by him at the Association of American Geologists in April last, at Albany, and referred to in explanation of an interesting paper on the geology of the region in question, an abstract of which is contained in their proceedings, in the present volume of this Journal.

Mr. Nicollet also devoted much effort to the collection and preservation of the various Indian dialects, and in fact every thing which could illustrate the history of this interesting race. It is said his collections of MS. notes on this subject are quite voluminons.

All who had the pleasure of knowing him, and enjoying his fine social and moral qualities, will hear of his premature loss with deep regret.

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ACKNOWLEDGMENTS TO CORRESPONDENTS, FRIENDS AND STRANGERS.

Remarks.—This method of acknowledgment has been adopted, because it is not always practicable to write letters, where they might be reasonably expected; and still more difficult is it to prepare and insert in this Journal, notices of all the books, pamphlets, &c., which are kindly presented, even in cases, where such notices, critical or commendatory, would be appropriate; for it is often equally impossible to command the time requisite to frame them, or even to read the works; still, judicious remarks, from other hands, would usually find both acceptance and insertion.

In public, it is rarely proper to advert to personal concerns; to excuse, for instance, any apparent neglect of courtesy, by pleading the unintermitting pressure of labor, and the numerous calls of our fellow-men for information, advice, or assistance, in lines of duty, with which they presume us to be acquainted.

The apology, implied in this remark, is drawn from us, that we may not seem inattentive to the civilities of many respectable persons, authors, editors, publishers, and others, both at home and abroad. It is still our endeavor to reply to all letters which appear to require an answer; although, as a substitute, many acknowledgments are made in these pages, which may sometimes be, in part, retrospective.—*Eds.*

SCIENCE.—FOREIGN.

Recherches sur les Poissons Fossiles. Texte et Planches. Liv. 14. 1841.

Histoire Naturelle des Poissons d'Eau Douce de l'Europe Centrale. Tome I, texte, contenant l'Embryologie des Salmones. Liv. 2, planches. 1841.

Etudes Critiques sur les Mollusques Fossiles. Liv. 2, avec plan ches, contenant les Myes du Jura et de la Craie Suisses. 1842.

Monographies d'Echinodermes, Vivans et Fossiles. Liv. 2, contenant les Scutelles; liv. 3, les Galerites et les Dysaster, par E. Desor; liv. 4, l'Anatomié du genre Echinus, par G. Valentin. Planches.

Nomenclator Zoologicus, continens Nomina Systematica Generum Animalium, tam viventium quam fossilium. Fasiculus 1, continens Mammalia, Echinodermata et Acalephas. Fasciculus 2, continens Aves.—With several prospectuses. All the above from the Author, M. Louis Agassiz, Neuchatel, Switzerland.

Recit d'une course faite aux Glaciers en hiver, par MM. Agassiz et Desor. From the Authors.

Aperçu general de la Structure Geologique des Alpes, par M. Studer; precede de quelques observations generales, par M. Desor. From the Author.

Letter to Prof. Forbes concerning his alleged discovery of the "lamellar structure of glaciers." From the Author, M. Agassiz.

Evolution of Light from the Living Human Species; by Sir Henry Marsh, Bart. M. D., M. R. I. A. Dublin, 1842.

Lectures on Agricultural Chemistry and Geology. Part II. By James F. W. Johnston, M. A., F. R. S.

The Effects of a Lightning Flash on the steeple of Buxton Church, and observations on Lightning Conductors generally; by Charles V. Walker, Sec. Lond. Elec. Soc. London, May 17th, 1842. From the Author.

On the Action of Lightning Conductors. London, July 19th, 1842.

Memoir on the difference between Leyden discharges and Lightning Flashes; by Charles V. Walker. From the proceedings of the London Electrical Society. London, Dec. 20th, 1842. From the Author.

Annuaire Magnetique et Meteorologique du corps des Ingenieurs des Mines de Russie, ou recueil des observations Magnetique et Meteorologique faites dans l'entendre de l'empire de Russie, et publiees par l'ordre de S. M. l'Empereur Nicolas I. Année, 1840–1842. Received March, 1843, from M. le Comte Cancrine, chef du corps des Ingenieurs.

Introduction to the Atomic Theory; by Chas. Daubeny, M. D., Prof. Chem. Oxford, 1841.

