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CONTENTS OF VOLUME XLVII.

NUMBER I.

NUMBER II.

ERRATA.

Page 105, bottom line, for "baryta 72," read "76."
" 187, line 13 from top, for "Progrèe," read "Progrès."
" 208, line 13 from bottom, for "XVI," read "XLVI."
" 282, line 8 from bottom, read "Robert *C.* Winthrop, Esq."
" 294, line 14, for "Saur*i*oidichnites," read "Sauroidichnites."
" " line 22, for "five," read "four."
" " line 25, for "two," read "one."
" " line 27, for "thirty three," read "thirty four."
" 308, line 5, for "Fig. 10," read "Fig. 10*a*."
" 314, line 13, for "*Slate*," read "*Sandstone*."
" 315, line 15, and page 316, line 7, for "fig. 9," read "Plate IV."
" 317, under *Pachydactyli*, insert a new species, "5. O. tuberosus."
" 318, line 9, for "thirty two," read "thirty four."
" 350, note, for "p. 136," read "p. 36."

DIRECTION TO THE BINDER.

The Portrait of Mr. Maclure should, in binding the volume, be placed so as to face the Memoir, page 1.

Wm Maclure

THE

AMERICAN

JOURNAL OF SCIENCE, &c.

Art. I.—*A Memoir of William Maclure, Esq., late President of the Academy of Natural Sciences of Philadelphia;* by Samuel George Morton, M. D., one of the Vice Presidents of the Institution.* [Read before the Academy, July 1, 1841.]

TO

ALEXANDER MACLURE, Esq., and to Miss ANNA MACLURE,

THIS MEMOIR OF THEIR ILLUSTRIOUS BROTHER

IS MOST RESPECTFULLY INSCRIBED

BY

THE AUTHOR.

The most pleasing province of biography is that which commemorates the sway of the affections. These, however variously expressed, tend to the diffusion of religion, of virtue and of knowledge, and consequently of happiness. He who feeds the hungry, or soothes the sorrowful, or encourages merit, or disseminates truth, justly claims the respect and gratitude of the age in which he lives, and consecrates his name in the bosom of posterity. The benefactions of a liberal mind not only do good of themselves, but incite the same spirit in others; for who can behold the happy results of useful and benevolent enterprise, and not feel the godlike impulse to participate in and extend them?

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* We ought long since to have laid this memoir before our readers, only a small part of whom probably have ever seen it. Mr. Maclure may be considered as the father of American geology, and was a most efficient patron of all other branches of science.—*Eds.*

The study of natural history in this country, thought late in attracting general attention, has expanded with surprising rapidity. Thirty years ago all our naturalists were embraced in a few cultivators of botany and mineralogy, while the other branches were comparatively unheeded and unknown. The vast field of inquiry was devoid of laborers, excepting here and there a solitary individual who pursued the sequestered paths of science, filled with an enthusiasm of which the busy world knew nothing. How widely different is the scene which *now* presents itself to our view! We see the unbounded resources of the land brought forth to the light of day, and made to minister to the wants and the intelligence of humanity. Every region is explored, every locality is anxiously searched for new objects of utility, or new sources of study and instruction.

In connection with these gratifying facts, it will be reasonably inquired, who were they who fostered the early infancy of science in our country? Who stood forth, unmindful of the sneer of ignorance and the frown of prejudice, to unveil the fascinating truths of nature?

Among the most zealous and efficient of these pioneers of discovery was William Maclure.

This gentleman, the son of David and Ann Maclure, was born at Ayr in Scotland, in the year 1763; and he there received the primary part of his education under the charge of Mr. Douglass, an intelligent teacher, who was especially reputed for classical and mathematical attainments. His pupil's strong mind readily acquired the several branches of a liberal education; but he has often remarked, that from childhood he was disposed to reject the learning of the schools for the simpler and more attractive truths of natural history. The active duties of life, however, soon engrossed his time and attention; and at the early age of nineteen years he visited the United States with a view to mercantile employment. He landed in the city of New York; and having made the requisite arrangements, returned without delay to London, where he commenced his career of commercial enterprise as a partner in the house of Miller, Hart & Co. He devoted himself to business with great assiduity, and speedily reaped a corresponding reward. In the year 1796 he again visited America, in order to arrange some unsettled business of the parent establishment: but in 1803 we find him once more in England, not, however, as a merchant, but in the capacity of a public functionary; for Mr. Maclure was at this time appointed a commissioner to settle the claims of American citizens on the government of France, for spoliations committed during the revolution in that country. In this arduous and responsible trust Mr. Maclure was associated with two colleagues, John Fenton Mercer and Cox Barnet, Esqs.; and by the ability and diligence of this commission, the object of their appointment was accomplished to general satisfaction.

During the few years which Mr. Maclure passed on the Continent in attention to these concerns, he took occasion to visit many parts of Europe for the purpose of collecting objects in natural history, and forwarding them to the United States—which from his boyhood had been to him the land of promise, and subsequently his adopted country. With this design he traversed the most interesting portions of the old world, from the Mediterranean Sea to the Baltic, and from the British islands to Bohemia. Geology had become the engrossing study of his mind; and he pursued it with an enthusiasm and success to which time, toil and distance presented but temporary obstacles.

Instructed by these researches, Mr. Maclure was prepared, on his return to the United States, to commence a most important scientific enterprise, and one which he had long contemplated as the great object of his ambition, viz. a geological survey of the United States.

In this extraordinary undertaking we have a forcible example of what individual effort can accomplish, unsustained by government patronage, and unassisted by collateral aids. At a time when scientific pursuits were little known and still less appreciated in this country, he commenced his herculean task. He went forth with his hammer in his hand and his wallet on his shoulder, pursuing his researches in every direction, often amid pathless tracts and dreary solitudes, until he had crossed and recrossed the Alleghany mountains no less than fifty times. He encountered all the privations of hunger, thirst, fatigue and exposure, month after month, year after year, until his indomitable spirit had conquered every difficulty, and crowned his enterprise with success.

Mr. Maclure's observations were made in almost every state and territory in the Union, from the river St. Lawrence to the

Gulf of Mexico; and the memoir which embraced his accumulated facts, was at length submitted to the American Philosophical Society, and printed in their Transactions for the year 1809.*

Novel as this work was, and replete with important details, its author did not suspend his researches with its publication, but resumed them on a yet more extended scale, in order to obtain additional materials, and test the correctness of his previous views. In after life he often recurred with pleasure to the incidents connected with this survey; some of which, though vexatious at the time, were subsequently the theme of amusing anecdote. When travelling in some remote districts, the unlettered inhabitants seeing him engaged in breaking the rocks with his hammer, supposed him to be a lunatic who had escaped from confinement; and on one occasion, as he drew near a public house, the inmates, being informed of his approach, took refuge in-doors, and closing the entrance held a parley from the windows, until they were at length convinced that the stranger could be safely admitted.

Incidents of this kind, and many others which occurred to him, appear to have influenced the following remarks in the Preface to his Geology: "All inquiry into the nature and properties of rocks, or the relative situation they occupy on the surface of the earth, has been much neglected. It is only since a few years that it has been thought worth the attention of either the learned or unlearned; and even now a great proportion of both treat such investigations with contempt, as beneath their notice. Why mankind should have so long neglected to acquire knowledge so useful to the progress of civilization—why the substances over which they have been daily stumbling, and without whose aid they could not exercise any one art or profession, should be the last to occupy their attention—is one of those problems perhaps only to be solved by an analysis of the nature and origin of the power of the few over the many."

Notwithstanding that Mr. Maclure thus felt himself almost alone in his pursuits in this country, he did not relax his ardor in the cause of science, but continued to extend and complete his geological survey; which, after receiving his final revisions, was again presented to the Philosophical Society on the 16th of May,

* This memoir is entitled, "Observations on the Geology of the United States, explanatory of a Geological Map." It was read January 20, 1809, and is published in the sixth volume of the Society's Transactions.

1817, eight years after their reception of the original draft. The amended memoir was now republished, both in the Society's Transactions and in a separate volume, accompanied by a colored map and sections; and while it placed its author among the first of living geologists, excited a thirst for inquiry and comparison which has continued to extend its influence over every section of our country.

It is not proposed, in this place, to analyze this valuable contribution to American science. It may be sufficient to remark, that every one conversant with geology is surprised at the number and accuracy of Mr. Maclure's observations; for the many surveys which have been recently conducted in almost every state in the Union, have only tended to confirm his correctness as to the extent and relative position of the leading geological formations of this country; while the genius and industry which could accomplish so much, must command the lasting respect and admiration of those who can appreciate the triumphs of science. In the evening of his days Mr. Maclure beheld with unmixed pleasure, the progress of geology in his adopted country: he saw state after state directing geological surveys under the supervision of zealous and able naturalists: he rejoiced to observe how their observations harmonized with his own; and it was among his most pleasing reflections, as age and infirmity drew near, that he had once trodden almost solitary and unheeded, that path which is now thronged with votaries of science and aspirants for honor.