Elements of Agricultural Chemistry and Geology; by Prof. Johnson. Second edition. London. pp. 250, 12mo. From the Author.

Revue des Fossiles du Government du Moscow, par C. Fischer de Waldheim. No. 1, Belemnites. From the Author, forwarded by Charles Cramer, Esq.

The British Quarterly Journal of Dental Surgery, edited by J. Robinson, Esq. London, March, 1843. From the Editor.

Proceedings of the London Electrical Society. Session, 1842–3. Edited by the Secretary. April. From the Society.

Received April, 1843, from Signor Giovanni Michelotti, Turin, the following works, viz.

Brevi Cenni sullo condizione attuale della Sardigna, per l'Auv. Giovanni Michelotti. Torino, 1842. Ten copies.

Cenni Statistici sopra la Ricovero di Mendicita di Torino Nell Anno, 1841. pp. 36. Two copies.

Atti della seconda riunione degli scienziata Italiani tenuta en Torino. Nel Septembre, del 1840. pp. 400, quarto, pamphlet form.

Annali della scienze del regno Lombardo Veneto, opera periodica di Alenni collaboratori. 1841, Gennajo e Febbrajo, Marzo e Aprile, Maggio e Gingno, Luglio e Agosto.

De solaris in supracretaceis Italicæ stratio repertio. Auctore Jean Michelloti. Two copies.

Saggio storico del rizopodi caracteristici del terreni sopracreta cei, per Giovanni Michelotti. Modena, 1841. Quarto pamphlet. Five copies.

Saggio orittografico sulla classe del Gasteropodi fossili del terreni terziarii del Piemonte di Luigi Bellardi e Giovanni Michelloti. Two copies.

Catologo medaglio. Musio Numismatico Lavy appartnente alla R. Academia, della scienze di Torino. In two vols., 4to. pp. 500. Two copies.

Caroli Allionii Flora Pedimontana. Three volumes.

On the Distribution and Classification of the order of the Palæozoic Deposit of the north of Germany and Belgium, and on their comparison with formations of the same age in the British Isles; by Rev. A. Sedgwick, F. R. S., and Roderick I. Murchison, Esq., F. R. S. pp. 400, quarto, pamphlet form. With a set of plates. London, 1842. From the Authors.

Address of Mr. Murchison before the Geological Society of London, Feb. 18th, 1842. From the Author.

Maidstone; its Geology, History, Antiquities and Traditions, discussed in a memoir read in that town by Douglas Allport, Esq. From Dr. Mantell.

Address before the Royal Society on its anniversary, Nov. 30, 1842, by the most noble Marquis of Northampton, president of the Society. From Dr. Mantell.

Dent on the errors of Chronometers, and explanation of a new construction of the Compensation Balance. London, 1842. Five copies. From the Author.

Dent on the construction and management of Chronometers, Watches and Clocks. Three copies. From the Author.

The Royal Society, Fellows and Council. November, 1842. Quarto pamphlet, pp. 30. From Dr. Mantell.

Extract of a letter from Prof. Hansteen, Christiana, to Prof. M. Forbes, Edinburgh. Small pamphlet. pp. 8.

The Geologist; edited by Charles Moxon, Esq., from Jan. 1st, 1842, to Dec. inclusive. From the Editor.

On the specific inductive capacities of certain electrical substances; by W. Snow Harris, Esq., F. R. S. London, 1842. Quarto pamphlet, pp. 172. From the Author.

Arsberättelse om Technologiens framsteg till Kongl. Vetenskaps Academien afgifen den 31 Mars, 1840; af G. E. Pasch. Stockholm, 1841. pp. 24.

Kongl. Vetenskaps Academien Andlingar. För Ar 1840. Stockholm, 1841.

Tal af Academiens Præses Grefve M. Rosenblad. Stockholm, 1840.

Tal om Jordbrukets närvarande tillstand inom fadernestlandet, hundren för dess fooksfran och utsigterna for dess framtid. hallet I. K. V. A. vid. Præsidii Nedlaggande den 6 April 1842, af August, Auckarsward. Stockholm, 1842, pp. 42.