In truth, what among temporal considerations is more remarkable and gratifying than the progress which has been made in elucidating the geology of this country during the past thirty years? So extended a field, so many obstacles, and so little patronage, seemed at first view to present insuperable difficulties; and it was feared, and not without reason, that while every part of Europe was explored under the patronage of national governments, the vast natural resources of this country would long remain unsearched and unimproved; not for the want of zeal and talent, but from a deficiency of that encouragement which is necessary to great and persevering exertion. Happily, however, the day of doubt has passed; and our state governments now vie with each other in revealing those buried treasures which minister so largely to the wealth, the comfort and the intelligence of man.

The time which Mr. Maclure allotted to repose from his geological pursuits was chiefly passed in Philadelphia; where he watched the rise of a young but promising institution, devoted exclusively to natural history, and numbering among its members whatever our city then possessed of scientific taste and talent. This institution was the Academy of Natural Sciences of Philadelphia; and as its history, from this period, is inseparably connected with the life of Mr. Maclure, let us briefly inquire into its origin and progress.

The Academy was founded in January, 1812, at which period a few gentlemen, at first but seven in number, resolved to meet once in every week for the purpose of conversing on scientific subjects, and thus communicating to each other the results of their reading, observation and reflection.

Although Mr. Maclure was absent from the city at the initiatory meeting, he had no sooner returned than his name was enrolled on the list of members; and from that hour and with this circumstance the prosperity of the institution commenced. Arrangements were soon after entered into for the delivery of courses of lectures, chiefly on chemistry and botany; and the library and museum were at once replenished with books and specimens from Mr. Maclure's European collections.

On the 30th of December, 1817, Mr. Maclure was elected President of the Academy; to which office of confidence and honor he was annually reëlected up to the time of his death, a period of more than twenty-two years.

Under his auspices the Journal of the Academy (which now numbers eight octavo volumes) was commenced with energy and talent; and such was his interest in its progress, that a considerable portion of the first volume was printed in an apartment of his own house.

Among the most ardent of Mr. Maclure's colleagues at this time was Mr. Thomas Say, a gentleman who united in a remarkable degree the love of science and the social virtues. Enthusiastic in his favorite studies, and possessed of a singular tact for detecting the varied relations of organized beings, he early attracted the notice and secured the esteem of Mr. Maclure; and the friendship which thus grew up between them, continued unaltered by time or circumstance to the end of life. How much the Academy and the cause of natural history owe to the united efforts of these gentlemen, I need not declare; for not only here, but wherever their favorite pursuits are loved and cultivated, their names will be inseparably interwoven with the records and the honors of science.

During the year 1817 Mr. Maclure chiefly occupied himself in the publication of his Geology in a separate volume; after which he devoted himself with assiduity to the interests of the Academy. Previous to the year 1819 he had already presented the institution with the larger part of the fine library he had collected in Europe, embracing nearly fifteen hundred volumes; among which were six hundred quartos and one hundred and forty-six folios on natural history, antiquities, the fine arts, voyages and travels. The value of these acquisitions was greatly enhanced by the fact that they were possessed by no other institution on this side of the Atlantic. The Academy therefore derived from this source a prosperity and permanence which, under other circumstances, must have been extremely slow and uncertain; while science at the same time received an impulse which has never faltered, and which has been subsequently imparted to every section of our country.*

In the winter of 1816–17 Mr. Maclure visited the West Indies, for the purpose of ascertaining by personal observation, the geology of that chain of islands known as the Antilles. With this view he visited and examined nearly twenty of these islands, in the Caribbean Sea, from Barbadoes to Santa Cruz and St. Thomas inclusive. He bestowed especial attention on those portions of the series which are of volcanic origin, of which the Grenadines form the southern and Saba the northern end of the chain. The results of this voyage of observation, in which he was accompanied by his friend Mr. Lesueur, were submitted to the Academy on the 28th of October, 1817, and soon afterwards published in the Society's Journal.†

In 1819 Mr. Maclure's active mind was again directed to Europe. Embarking at New York he went direct to France, and not long afterwards to Spain. He was induced to visit the latter country on account of the liberal constitution promulgated by the Cortes, which promised a comparatively free government to a country long oppressed by every species of bondage. His plan was to establish a great agricultural school, in which physical labor should be combined with moral and intellectual culture. His views were almost exclusively directed to the lower and consequently uneducated classes, whom he hoped to elevate above the

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* Notice of the Academy of Natural Sciences, p. 13.

† Journal of the Academy of Natural Sciences, Vol. I.

thraldom to which they had been subjected by the institutions of their country. He purchased of the government 10,000 acres of land near the city of Alicant; and having repaired the buildings, and placed the estate in complete order, he prepared to commence his scheme of practical benevolence. Scarcely, however, were these arrangements made, when the constitutional government was overthrown, and the old institutions, with all their abuses, were again imposed upon this unfortunate country. The property which Mr. Maclure had purchased from the Cortes had been confiscated from the church; and as the priesthood were now reinvested in their estates, they proceeded to dispossess him without ceremony or reimbursement.

Disappointed and mortified by this adverse termination of his plans, Mr. Maclure abandoned them as hopeless, and prepared to return to the United States. Before doing so, however, he visited various parts of southern Spain, chiefly with a view to scientific investigation. But even in this unoffending employment he found himself surrounded by new dangers, which compelled him to relinquish much that he had proposed to accomplish; and his feelings, and the causes which gave rise to them, are forcibly expressed in a letter to his friend Professor Silliman, dated Alicant, March 6, 1824.

"I have been much disappointed in being prevented from executing my mineralogical excursions in Spain, by the bands of powerful robbers that have long infested the astonishingly extended surface of uncultivated and inhospitable wilds in this naturally delightful country. Not that I require any money worth the robbing to supply me with all that I need—for the regimen which I adopt for the promotion of my health, demands nothing but water and a very small quantity of the most common food—but these barbarians have adopted the Algerine system of taking you, as a slave, to the mountains, where they exact a ransom of as many thousand dollars as they conceive the property you possess will enable you to pay."*

On returning to the United States in 1824, Mr. Maclure was still intent on establishing an agricultural school on a plan similar to that he had attempted in Spain. At this juncture the settlement at New Harmony, in Indiana, had been purchased by the eccentric author of the Social system; and many intelligent per-

* American Journal of Science, Vol. VIII, p. 187.

sons, deceived by a plausible theory, went forth to join the Utopian colony; and Mr. Maclure himself, willing to test the validity of a system which seemed to promise something for human advantage, resolved to establish, in the same locality, his proposed agricultural school. He did not, at the same time, adopt all the peculiar views of this fugitive community, to many of which, in fact, he was decidedly opposed; but he consented to compromise some of his opinions in order to accomplish, in his own phrase, "the greatest good for the greatest number." For this purpose he forwarded to New Harmony his private library, philosophical instruments and collections in natural history, designing, by these and other means, to make that locality the centre of education in the West. That the Social scheme was speedily and entirely abortive, is a fact familiar to every one; but Mr. Maclure having purchased extensive tracts of land in the town and vicinity of New Harmony, continued to reside there for several years, in the hope of bringing his school into practical operation.

In leaving Philadelphia for New Harmony, Mr. Maclure induced several distinguished naturalists to bear him company, as coadjutors in his educational designs; and among them were Mr. Say, Mr. Lesueur, Dr. Troost, and a few others, who had already earned an enviable scientific reputation.

For various reasons, which need not be discussed in this place, the school did not fulfill the expectations of its founder, who was at length constrained to relinquish it; and the less reluctantly, as the approach of age and the increasing delicacy of his constitution admonished him of the necessity of seeking a more genial climate. We accordingly find him, in the autumn of 1827, embarking for Mexico in company with his friend Mr. Say. They passed the winter in that delightful country, and employed their time in observing and recording the various new facts in science which there presented themselves; and on the approach of summer they returned to the United States.

Mr. Maclure was so pleased with the climate of Mexico, and so solicitous to study the social and political institutions of that country, that he determined to return the same year; and with this intent he visited Philadelphia, proceeded thence to New Haven, and presided for the last time at a meeting of the American Geological Society in that city on the 17th of November, 1828. Of this institution he had also long been President, and took an active interest in its prosperity, which was strengthened by his regard for his friend, Professor Silliman—a man justly esteemed for his zealous and successful exertions to advance the interests of science, not less than for his extensive acquirements and his many virtues. On this occasion Mr. Maclure declared his intention to bring back with him from Mexico a number of young native Indians, in order to have them educated in the United States, and subsequently diffuse the benefits of instruction among the people of their own race. This benevolent object, however, was not accomplished; for in the ordering of Providence he did not live to return.

From New Haven Mr. Maclure proceeded to New York, and embarked for Mexico. Time and distance, however, could not estrange him from that solicitude which he had long cherished for the advancement of education in his adopted country; and from his remote residence he kept a constant correspondence with his friends in the United States, among whom was the author of this memoir.

Mr. Say died in 1834 at New Harmony;* and Mr. Maclure was thus deprived of one of his oldest and firmest friends. The loss seemed for a time to render him wavering as to his future plans; but convinced on reflection that his educational projects in the west could be no longer fostered or sustained, he resolved to transfer his library at New Harmony to the Academy of Natural Sciences. This rich donation was announced to the Society in the autumn of 1835; and Dr. Charles Pickering, who had been for several years librarian of the institution, was deputed to superintend the conveyance of the books to Philadelphia; a trust which was speedily and safely accomplished.

This second library contained 2259 volumes, embracing, like the former one, works in every department of useful knowledge, but especially natural history and the fine arts, together with an extensive series of maps and charts.

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* Mr. Say was one of the founders of the Academy; and among the last acts of his life, he provided for the further utility of the institution by requesting that it should become the depository of his books and collections. This verbal request was happily confided to one whose feelings and pursuits were congenial to his own; and the Academy is indebted to Mr. and Mrs. Say for some of its most valuable acquisitions.

An interesting and eloquent memoir of Mr. Say was written by Dr. Benjamin Horner Coates, and published under the auspices of the Academy in 1835.

Mr. Maclure's liberality however was not confined to a single institution: the American Geological Society, established as we have already mentioned at New Haven, partook largely of his benefactions, both in books and specimens; and in reference to these repeated contributions Professor Silliman has expressed the following brief but just and beautiful acknowledgment: "This gentleman's liberality to purposes of science and humanity has been too often and too munificently experienced in this country, to demand any eulogium from us. It is rare that affluence, liberality, and the possession and love of science unite so signally in the same individual."*

Since the year 1826 the Academy had occupied an edifice in some respects well adapted to its objects; but the extent and value of the library, suggested to Mr. Maclure the necessity of a fire-proof building. In order to accomplish this object, he first transferred to the Society a claim on an unsettled estate for the sum of five thousand dollars, which was followed in 1837 by a second donation of the same amount. Meanwhile, having matured the plan of the new Hall of the Academy, and having submitted his views to the members, he transmitted in 1838 an additional subscription for ten thousand dollars.

Thus sustained by the splendid liberality of their venerable President, the Society proceeded without delay in the erection of a new building. The corner-stone was laid, with due form, on the 25th of May, 1839; on which occasion an appropriate address was delivered by Professor Johnson. The edifice thus auspiciously begun, was conducted without delay to completion; and the first meeting of the Society within its walls was held on the 7th day of February, 1840.

Mr. Maclure had fervently desired and fully expected to revisit Philadelphia; but early in the year 1839 his constitution suffered several severe shocks of disease, and from that period age and its varied infirmities grew rapidly upon him. Under these circumstances he became more than ever solicitous to return to the United States, to enjoy again the companionship of his family and friends, and to end his days in that land which had witnessed alike his prosperity and his munificence.

He made repeated efforts to accomplish this last wish of his heart; and finally arranged with his friend Dr. Burrough, then United States Consul at Vera Cruz, to meet him at Jalapa with

* American Journal of Science, Vol. III, p. 362.

a *littera* and bearers, in order to conduct him to the sea-coast. Dr. Burrough faithfully performed his part of the engagement; but after waiting for some days at the appointed place of meeting, he received the melancholy intelligence that Mr. Maclure, after having left Mexico and accomplished a few leagues of his journey, was compelled by illness and consequent exhaustion to relinquish his journey.

Languid in body, and depressed and disappointed in mind, Mr. Maclure reluctantly retraced his steps; but being unable to reach the capital, he was cordially received into the country house of his friend, Valentin Gomez Farias, ex-President of Mexico, where he received all the attentions which hospitality could dictate. His feeble frame was capable of but one subsequent effort, which enabled him to reach the village of San Angel; where, growing weaker and weaker, and sensible of the approach of death, he yielded to the common lot of humanity on the 23d day of March, 1840, in the seventy seventh year of his age.

Thus closed a life which had been devoted, with untiring energy and singular disinterestedness, to the attainment and diffusion of practical knowledge. No views of pecuniary advantage, or personal aggrandizement, entered into the motives by which he was governed. His educational plans, it is true, were repeatedly inoperative, not because he did too little, but because he expected more than could be realized in the social institutions by which he was surrounded. He aimed at reforming mankind by diverting their attention from the mere pursuit of wealth and ambition to the cultivation of the mind; and espousing the hypothesis of the possible "equality of education, property and power" among men, he labored to counteract that love of superiority which appeared to him to cause half the miseries of our species. However fascinating these views are in theory, mankind are not yet prepared to reduce them to practice; and without entering into discussion in this place we may venture to assert, that what religion itself has not been able to accomplish, philosophy will attempt in vain.

Mr. Maclure's character habitually expressed itself without dissimulation or disguise. Educated in the old world almost to the period of manhood, and inflexibly averse to many of its established institutions, he was prone to indulge the opposite extremes of opinion, and became impatient of those usages which appeared to him to fetter the reason and embarrass the genius of man; and while he rejoiced in the republican system of his adopted country, he aimed at an intellectual exaltation which, to common observation at least, seems incompatible with the wants and impulses of our nature.

Fully and justly imbued with the importance of disseminating practical truth, he strove through its influence to bring the several classes of mankind more on a level with each other, not by invading the privileges of the rich, but by educating the poor; thus enforcing the sentiment that "knowledge is power," and that he who possesses it will seldom be the dupe of designing and arbitrary minds. With a similar motive he endeavored to inculcate the elements of political economy, by the publication of epistolary essays in a familiar style, which have been embodied in two volumes with the title of *Opinions on Various Subjects.* They discover a bold and original mind, and a fondness for innovation which occasionally expresses itself in a startling sentiment; but however we may differ from him on various questions, it must be conceded that his views of financial operations were remarkably correct, inasmuch as he predicted the existing pecuniary embarrassments of this country at the very time when the great mass of observers looked forward to accumulating wealth and unexampled prosperity.

Let it not be supposed that Mr. Maclure's benevolent efforts were restricted to those extended schemes of usefulness to which we have so often adverted. Far, very far from it. His individual and more private benefactions, were such as became his affluent resources, influenced by a generous spirit. He habitually extended his patronage to genius, and his cordial support to those plans which, in his view, were adapted to the common interests of humanity. There are few cabinets of natural history in our country, public or private, that have not been augmented from his stores; and several scientific publications of an expensive character, have been sustained to completion by his instrumentality. While in Europe he purchased the copperplate illustrations of some important works both in science and art, with the intention of having them republished at home in a cheaper form, in order to render them accessible to all classes of learners. Among these works was Michaux's Sylva, which has been published, since his death, in conformity to his wishes.

Mr. Maclure was singularly mild and unostentatious in his manner; and though a man of strong feelings, he seldom allowed his temper to triumph over his judgment. Cautious in his intimacies, and firm in his friendships, time and circumstance in no degree weakened the affections of his earlier years. Though affable and communicative, Mr. Maclure was much isolated during the last thirty years of his life; partly owing to a naturally retiring disposition, partly to the peculiarity of some of his opinions, in respect to which, though unobtrusive, he was inflexible—but mainly to that frequent change of residence which is unfavorable to social fellowship. Hence it is that of the thousands who are familiar with his name in the annals of science, comparatively few can speak of him from personal knowledge.

In person he was above the middle stature, and of a naturally robust frame. His constitution was elastic, and capable of much endurance of privation and fatigue, which he attributed chiefly to the undeviating simplicity of his diet. His head was large, his forehead high and expanded, his nose aquiline; and his collective features were expressive of that undisturbed serenity of mind which was a conspicuous trait of his character.

Those who knew him in early life, represent him to have been remarkable for personal endowments; a fact which is evident in the full-length portrait now in possession of his family, painted by the celebrated Northcote.*

Such was WILLIAM MACLURE, whose long, active and useful life is the subject of this brief and inadequate memorial. His remains are entombed in a distant land, and even there the spirit of affection has raised a tablet to his memory. But his greater and more enduring monument, is the edifice within whose walls we are now met to recount and perpetuate his virtues. Wherever we turn our eyes we behold the proofs of his talent, his zeal, his munificence. We see an institution which, under his fostering care, has already attained the manhood of science, and is destined to connect his name with those beautiful truths which formed the engrossing subject of his thoughts. We see around us the collections that were made with his own hands, vastly augmented, it is true, by the zeal of those who have been stimulated by his example. Here are the books which he read—to him the fountains of pleasure and instruction. Here has he concentrated the works of nature, the sources of knowledge, the incentives to study; and, actuated

* See the head affixed to this memoir, which is taken from a copy of Northcote's picture.

by his liberal spirit, we open our doors to all inquiring minds, and invite them to participate, with us, in these invaluable acquisitions; and while we regard them as a trust to be transmitted to posterity, let us honor the name and cherish the memory of the man from whom we derived them.