Arsberättelser om nyare Zoologiska arbeten och Upptäckter, till Kongl. Vetenskaps-Academin afgifne för aren 1837–40. Af C. J. Sundewall. Stockholm, 1841. pp. 585. All from the Swedish Academy, through their perpetual Secretary, M. Jac. Berzelius.

SCIENCE.—DOMESTIC.

A Muck Manual for Farmers. Lowell, Mass. 1843. From the Author, Samuel L. Dana. 12mo. pp. 232.

Elements of Geology, with an outline of the Geology of North Carolina, for the use of students of the University; by Prof. Mitchell. 1842. Small octavo, pp. 141. From the Author.

Monograph of the Fresh Water Mollusca of the United States. No. 6. Two copies. From the Author, S. S. Haldeman, Genus Physa. Phil. 1843.

Farmer's Register, Vol. X, No. 12, containing an essay on calcareous manures; by Edmund Ruffin. Petersburg, 1842.

The Medical News Library, Vol. I, Nos. 1 and 2. Philadelphia. A continued series.

Transactions of the Society of Alumni of the College of Physicians and Surgeons of the University of the State of New York. No. 1, 1842.

Quarterly Summary of the Transactions of the College of Physicians of Philadelphia. 1843.

Transactions of the Am. Phil. Soc. Part II, Vol. 3. Observations on the species Unio. Two copies. From the Society, and I. Lea, Esq.

Binomial Theorem and Logarithms, for the use of the Midshipmen in the Naval School at Philadelphia. By and from Wm. Chauvenet. 1843. Small octavo, pp. 91.

Geological History of Manhattan or New York Island; by Issachar Cozzens, Jr. New York, 1843. pp. 114, small 8vo. From the Author.

Reports of the first, second, and third meetings of the Association of American Geologists and Naturalists at Philadelphia, in 1840 and 1841, and at Boston, 1842, embracing its proceedings and transactions. Boston, Oct. pp. 544.

Animal Chemistry, by Justus Liebig, M. D. Edited from the author's manuscripts, by Wm. Gregory, M. D.: with additions, notes and corrections, by Dr. Gregory and others; by John W. Webster, M. D., Prof. Chem. in Harvard University, Cambridge.

A Catalogue of the Birds of Connecticut; by Rev. J. H. Linsley, A. M. From the Am. Jour. Science, Vol. XLIV, No. 2. From the Author.

Transactions of the American Philosophical Society for Promoting Useful Knowledge, held at Philadelphia. Vol. VIII, Part Third. 1843. From the Society.

Description of the American Limacidæ; by A. Binney, Esq. Boston. Three copies.

MISCELLANEOUS.—FOREIGN.

List of prices of mathematical and other apparatus, for sale by E. M. Clark, London.

Memoires sur le Canada depuis 1749 jusqu'a 1760, entrois parties avec cartes et le plans lithographies, 1838.

A number of bookseller's catalogues.

Twenty sixth report to the 20th November, 1842, of the London Provident Institution.

Sketch of the writings and philosophical character of Augustin P. De Candolle, Prof. of Nat. Hist. Geneva; by Prof. Daubeny, of Oxford. From the Author.

Austria; by W. R. Wilde, M. R. I. A. Dublin. From the Publishers.

Fourth annual report of the Morrison Education Society. Macao, 1842. From Rev. S. R. Brown.

Journal of a tour through the United States and in Canada, made during the years 1837–38; by Charles Daubeny, M. D., F. R. S., &c. Oxford. pp. 232, 12mo. From the Author.

MISCELLANEOUS.—DOMESTIC.

Tenth Annual Report of the Trustees of the State Lunatic Hospital at Worcester. Dec. 1842. Boston. From Dr. S. G. Howe.

Catalogue of Williston Seminary, fall term, 1842. East Hampton, Mass.

Twenty fourth Annual Report of the Directors of the New York Institution for the Deaf and Dumb. 1843. From H. P. Peet, Secretary.

Inaugural Address of the Hon. A. Gallatin, LL.D., on taking the chair as President of the New York Historical Society. New York, 1843. From the Society.

Reports to the Providence Athenæum, Feb. 1837 and Sept. 1838; by Stephen Trip, 1842. Do. submitted, Sept. 1842.

Seventeenth Annual Report of the Board of Managers of the Prison Discipline Society. Boston, 1842.

Transactions of the Hartford County Agricultural Society for 1842.

Northern Lakes as a residence for the invalids of the South; by Daniel Drake, M. D. Louisville, Ky. 1842. From the Author.

Annual Reports of the Interments in the city and county of New York, for 1842; by J. H. Griscom, M. D. From the Author.

Twenty second Annual Report of the Board of Directors of the Mercantile Library Association, Clinton Hall. New York, 1843.

Catalogue of Kemper College, 1842–3. St. Louis, Missouri.

Catalogue of Middlebury College, 1842-3. Middlebury, Vt.

New England and the West; by R. W. Haskins, Buffalo, N. Y. From the Author.

The Destinies of War and Labor. An essay delivered by A. E. Gwynne, before the Hamilton Chapter of the Alpha Delta Phi Society, 1842. Clinton.

Prof. Hitchcock's Anniversary Address before the Mount Holyoke Female Seminary. Amherst, Mass. 1843. From the Author.

Mr. Colman's Agricultural Address, 1842, at Rochester, N. Y. From the Author.

A Discourse on the rightfulness and expediency of Capital Punishments; by Rev. Wm. T. Dwight, Portland, Me. 1843. From the Author.

Gambier Catalogue for 1842–3. Gambier, Ohio. Two copies.

Circular and Catalogue of Willoughby University, 1842–3. Clinton, Ohio.

The Chicora, Nos. 9, 10, 11, 12. Charleston, S. C. From J. B. Legare, Esq.

President's Message and documents accompanying, to the 3d session of the 27th Congress. From Hon. S. J. Andrews.

Speech of Hon. Willis Hall in committee of the whole on the governor's message.

Report by Mr. Ferris on the electro-magnetic telegraph, Dec. 30, 1842. From Hon. F. Granger.

Report of Mr. Aycrigg on the Coast Survey, Feb. 9th, 1843. From Hon. J. Trumbull. From Hon. S. J. Andrews. From Hon. W. W. Boardman. From Hon. C. Morgan.

Report of the Commissioners of Patents, Feb. 1843.

Report by Hon. Mr. Pendleton, Ohio, on Military Posts, Council Bluffs, to the Pacific Ocean, Jan. 4th, 1843. From Hon. J. Trumbull.

Mr. Underwood's report relative to steamboat explosions. From Hon. W. W. Boardman.

Collection of the documents that appeared in relation to Dr. Sewall's drawings on the human stomach.

New England's Memorial, by Nathaniel Morton. Fifth edition, with notes, by John Davis. Boston, 1826. pp. 476, 8vo. From the Author.

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Remarks.—This method of acknowledgment has been adopted, because it is not always practicable to write letters, where they might be reasonably expected; and still more difficult is it to prepare and insert in this Journal, notices of all the books, pamphlets, &c., which are kindly presented, even in cases, where such notices, critical or commendatory, would be appropriate; for it is often equally impossible to command the time requisite to frame them, or even to read the works; still, judicious remarks, from other hands, would usually find both acceptance and insertion.

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SCIENCE.—FOREIGN.

Account of the Museum of Economic Geology and Mining Records Office established by government in the department of her Majesty's Commissioners of Woods and Forests, under the direction of Sir Henry De La Beche, F. R. S., F. G. S.; by T. Sopwith, F. G. S. London, 1843. From R. Phillips.

On the theory and construction of a Serismour, or instrument for measuring earthquake shocks, and other concussions; by James D. Forbes, Esq., F. R. S., &c. From the Transactions of the Royal Soc. of Edinburgh, Vol. XV, part 1, 1841. From the author.

On a remarkable structure observed by the author in the ice of glaciers; by J. D. Forbes, Esq., with a plate. Read before the Royal Society of Edinburgh, 1841. From the author.

Observations on the Aurora Borealis from Sept. 1834, to Sept. 1839; by Robert Snow, Esq. London, 1842. From the author.

Prof. Forbes' Account of his recent Observations on Glaciers; and A Fourth Letter on the Glacier Theory to Prof. Jameson; by Prof. Forbes. From the author.

Report of the Manchester Geological Society at their Fourth Annual Meeting, Oct. 27th, 1842, Jas. Heywood, Esq. in the Chair. From the Council of the Society.