The death of Mr. Maclure was announced to the Academy of Natural Sciences on Tuesday evening, the 28th of April, on which occasion the following resolutions were unanimously adopted:

Resolved, That the Academy has learned with deep concern the decease, at San Angel, near the city of Mexico, of their venerable and respected President and benefactor, William Maclure, Esq.

Resolved, That although his declining health induced him to reside for some years in a distant and more genial clime, this Academy cherishes for Mr. Maclure the kindest personal recollections, and a grateful sense of his contributions to the cause of science.

Resolved, That as the pioneer of American geology, the whole country owes to Mr. Maclure a debt of gratitude, and in his death will acknowledge the loss of one of the most efficient friends of science and the arts.

Resolved, That as the patron of men of science, even more than for his personal researches, Mr. Maclure deserves the lasting regard of mankind.

Resolved, That a member of the Academy be appointed to prepare and deliver a discourse commemorative of its lamented President.

Resolved, That the Corresponding Secretary be requested to communicate to the family of Mr. Maclure a copy of these resolutions.

Mr. Maclure died before he had accomplished *all* his views in respect to this institution; for, looking forward, as he did, to renewed personal intercourse with its members, he intended to inquire for himself into the most available modes of extending its usefulness. This, as we have seen, was denied him; but the spirit of science which was inherent in him, has descended upon his brother and sister; and to these estimable and enlightened individuals, we owe the consummation of all that their brother had proposed in reference to the Academy, which will be hereafter enabled to devote its resources exclusively to the advancement of those objects for which it was founded.

List of the published works and memoirs of Mr. Maclure.

The following list embraces the separate works (two in number) and miscellaneous papers written by Mr. Maclure during his residence in the United States. It is not presumed that the list is complete; for it is more than probable that he contributed something to the periodical journals of England, France, Spain, and perhaps of Mexico, while resident in those countries.

A reference to the following essays will show how exclusively Mr. Maclure's mind was devoted to matters of fact, seldom indulging in hypothesis, and never yielding himself, at least in his writings, to purely imaginative reflections.

1. Observations on the Geology of the United States of America, with some Remarks on the Nature and Fertility of Soils, &c. Philadelphia, 1817. 8vo. This is a corrected reprint from the Transactions of the American Philosophical Society.
2. Opinions on Various Subjects, 2 vols. 8vo. This work is epistolary, and was chiefly written in Mexico. It embraces reflections on many subjects, but is mainly devoted to Political Economy.

Memoirs in the Journal of the Academy of Natural Sciences of Philadelphia:

1. Observations on the [Geology of the] West India Islands, from Barbadoes to Santa Cruz, inclusive. Vol. I, p. 134.
2. Essay on the Formation of Rocks; or an Inquiry into their probable Origin, and their present Form and Structure. Vol. I, p. 261.

Memoirs in the American Philosophical Transactions:

1. Observations on the Geology of the United States, explanatory of a Geological Map. Vol. VI, p. 91. 1809.
2. The same Memoir, corrected and extended. Vol. I, New Series, 1817.

Memoirs in the American Journal of Science and Arts, conducted by Professor Silliman:

1. Hints on some of the Outlines of Geological Arrangement. Vol. I, p. 209.
2. Conjectures on the probable changes that have taken place in the Geology of the Continent of America, east of the Stony Mountains. Vol. VI, p. 98.
3. Miscellaneous Remarks on the Systematic Arrangement of Rocks, and on their probable Origin. Vol. VII, p. 261.
4. Notice of the Anthracite Region of Pennsylvania. Vol. X, p. 205.

5. Remarks on the Igneous Theory of the Earth. Vol. XVI, p. 351.
6. Geological Remarks relating to Mexico. Vol. XX, p. 406.

The same periodical also contains many detached observations, and fragments of letters communicated to the editor of that work.

Memoirs published in the Journal de Physique, de Chimie et d'Histoire Naturelle, Paris:

1. Extrait d'une Lettre de M. William Maclure, à J. C. Delamètherie, sur la Geologie des Etats Unis. Tome 69, p. 201. (1809.)
2. Observations sur la Geologie des Etats Unis, servant à expliquer une Carte Geologique. Tome 69, p. 204. (1809.)

This last memoir is a translation from the original in the Transactions of the American Philosophical Society.

Note.—The Hall of the Academy of Natural Sciences of Philadelphia is situated at the corner of Broad and George streets—forty five feet front on the former, and eighty five feet in depth on the latter. The building is fire-proof, and presents a single saloon with three ranges of galleries, beneath which, in the basement, is a lecture-room capable of accommodating four hundred persons.

The institution was founded in 1812, and incorporated in 1817, and enjoys a perpetual exemption from taxation by legislative enactment.

The Museum embraces extensive collections in every department of of Natural History, arranged according to the most approved systems, viz.

2,500 Minerals.
3,000 Fossil Organic Remains.
10,000 Species of Insects.
2,400 Species of Shells.
1,000 Species of Fishes and Reptiles.
1,300 Species of Birds; a small but valuable collection of Quadrupeds, and an extensive series of Comparative Anatomy.

The Herbarium contains about 35,000 species of plants, arranged according to the natural system.

The Library embraces 7,000 volumes, and is always accessible to members, and to visitors attended by members, excepting only those occasions when the Academy is open to the public, viz. on the afternoons of Tuesday and Saturday. Admission free of charge.

Art. II.—*Researches in Elucidation of the Distribution of Heat over the Globe, and especially of the Climatic Features peculiar to the Region of the United States;* by Samuel Forry, M. D., Author of "The Climate of the United States and its Endemic Influences," Editor of "The New York Journal of Medicine and the Collateral Sciences," etc.

Climatology, notwithstanding of the highest interest to man in every conceivable relation of his present existence, has, in the wonderful advancement of human knowledge during the current half century, especially as regards the natural sciences, by no means kept pace with the progress of its kindred branches. With us, the barren work of M. Volney on the climate of the United States, written forty-five years ago, when this French *savant* made a flying visit through our country, is still quoted by every writer on this topic. To Baron Humboldt is due the distinguished credit of having first generalized the various meteorological data, which had been accumulated in different parts of the globe; but so little do philosophers seem to have profited by these deductions, that even Charles Lyell, Esq., in his "Principles of Geology," when speaking of the mild climate of Europe, says—"but this region, constituting only one seventh of the whole globe, proved eventually *to be the exception to the general rule.*" Now it will be a leading object of this paper, contrary to the opinion here advanced on such high authority, to demonstrate the harmony of the laws of climate throughout the globe.*

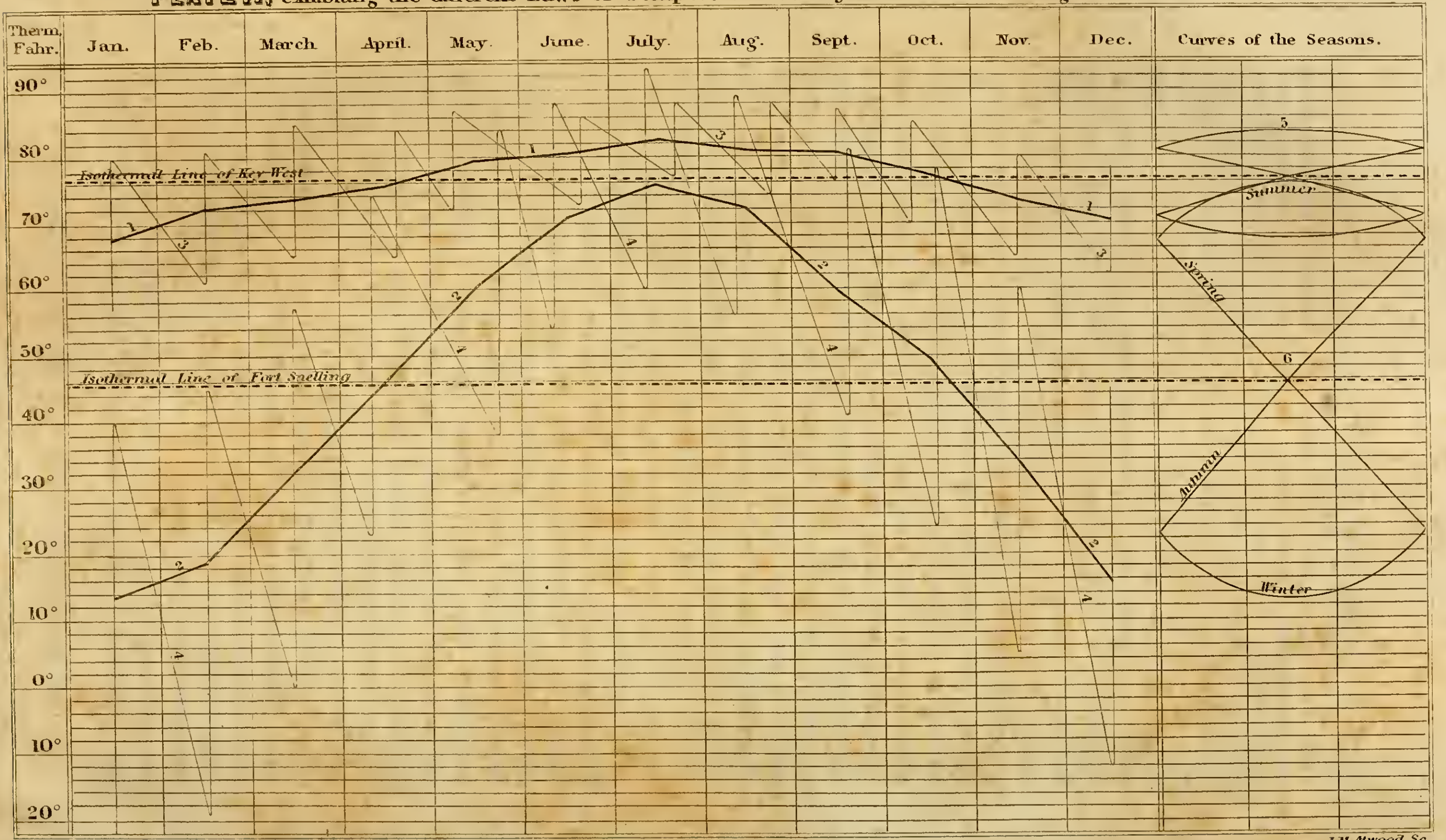
The merit of being first to establish, on an extensive scale, a system of meteorological observations, with a view to the elucidation of the climate of the United States, is due to the Medical Bureau of the Army; and these registers date back regularly to the year 1819, when Dr. Joseph Lovell was the Surgeon General. The only instruments used at first were a thermometer and vane; and to these, the observations were long confined, with general notices of the weather. At the present time, however, observations are taken on a more extended scale, comprising the barometer, the thermometer attached and the thermometer detached,

* In presenting the present analysis of our various writings on the laws of temperature of the United States, we have not hesitated, as regards the dry details of mere facts, to adopt the language used on previous occasions.

PLATE I, illustrating the general Laws of Temperature throughout the United States.

J. M. Atwood Sc.

PLATE II, exhibiting the different Laws of Temperature at Key West & Fort Snelling.



J. M. Atwood Sc.

the pluviometer, Daniell's hygrometer, the wet bulb, and observations upon the clouds, the clearness of the sky, and the force and direction of the winds.*

These data were allowed to accumulate in the Medical Bureau for twenty years, before any comprehensive attempt was made to determine their relations to one another, and to deduce from them general laws; and it fell to our lot, under the direction of the present Surgeon General, Thomas Lawson, Esq., to present a systematic arrangement of these isolated facts, embracing the climatology of a vast district, extending from the oldest settlements on the Atlantic shores to the farthest outposts of civilized occupation, even to the coasts of the Pacific. Thus were presented, under the sanction of the War Department, unlike all other treatises on the same subject, which are generally loosely written and made up of the most vague and general statements, deductions based upon precise instrumental observations.

As regards the phenomena of superficial terrestrial temperature, let it here suffice to refer to its dependence upon two classes of causes, viz. those resulting from celestial relation, and those produced by geographical position. The former, which may be called the *primary* constituents of climate, result from the globular figure of the earth, its diurnal motion upon its axis, and the obliquity of its motion in an elliptical orbit in regard to the plane of the equator. Now, if this class of causes solely controlled the phenomena of terrestrial temperature, climates might be classified with mathematical precision; but the effects produced by solar heat are so much modified by local causes, that the climatic features of any region can be determined only by observation. It is these last, the *secondary* constituents of climate, that we have chiefly to do with, in the present inquiry; and among these geographical or local causes, the following may be regarded as the principal:—1. The action of the sun upon the surface of the earth. 2. The vicinity of great seas and their relative position. 3. The elevation of the place above the level of the sea. 4. The

* The Medical Bureau procured a number of Daniell's hygrometers from London—an instrument characterized by beauty, simplicity, and portableness; but, however well it may be adapted to the humid climate of England, experience soon demonstrated its inapplicability to the arid atmosphere of the United States, with the exception perhaps of our southern borders; and hence has been imposed upon the Department, the necessity of using the wet bulb thermometer in determining the hygrometric condition of the air.

prevalent winds. 5. The form of lands, their mass, their prolongation toward the poles, their temperature and reflection in summer, and the quantity of snow which covers them in winter. 6. The position of mountains relatively to the cardinal points, whether favoring the play of descending currents or affording shelter against particular winds. 7. The color, chemical nature, and radiating power of soil, and the evaporation from its surface. 8. The degree of cultivation and the density of population. 9. Fields of ice which form, as it were, circumpolar continents, or drift into low latitudes. It is these causes that determine the deviations of the *isothermal*, *isocheimal*, and *isotheral* lines from the same parallels of latitude.

In the investigation of the laws of climate, a range of subjects so multifarious as to comprise almost every branch of natural philosophy, is embraced; but its true province is properly restricted to a general view of these subjects, which if based on legitimate deductions of observed phenomena, should enable us to reduce the infinite variety of appearances presented to us in nature, to a few general principles. It is by means of this generalization that the subject will be elevated to the diguity of a science. Climate comprises not only the temperature of the atmosphere, but all those modifications of it which produce a sensible effect on the physical and moral state of man, as well as on all other organic structures, such as its serenity, humidity, changes of electric tension, variations of barometric pressure, its tranquillity as respects both horizontal and vertical currents, and the admixture of terrestrial emanations dissolved in its moisture. Climate, in a word, constitutes the aggregate of all the external physical circumstances appertaining to each locality in its relation to organic nature.

In the present inquiry, however, our labors will be restricted almost wholly to the mere physical laws of climate. As the climate of every region has an inseparable relation with its physical characters, it follows that in the investigation of its climatic features, a geographical description becomes an essential preliminary; but, in the present instance, the country to be described is of so vast an extent as to preclude any thing beyond the most general outlines. It was well remarked by Malte-Brun, that "the best observations upon climate often lose half their value, from the want of an exact description of the surface of the country." Presuming that our readers are sufficiently well acquainted, for the present purpose, with the physical features of the vast region stretching from the Atlantic to the Pacific, and from the Gulf of Mexico to the inland seas on our northern frontiers, the descriptions will be limited to such parts alone as are essential.

One of the most striking characteristics of the physical geography of the United States, and which, it will be seen, induces the most remarkable modifications of climate, is the existence of those great inland basins of water which lie on our northern frontier. Of so vast an extent are these ocean-lakes, that one of them (Lake Superior) has a circuit, following the sinuosities of the coast, of 1,750 miles. The basin of the St. Lawrence is truly a region of "broad rivers and streams," containing, it is estimated, an area of 400,000 square miles, of which 94,000 are covered with water. From the western extremity of Lake Superior to the Gulf of St. Lawrence, the distance is about 1,900 miles. These ocean-lakes have been estimated to contain 11,300 cubic miles of water,—a quantity supposed to exceed more than half of all the fresh water on the face of the globe. The deepest chasms on the surface of either continent are presented perhaps by the depression of these lakes; for though elevated near 600 feet above, the bottom of some is as far beneath, the level of the ocean. Lakes Huron and Michigan, which have the deepest chasms, have been sounded to the amazing depth of 1,800 feet without discovering bottom.

The following table, which gives the mean length, breadth, depth, area, and elevation of these several collections of water, is taken from a recent report made by Douglas Houghton, Esq., state geologist of Michigan.

Collections of water.	Mean length in miles.	Mean breadth in miles.	Mean depth in feet.	Elevation above level of the sea, in feet.	Area in square miles.
Lake Superior,	400	80	900	596	32,000
Green Bay,	100	20	500	578	2,000
Lake Michigan,	320	70	1000	578	22,400
Lake Huron,	240	80	1000	578	20,400
Lake St. Clair,	20	18	20	570	360
Lake Erie,	240	40	84	565	9,600
Lake Ontario,	180	35	500	232	6,300
River St. Lawrence,			20		940
				Aggregate,	94,000

With these preliminary remarks, we are prepared to enter into a detail of the numerical results furnished in the several systems of climate pertaining to the United States. The military posts furnishing the thermometrical data will consequently be classified as under—

General divisions of the United States.	Systems of climate.
1. Northern.	1st Class.—Posts on the coast of New England, extending as far south as the harbor of New York.
	2d " Posts on the northern chain of lakes.
	3d " Posts remote from the ocean and inland seas.
2. Middle.	1st Class.—Atlantic coast from Delaware Bay to Savannah.
	2d " Interior stations.
3. Southern.	1st Class.—Posts on the Lower Mississippi.
	2d " Posts in the Peninsula of East Florida.