On the Galvanic Properties of the Principal Elementary Bodies, with a description of a new Chemico-Mechanical Battery; by Alfred Smee, Esq. London, 1840. From the author.

On the Public Institutions for the advancement of Agricultural Science which exist in other countries; by Chas. Daubeny, M. D., F. R. S. London, 1842. From the author.

Tables for the extemporaneous applications of Corrections for Temperature; by S. Elliot Hoskins, M. D. Guernsey, 1842. Two copies from the author.

Lecture on the application of Science to Agriculture; by Charles Daubeny, M. D., F. R. S. London, 1842. From the author.

On the intimate rationale of the Voltaic Force; by Alfred Smee, F. R. S. London, 1842. From the author.

Memoirs and Proceedings of the Chemical Society, Part 2. From Mr. Teschemacher.

De mutationibus quas subit momentum virgæ magneticæ, partim ob temporis, partim ob temperaturæ mutationes; auctore Christophore Hansteen. Christianiæ, 1842.

Experimental Researches in Electricity, 18th series; by Michael Faraday, Esq. D. C. L. From the Philosophical Transactions, Part I, 1843. London. From the author.

On the transparency of the Atmosphere, and the law of extinction of the Solar Rays in passing through it; by Prof. Forbes. From the Philos. Trans., Part II, 1842. London. From the author.

Delphinus Leucopleurus, nova species, descripta ab H. Rasch, conservatore Musei Zoologici universitatis regiæ Fredericianæ. Christianiæ, 1843. Received August 27th, 1843.

Descriptio ornamentorum maximam partem aureorum et nummorum sæculi viii, vi. et ixni in prœdio Hœn, in parochiâ Eger in Diœcesi Norvegiæ Agershusiensi repertorum; auct. Chr. Andr. Holmhoc. Christianiæ, 1835. Received August 27th, 1843.

Semina Horti Botanici Christianiensis, 1842, collecta. From the Royal University. Christiana, Norway.

SCIENCE.—DOMESTIC.

Elements of Chemistry, including the applications of the Science in the Arts, with numerous illustrations; by Thos. Graham, F. R. S. With notes and additions, by Robert Bridges, M. D. Philad. 1843. Published by Lea & Blanchard. From the publishers. Large 8vo, pp. 749.

Reliquæ Baldwinianæ—selections from the Correspondence of the late Wm. Baldwin, M. D., U. S. N.; compiled by Dr. Darlington. Philad. 1843. 12mo. pp. 346. From H. C. Townsend, Esq.

Report on the exploration of the country lying between the Missouri River and the Rocky Mountains, on the line of the Kansas and Great Platte rivers; by Lieut. J. C. Fremont, of the corps of Topographical Engineers. Washington, 1843. pp. 243. From Col. Abert, and do. from Hon. Mr. Huntington, Norwich.

Lithotripsy, or breaking of stone in the bladder; by Alvan Goldsmith, M. D. New York, 1843. From the author.

Foreign Agriculture, No. I. The Economy of Farming, from the German of Prof. Benger, with copious notes from other authors; by E. Goodrich Smith. New York, 1843. From the author.

An historical sketch of the state of American Medicine before the Revolution; by John B. Beck, M. D. Albany, 1842. From the author.

Suggestions of new theories to the scientific; by A. Girard. Mobile, 1843.—With a newspaper containing an article entitled, Suggestions of a new Planetary System. From the author.

Observations of Encke's Comet at the High School Observatory, Philadelphia, March–April, 1842, with the Fraunhofer Equatorial; by S. C. Walker and E. O. Kendall. Read May, 1842. From the authors.

Contributions to the Geology of the Tertiary Formations of Virginia, second series; by Prof. W. B. Rogers and Prof. H. D. Rogers. Read March, 1839.

MISCELLANEOUS.—DOMESTIC.

Address to the Norfolk County Temperance Society at their meeting at Quincy, 29th Sept. 1842; by J. Q. Adams. From J. Harrington.

An Election Sermon, by the Rev. Samuel C. Jackson, Jan. 7th, 1843, before his Excellency the Governor of Massachusetts, John Davis, the Lieutenant Governor, and Common Council.

The Hierophant, or Monthly Expositor of Sacred Symbols and Prophecy; conducted by George Bush. No. III, Aug. 1842.