These general divisions, intended as well to facilitate description as to express the operation of general laws, may be regarded, in a great measure, as arbitrary. The *northern* embraces a region characterized by the predominance of a low temperature; in the *southern*, a high temperature prevails; while the *middle* exhibits phenomena vibrating to both extremes. Each of these general divisions, as exhibited in the table above, is subdivided into well marked classes or systems.

As the present paper will not allow the admission of extensive tables of figures, the writer is obliged to confine himself to mere results, referring the reader who may be more curious on this subject to the author's work, "The Climate of the United States and its Endemic Influences," which contains a series of extensive tabular abstracts of instrumental observations. These results are obtained from observations made at the various military posts between 24° 33′ and 46° 39′ of north latitude, embracing a space of 22° 6′, and an extent of longitude stretching from the Atlantic to the Pacific. The thermometrical observations were made thrice daily; and as the mean of each month is calculated from 90, and of each year from 1,095 observations, the numerical ratios, it is believed, will give an approximation to the truth as near as can be realized by ordinary observation, and a mean sufficiently correct for every contemplated purpose. The results, at the majority of the posts, are based on from five to ten thousand observations.

1. *The Northern division.*—As this region presents the greatest diversity of physical character, so it exhibits the most marked variety of climate. East of the chain of great lakes, there are several mountain ranges, which, with the exception of a few summits, seldom attain a height of more than 2,500 feet above the level of the sea; and of this elevation, perhaps one half is formed by the table-lands upon which the ridges rest. Above the falls of Niagara, the region of the lakes is elevated 600 to 700 feet above the ocean, but there are scarcely any ridges that deserve the name of mountains. This immense tract is, with the exception of the eastern states, nearly altogether in a state of nature, being still covered with its dense primeval forests. But the most striking characteristic in the physical geography of this division, is that produced by its vast lakes or inland seas. We here behold a chain of lakes presenting a superficial area of 94,000 square miles, with a mean depth of 1,000 feet in the principal lakes, the details of which have just been given.

In accordance with the diversity in the physical geography, we find that on the sea-coast of New England, the influence of the ocean modifies the range of the thermometer, thus equalizing the temperature of the seasons. Advancing into the interior, the extreme range of the temperature increases, and the seasons are violently contrasted. Having come within the influence of the great lakes, a climate like that of the sea-board is found; and proceeding into the region beyond the modifying agency of these inland seas, an excessive climate is again exhibited. And if we continue our route as far as the Pacific Ocean, a climate even more mild and equable than similar parallels in Western Europe, as will be satisfactorily demonstrated, will be presented. The variations of the *isotheral* and *isocheimal* curves—the lines of equal summer and of equal winter temperature, as illustrated in Plate I,—thus afford a happy illustration of the equalizing tendency of large bodies of water. Hence the former division of the surface of the earth into five zones, as regards its temperature, has been superseded in scientific inquiries, by a more precise arrangement. Places having the same mean annual temperature are connected by isothermal lines, and the spaces between them are called isothermal zones.

It is thus seen that, notwithstanding the mean annual temperature presents little variation on the same parallels, four striking inflections of the isotheral and isocheimal lines are exhibited in rapid succession, constituting two systems of climate, viz. that of the Atlantic Ocean and the great lakes, which pertains, comparatively speaking, to the class of *mild* or *uniform*, and that of the intervening tract and the region beyond the lakes, characterized as climates emphatically *excessive* or *rigorous*. The difference of climate, as the mean annual temperature is nearly the same, is, therefore, owing to the unequal distribution of heat among the seasons, as is well illustrated in the accompanying map. At the posts on large bodies of water, the mean temperature of winter is higher and that of summer lower than in the opposite localities; but these results are more satisfactorily evidenced by comparing the difference between the mean temperature of winter and summer, and the warmest and coldest month in each system of climate. Thus Fort Brady, at the outlet of Lake Superior, shows a difference of only 42°·11 between the mean temperature of winter and summer, while Hancock Barracks, half a degree farther south, in the State of Maine, distant only one hundred and fifty miles from the sea-coast, exhibits a disparity of 46°·19; and comparing the warmest and coldest month, the difference of the former is 47°·22, and that of the latter 54°·70. Again, Forts Sullivan and Snelling, in opposite systems of climate, are very nearly in the same latitude, the former at Eastport, on the coast of Maine, and the latter at the junction of the St. Peter's and Mississippi, Iowa. At Fort Sullivan, the difference of winter and summer is 39°·15, and that of the warmest and coldest month, 43°·87; while at Fort Snelling, these ratios are respectively 56°·60 and 61°·86. Fort Howard is also in the same latitude, but as it is situated at the extreme point of one of the smaller lakes, (Green Bay, Wiskonsan,) the temperature is partially modified, these averages being 50°·05 and 54°·11. Next come four posts, all of which are nearly on the same parallel, three being of the class of *uniform* climates, and one of that of *excessive*. Of the former, two, Forts Preble and Constitution, are on the ocean, and the other, Fort Niagara, is on Lake Ontario. At these posts, in the order just named, the difference between the mean temperature of winter and summer is respectively 41°·03, 36°·33, and 41°·73; while, on the other hand, at the *excessive* post, Fort Crawford, Wiskonsan—a point a few minutes farther south than the three former—the difference is 50°·89. The results at Salem, Massachusetts, based on thirty three years' observation by Dr. Holyoke, though not directly under the influence of the ocean, confirms the same law, the difference between the mean temperature of winter and summer being only 41°·66. At all these points, the contrast in the difference of the mean temperature of the warmest and the coldest month, is equally striking. The next points of comparison, as lying on the same parallel, are Forts Wolcott and Trumbull on the Atlantic, and Council Bluffs, Fort Armstrong, and West Point, in the opposite localities. The difference between the mean temperature of summer and winter at Fort Wolcott, Newport, Rhode Island, is 36°·55, and at Fort Trumbull, New London, Connecticut, it is 32°·56; while at Council Bluffs, near the junction of the Platte and Missouri, it is 51°·35—at Fort Armstrong, Illinois, 49°·05—and at West Point, N. Y., 40°·75. Between the two posts on the ocean and the two far in the interior, the difference between the mean temperature of summer and winter presents a disparity of from 15° to 17°; and as respects Fort Trumbull and West Point, which are precisely on the same latitude, the difference between these two opposite seasons, notwithstanding the latter is not more than fifty miles from the ocean, is 8°·19 less at the former post. As regards the difference between the mean temperature of the warmest and coldest months, these laws find confirmation in every instance. So remarkable is the influence of large bodies of water in modifying the range of the thermometer, that although Fort Brady, at the Sault St. Marie, Michigan, is nearly 7° north of Fort Mifflin, near Philadelphia, and notwithstanding the mean annual temperature is more than 14° less, yet the contrast, in the seasons of winter and summer, is not so great at the former as at the latter. Fort Columbus, in the harbor of New York, offers, in some respects, an exception to the laws just developed, the range of the thermometer being greater than at some points farther north. As these results, which are based on nine years' observations, made on an island free from any agency which large towns may exercise, are doubtless correct, some causes of a local nature must exist to produce this effect. It is more than probable that this locality, in consequence of the configuration of the coast, does not lie in the direction of the most prevalent ocean-winds, and that hence its temperature is but partially modified.

The climate of Fort Snelling, which is the most excessive among all the military posts in the United States, resembles that of Moscow in Russia, as regards the extremes of the seasons, notwithstanding the latter is 11° farther north; but at Moscow the mean temperature both of winter and summer is lower—that of winter being as 10°·78 to 15°·95, and that of summer as 67°·10 to 72°·75. At Edinburgh, Scotland, in the same latitude as Moscow, the difference between the mean temperature of winter and summer, is, on the other hand, not one-third as great, being only 17°·90; and even at North Cape, on the island of Maggeroe, in latitude 71°, which is the most northern point of Europe, this difference between the two seasons, so great is the modifying influence of the ocean, is no more than 19°·62, while at Uleo, in the interior of Lapland, the difference between the mean temperature of summer and winter is 45°·90.

In these comparisons of the Northern division, no particular reference has yet been made to Fort Vancouver, in Oregon Territory. This region bears the same climatic relation to our coast and to that of Eastern Asia, as the western coast of Europe does. The mean annual temperature is about 10° higher than that of the posts on the same parallel on our own coast. So mild and uniform are the seasons at Fort Vancouver, that the difference between the mean temperature of winter and summer is only 23°·67, a mean which is less than that of Italy or southern France, and only about two-fifths of that of Fort Snelling, Iowa, notwithstanding the latter is nearly 1° farther south. This contrast is well exhibited in Plate I; for while the mean temperature of spring, summer and autumn, at Fort Vancouver, is about the same as at Fort Wolcott, Rhode Island, the winter line comes nearly as far south as Fort Gibson, Arkansas. But even this comparison, at first view, falls short of the reality; for, as regards the difference between the mean temperature of winter and summer, the contrast is less at Fort Vancouver than at Cantonment Clinch near Pensacola, or Petite Coquille near New Orleans. These results, however extraordinary they may appear, find, as will be seen, an explanation in physical causes.

The next point demanding attention is the difference between the mean temperature of winter and spring, which is much the greater in the *excessive* or rigorous climates. Taking places in the same latitude and in opposite systems of climate, it is found at Fort Brady to be 18°·42, while at Hancock Barracks it is 24°·49; at Fort Sullivan it is 17°·16, while at Forts Snelling and Howard, it is respectively 30°·83 and 24°·10, the latter being partially modified by Green Bay; at Forts Preble, Niagara, and Constitution, and the city of Salem, the ratios are 18°·42, 16°·77, 16°·83 and 17°·89, and at Fort Crawford, on the other hand, it is 25°·83; and lastly at Forts Wolcott and Trumbull, it is 14°·71 and 11°·67, while at Council Bluffs, Fort Armstrong, and West Point, it is respectively 27°·47, 23°·99 and 18°·82. Fort Columbus, as in the preceding comparisons, stands as an exception, its ratio, notwithstanding it is lower than any one in the opposite class, being the highest in its own, with the exception of two posts. The peculiarity in the increase of the temperature of spring, as manifested in the vegetable kingdom, constitutes a feature which strongly characterizes excessive climates; for, as Baron Humboldt remarks, "a summer of uniform heat excites less the force of vegetation, than a great heat preceded by a cold season." Accordingly we find that in these excessive climates, (unlike the uniform ones on the ocean and lakes, in which the air is moist and the changes of the seasons slow and uncertain,) summer succeeds winter so rapidly that there is scarcely any spring, and vernal vegetation is developed with remarkable suddenness. At Fort Vancouver, the difference between the mean temperature of winter and spring is only 6°·67, which is about one third of the difference observed at the posts in our *modified* climates on the same parallel, and little more than one fifth of the difference exhibited in the *excessive* climate of Fort Snelling.

Another feature which characterizes these two systems of climate remains to be considered, viz. the mean annual range of the thermometer. Comparing the posts on the same parallel, the following relations are found:—At Fort Brady, on the one hand, the range is 110°, and at Hancock Barracks, on the other, it is 118°; at Fort Sullivan it is 104°, while at Forts Snelling and Howard, it is 119° and 123°; at Forts Preble, Niagara and Constitution, it is respectively 99°, 92° and 97°, while at Fort Crawford, on the same parallel, it is 120°; and lastly at Forts Wolcott and Trumbull, it is 83° and 78°, while at Council Bluffs, Fort Armstrong and West Point, it is 120°, 106°, and 91°. Fort Columbus, as before, presents an exception. In further elucidation of the law regulating the extremes of temperature, the four following posts, which are all nearly on the same parallel of 41° 30′, the first two being on the ocean, and the last two far in the interior, remote from large bodies of water, may be adduced as striking examples:

	Highest.	Lowest.	Mean annual range.
Fort Wolcott, Newport, R. I.,	85	2	83
" Trumbull, New London, Ct.,	87	9	78
Council Bluffs, near the confluence of Platte and Missouri,	104	—16	120
Fort Armstrong, Rock Island, Ill.,	96	—10	106

These results, it may be necessary to add, exhibit the *average* range of a series of years. The *extreme* range, for example, at Fort Brady, during a period of eleven years, (from 1820 to 1830 inclusive,) is 130°, the mercury sinking in 1826 as low as —37°, and rising in 1830 to 93° Fahr. At Fort Snelling in 1821, the mercury sunk to —32°, and in 1827 rose to 96°, being a range of 128°. At Fort Howard, in 1823, it rose to 100° and sunk to —38°, being a range in the same year of 138°. At Fort Crawford we find the mercury in 1820 noted as high as 99°, and in 1821 as low as —36°, being a range of 135°; at Fort Armstrong, in 1821, as low as —28°, and in 1830 as high as 98°, being a range of 126°; and lastly at Council Bluffs as low, in 1820, as —22°, and in 1822 as high as 108°, being an extreme range of 130°. At the last named post, the thermometer rose every year above 100°. When the Southern division of the United States comes under investigation, it will be seen that the mercury there seldom rises as high as in our northern regions.

Hence it follows that latitude alone constitutes a very uncertain index of the character of climate; for although two places may have the same mean annual temperature, and thus be on the same isothermal line, yet the distribution of heat among the seasons may be extraordinarily unequal. So much, indeed, may the phenomena of superficial terrestrial temperature, as depending on the position of the sun, be modified by local causes, that a classification of climates, or a system of medical geography, having for its basis mere latitude, is wholly inadmissible.

It is thus seen that the climatic features of the coast of New England and of the region of the great lakes, exhibit a striking resemblance, while those of the third class of the same division are very dissimilar. In the climate of the third class of posts, distinguished by great extremes of temperature, by seasons strongly contrasted, and a corresponding dryness of the atmosphere, (unlike the first two classes, in which the air is moist and the changes of the seasons slow and uncertain,) a constant and rapid succession is observed among the seasons. Summer, for example, succeeds winter so rapidly that there is scarcely any spring, the influence of which is surprisingly manifested in the vegetable kingdom. As the summers of the third class are remarkable for extremes of temperature, the mercury often rising in June, July, and August, to 100° Fahr. in the shade, so the winters are equally characterized by extreme severity. From November to May, cold weather prevails, the ground being often covered with snow to the depth of three or four feet, and the general range of the thermometer being from the freezing point to 30° below zero.

The lowest temperature, taking the mean of a month, occurred at Forts Howard and Snelling. At the former, the mean of the month of February, 1829, at 7 o'clock A. M., is $-3°{\cdot}17$, and the mean of December, 1822, at Fort Snelling, is $-3°{\cdot}61$. This, it is to be observed, is merely the average of the morning observations for the month. Although the extreme severity of the winters at the posts remote from large bodies of water, has been already fully illustrated ; yet the following remarks made by Surgeon Beaumont when stationed in 1829 at Fort Crawford, Wiskonsan, which is in the latitude of Fort Wolcott, R. I., may be added in further elucidation : " The month of January was remarkably mild and pleasant, the ground dry and free from snow, and the Mississippi unusually low and unfrozen. February was extremely cold, the weather clear and dry, and the thermometer ranging during the month from the freezing point to 23° below zero. From the 1st to the 16th, the mercury stood every morning, with the exception of three, (the 6th, 7th, and 8th,) between $-4°$ and $-23°$, and did not rise above 20° above zero during these days. On the 2d, 3d, 4th, 5th, 9th, 10th, 11th, 13th, 14th, and 15th, the mercury at sunrise stood respectively at 14°, 16°, 4°, 16°, 23°, 18°, 20°, 18°, 10°, 6°, and 4° below zero ; and on the 9th and 14th, it continued under $-8°$ during the 24 hours. During the month the prevailing winds were northerly and dry, and the proportion of fair and cloudy weather was—clear twenty-two days, cloudy three, variable one, and snowy two. The mean depth of snow was about six inches. The month of March has been unusually cold and dry, with one or two light falls of snow, which, with the previous coat, has just been dissolved by the warmth of the solar rays without any rain. The ice on the Mississippi, which broke yesterday, [March 30th,] is now moving off *en masse.*"

Scarcely does a winter elapse that the Hudson River is not frozen over even in the vicinity of the city of New York; while Philadelphia, and even Baltimore, lying on the same parallels which in Europe produce the olive and the orange, have their commerce often interrupted from the same cause. The Delaware, which is the latitude of Madrid and Naples, is generally frozen over five or six weeks each winter. Even the Potomac becomes so much obstructed by ice that all communication with the District of Columbia by this means, is suspended for weeks. Further north, the mouth of the St. Lawrence is shut up by ice during five months in the year; and Hudson's Bay, notwithstanding it is in the same latitude as the Baltic Sea, and of thrice the extent, is so much obstructed by ice, even in the summer months, as to be comparatively of little value as a navigable basin.

We find, however, even on our northern coast, a climate comparatively mild. As Nova Scotia is perfectly insular, with the exception of a neck of land eight miles wide, and is so much intersected by lakes and bays that nearly one-third of the surface is under water, the mercury seldom rises above 88° in summer, or sinks lower than 6° or 8° below zero in winter. In addition to this, some influence must be exercised by the Gulf Stream, which strikes upon this part of the coast, "in tides of from 60 to 70 feet, overflows the country to the distance of several miles, and converts the mouths of streams, fordable at low water, into extensive arms of the sea, where whole fleets may ride at anchor."