Report of the joint special committee of the Senate and House of Representatives of Massachusetts, to whom was referred the petition of George Latimer and more than 65,000 citizens of Mass. From Sam'l Greele. 1843.

Mid Lothian coal mining company's circular for 1843. Richmond.

Twenty-fifth annual report of the Asylum for the Insane, 1842. Philadelphia, 1842. From Dr. Pliny Earle.

Memoir of John Treadwell, LL. D., late Governor of Connecticut; by Prof. Olmsted, of Yale College. Pamphlet form, pp. 31. 1843. From the author.

The Dial. Nos. 12 and 13. Boston, April, 1843. From the Editors.

A Discourse at the ordination of the Rev. F. Butler, pastor of the Congregational Church in the East Parish of Windsor, Vt.; by Rev. J. Richards, pastor of the Church at Dartmouth College. 1843. From the Author.

Transactions of the Natural History Society of Hartford, No. I. Address on the birth day of Linnæus, May 24th, 1836; by Dr. S. F. Jarvis. Two copies.

Fifty-sixth annual report of the Regents of the University of New York, made to the Legislature, March 1, 1843. Albany.

Reply of Col. Abert and Mr. Markoe to the Hon. Mr. Tappan of the U. S. Senate. Washington, 1843.

Report of the select committee relative to the renewal of the State Railways with Pennsylvania cast iron rails; Mr. Trego, chairman. April 4th, 1843. Harrisburg.

"Facts for the People." Cincinnati, O., March, 1842.

Catalogue of fruit and forest trees for sale by Parsons & Co., Flushing, L. I. 1843.

Perkins Institution for the Blind. Annual report of the Trustees, 1843. Boston.

Report of the American Temperance Union, 1843. New York.

Report of the Am. Protestant Reformation Soc. N. Y. 1843.

First annual report of the Western Baptist Theological Institute of Covington, Ky. 1843. From E. Robins.

A new Grammar of the English Language. Boston, 1834. From the author.

Annual address before the Board of Trade of the city of Pittsburg, on Jan. 24th, 1842; by A. W. Loomis, Esq. Pittsburg, 1842.

Catalogue of the valuable Library of Mr. Town, for sale. Wiley & Putnam's Catalogues.

Army and Navy Chronicle. Washington, June 1st, 1843.

Collection of the documents which appeared in the public papers in relation to Dr. Sewall's drawings of the human stomach.

Dr. Pusey's Sermon. Published in New York.

Address before the Philological Institute, Dec. 8th, 1842, by T. J. Bigham, Esq. Pittsburg, 1842.

Order of Exercises at exhibition of Phillips Academy, Andover, Mass., August 8th, 1843.

Hunt's Magazine and Commercial Review, No. L, Aug. 1843.

An address delivered before the New Haven Horticultural Society, May 25th, 1843; by Alfred S. Monson, M. D., Pres. of the Soc'y.

A discourse on the duties and qualifications of an historian, delivered at the fourth anniversary of the Georgia Historical Society, Feb. 1843; by the Hon. M. King. Savannah.

Quarterly Journal of the American Education Society, Aug. 1843.

Catalogue of Western Reserve College for 1841–2. Hudson. From Prof. St. John.

Report on School House Architecture, made to the Board of Commissioners of Common Schools; by Henry Barnard, Esq. Hartford, 1842. From the author.

American Book Circular, with notes and statistics. 1843. From Wiley & Putnam, publishers, New York.

The progress and results of Emancipation in the English West Indies; by John Jay, of Bedford, N. Y. 1842.

The Snag Nullifier—description of an invention for preventing accidents by striking against snags. 1843. From the author, L. G. Mickles.

Annual circular of the Medical College of Louisiana, tenth session, 1843–4. New Orleans.

Annual announcement of the Jefferson Medical College of Phil. 1843–4.

Report of the joint special committee on the subject of the effects of lead pipes on well water in the city of Lowell; by Dr. S. L. Dana. 1842. From J. W. Grant, Esq.

Address delivered by Hon. Daniel Webster at the completion of the Bunker Hill Monument, June 17th, 1843.

Catalogus Collegii Hamiltonensis, 1843.

NEWSPAPERS.—FOREIGN.

The Nonconformist, London, July 12, 1843, from J. C. Dunlap, Esq.—A number of copies of the Scotsman and the Witness, Edinburgh, from J. Dunlap, Esq.—Jersey and Guernsey Advocate, April, 1843.

NEWSPAPERS.—DOMESTIC.

Albany Daily Tribune, March 17th, 1842, from Mr. Delavan.—New York Card.—Albany Patriot, from Mr. Delavan, containing some temperance discussions; a series of them, also of the Evening Journal.—Christian Freeman, Hartford, March 24th, 1843.—The Dayspring, Boston, June and July, 1842.—The Planters' Banner, Franklin, La.—The Clarion, Washington, D. C.—New York Daily Tribune.—Washingtonian Weekly News, New York, 1843.—Boston Semi-Weekly Courier, Feb. 1843.—The Literary Age, Feb. 1842, Philad.—Cincinnati Gazette.—American Messenger, Jan. 1843.—The Southern Chronicle, Jan. 11th, 1843.—The Midnight Cry.—The Protestant Vindicator, New York, Dec. 1842.—The Washingtonian Reformer, April, 1843.—Christian Intelligencer, New York.—The New World Monthly Messenger, Feb. 1842.—The Millennial World.—New York Spectator, March, 1843.—Republican, Savannah, Geo.—New York American, containing an article by Dr. Hare on lead pipes, June 9th, 1843.—Vicksburg Sentinel, Feb. 1843, with meteorological tables for that place.—Brooklyn News, June 3d, 1843, with a notice of a curious impression on stone found in that city. Ohio Observer, Feb. 2d, 1843, containing a letter from Mr. E. Andrews to Prof. Barrows, Hudson.—The Constitution, Middletown, Dec. 1842, from Dr. Barratt, containing an account of the hard winter of 1779–80; from the same source, a notice of the various floods in Connecticut river.—Newark Daily Advertiser, May 2, 1842, from Prof. Henry, with a notice of the comet.—Indiana Statesman, Jan. 1843.—Georgia Messenger, Macon, June, 1843, from S. T. Bailey, with a notice of the phenomenon witnessed in Georgia and Florida on the same day with the great earthquake in St. Domingo.—Middletown Constitution, from Dr. Barratt, with a scheme for restoring salmon to Connecticut river.—Albany Argus, with an article on the progress of pledge taking.—Bloomington (Iowa) Herald, with the meteorological journal for 1842.—Fishkill Standard, Feb. 28th, 1843, with an article on gold fish.—Western State Journal, Syracuse, May 3d, 1842.—Norristown Free Press, with an article on comets, April, 1843.—Vermont Temperance Advocate, Feb. 1843.—The Comet, a Tract for the Times.—Owego Gazette, July 7th, 1843, with a notice of the "Owego Female Academy."—United States Gazette, Philad., Aug. 5th, 1843, with an article headed, "Cambridge Astronomers."—New England Puritan, Boston, July 28th, 1843.—The Inquirer, Philad., Aug., 1843, with a notice of the American Journal.—Commercial Advertiser, Buffalo, from R. W. Haskins, with some scientific memoranda translated from the French.—Journal and Advertiser, Auburn, 1843, noticing death of Rev. Jas. Richards.—The Sabbath Vindicator, New York, Aug. 1843.—Pittsburgh Gazette, July 27th, with a notice of this Journal.—The Gazette, Boston, Aug. 26th, 1843.—The Christian World, Boston, July 22d, 1843, with a notice of Washington Allston.—Boston Semi-Weekly Gazette, from R. H. Dana, Jr., Aug. 21st, 1842, with an article headed "Mr. Sturgis' Letter."—New England Farmer, from Dr. C. T. Jackson, with some remarks on "Common Salts as Fertilizers," Boston, May, 1843.—New York Sun, Aug. 29th, 1843.—Albany Evening Gazette, July 7th, 1843.—Albany Atlas, Aug. 21st, 1842, from Mr. Delavan.

SPECIMENS.

Orthis, three miles below Rochester, Genessee River. From Samuel Griswold, Mumfordville, N. Y.

A suit of Pliocene Shells from the Tertiary of Italy, near Piedmont. From M. Avocat Jean Michelotti, à Turin.

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Prof. Forbes' elegant volume on the Glaciers, was received too late for notice in our present number, and several bibliographical notices intended for this number, are crowded out.

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