The meteorological phenomena of Canada, Nova Scotia, New Brunswick, and Newfoundland, according to the data furnished in the British Army statistics, are in perfect harmony with the laws of climate developed in the United States. The climate of Nova Scotia, from the causes just stated, exhibits a marked contrast to that of Lower Canada on the same parallels. In Newfoundland, the climate is similar to that of Nova Scotia; but the summers, in consequence of the melting of the icebergs on the coast, are less warm, of shorter duration, and subject to more sudden vicissitudes. In Canada, remote from the Lakes, the climate is of the most excessive character. At Quebec, when walking along the streets, the sleet and snow frequently freeze in striking against the face; and here too the alternations of temperature are so sudden, that the mercury has been known to fall 70° in the course of twelve hours. Cold weather sets in as early as November, from the end of which month till May the ground remains covered with snow, to the depth of three or four feet. When the winds blow with violence from the northeast, the cold becomes so excessively intense, that the mercury congealed in the thermometer serves no longer to indicate the reduction of temperature. Wine and even ardent spirits become congealed into a spongy mass of ice; and as the cold still augments, there follows congelation of the trees, which occasionally burst from this internal expansion, with tremendous noise. During winter, the general range is from the freezing point to 30° below zero. The seasons do not, as in more temperate regions, glide imperceptibly into each other. In June, July, and August, the heat, which often attains 95° Fahr., is frequently as oppressive as in the West Indies.

On our western coast, the extremely modified climate of the region of Oregon, on a parallel five degrees north of the city of New York, has been already illustrated. During a year's observations at Fort Vancouver, the lowest point is 17°, and the whole number of days below the freezing point, are only nine, all of which are noted in January. We are told by Mr. Ball, of the State of New York, by whom these observations were made, that he commenced plowing in January of the year 1833. "The vegetables of the preceding season," he says, "were still standing in gardens untouched by the frost. New grass had sprung up sufficiently for excellent pasture. * * * Though the latitude is nearly that of Montreal, mowing and curing hay are unnecessary, for cattle graze on fresh-growing grass through the winter. * * * Winters on the Columbia River are remarkably mild, there being no snow, and the river being obstructed by ice but a few days during the first part of January. Grass remained in sufficient perfection to afford good feed; and garden vegetables, such as turnips and carrots, were not destroyed, but no trees blossomed till March, except willow, alders, etc."

2. *Middle division.*—This division comprises two general systems of climates, which bear, in some degree, the same meteorological relation to each other as the modified climate of the great

lakes and the coast of New England does to that of the third class of the same division. The posts furnishing the meteorological data of the Middle division are the following:—Fort Mifflin, near Philadelphia, Washington City, Jefferson Barracks, near St. Louis, Fort Monroe or Old Point Comfort, in Virginia, Fort Gibson, in Arkansas, Fort Johnston on the coast of North Carolina, Augusta Arsenal, Georgia, Fort Moultrie, Charleston Harbor, and Fort Jesup, near Sabine River, Louisiana. The laws of climate developed in the preceding division, do not find so happy an illustration in this one; for as the physical causes act less prominently, the effects are less marked. These posts cannot be happily arranged into the two classes of uniform and excessive climes, as the majority of them are of a mixed character. Fort Mifflin and Washington City do not properly pertain to either class, being in a measure under the influence of the Atlantic, while the south-western stations experience the powerful agency of the Gulf of Mexico. As we proceed south, the seasons become, as a general rule, more uniform in proportion as the mean annual temperature increases. Although the thermometrical results given at Washington City, fairly place it in the class of excessive climates, yet on following the same parallel westward, a still greater contrast in the seasons is exhibited. Thus the difference between the mean temperature of winter and summer at Jefferson Barracks, notwithstanding it is about half a degree further south than Washington City, is 1°·80 greater; and on comparing Fort Gibson, Arkansas, with Fort Monroe on the coast of Virginia, though the latter is 1° 15′ north of the former, the difference at Fort Gibson, in the same respect, is 3°·69 greater. Fort Johnston, on the coast of North Carolina, which is 0° 32′ north of Augusta Arsenal, Georgia, also exhibits a less extreme in the opposite seasons. Fort Mifflin, near Philadelphia, shows a greater contrast in the opposite seasons, (so all-powerful is the equalizing influence of large bodies of water,) than any one of the following posts, all being from two to seven degrees farther north, viz. Brady, Sullivan, Preble, Niagara, West Point, Constitution, Wolcott, and Trumbull; and Washington City exhibits greater extremes than the three last named.

The general laws in reference to the difference between the mean temperature of winter and spring, already revealed in the Northern division, are here confirmed. Jefferson Barracks shows a greater inequality than Washington City, and Fort Gibson than Fort Monroe. Fort Jesup cannot be fairly compared, by way of contrast, with a position in the same latitude on the Atlantic, as the warm atmospheric currents from the Gulf of Mexico exercise there a very appreciable influence.

The laws developed as respects the mean annual range of the thermometer are also here corroborated. Washington City has a mean annual range of 84°, while that of Jefferson Barracks is 89°; the ratio of Fort Monroe, on the one hand, is 73°, and that of Fort Gibson, on the other, is 89°; and lastly, the range at Fort Johnston is 62°, while that of Augusta Arsenal is 73°.

It is thus seen that the climate of the region of the great lakes on our nothern frontier is not more contrasted in the opposite seasons than that of Philadelphia—an inference long since deduced from the fact that similar vegetable productions are found in each, while the same plants will not flourish in the interior of New York, Vermont, and New Hampshire. The region of Pennsylvania, as though it were the battle-ground on which Boreas and Auster struggle for mastery, experiences, indeed, the extremes of heat and cold. But proceeding south along the Atlantic plain, climate soon undergoes a striking modification, of which the Potomac River forms the line of demarcation. Here the domain of snow terminates. Beyond this point, the sledge is no more seen in the farmer's barn-yard. The table-lands of Kentucky and Tennessee, on the other hand, carry, several degrees farther south, a mild and temperate clime.

3. *The Southern division*, which is characterized by the predominance of high temperature remains to be considered. On approaching our southern coast, climate undergoes a most remarkable modification. The seasons glide imperceptibly into each other, exhibiting no great extremes. This is strikingly illustrated on comparing the difference between the mean temperature of summer and winter at Fort Snelling, Iowa, and at Key West, at the southern point of Florida, the former being 56°·60, and the latter only 11°·34. Compared with the other regions of the United States, the peninsula of Florida has a climate wholly peculiar. The lime, the orange, and the fig, find there a genial temperature; the course of vegetable life is unceasing; culinary vegetables are cultivated, and wild flowers spring up and flourish in the month of January; and so little is the temperature of the lakes

and rivers diminished during the winter months, that one may almost at any time bathe in their waters. The climate is so exceedingly mild and uniform, that besides the vegetable productions of the southern States generally, many of a tropical character are produced. The palmetto or cabbage palm, the live-oak, the deciduous cypress, and some varieties of the pine, are common farther north; but the lignum-vitæ, mahogany, logwood, mangrove, cocoa-nut, etc., are found only in the southern portion of the peninsula. Here also, in common with our southern borders, the fig, date, orange, lemon, citron, pomegranate, banana, olive, tamarind, papaw, guava, as well as cotton, rice, sugar-cane, indigo, tobacco, maize, etc., find a genial climate. In contemplating the scenery of East Florida in the month of January, the northern man is apt to forget that it is a winter landscape. To him all nature is changed; even the birds of the air—the pelican and flamingo—indicate to him a climate entirely new. The writer being attached in January, 1838, to a boat expedition, the double object of which was to operate against the Seminoles and to explore the sources of the St. John's, found, in the midst of winter, the high cane-grass, which covers its banks, intertwined with a variety of blooming morning-glory, (Convolvulus.) The thermometer at mid-day in the shade, stood at 84° Fahr., and in the sun rose to 100°; and at night we pitched no tents, but lay beneath the canopy of Heaven, with a screen perhaps over the face as a protection against the heavy dews. Notwithstanding the day attains such a high temperature, the mercury just before daylight often sinks to 45°, causing a very uncomfortable sensation of cold. Along the southeastern coast, at Key Biscayno, for example, frost is never known, nor is it ever so cold as to require the use of fire. In this system of climate, the rigors of winter are unknown, and smiling verdure never ceases to reign.

The climate of Pensacola and of New Orleans, the former represented by Cantonment Clinch and the latter by Petite Coquille, the two posts being respectively in the vicinity of these cities, is nearly as much modified, (in consequence of the agency of the Gulf of Mexico, and in regard to New Orleans the additional influence of large lakes,) as similar parallels in East Florida. The laws of temperature relative to East Florida have been perhaps more satisfactorily determined than in any other region of the United States. We have here the data of four posts fortunately situated, viz. Fort Marion at St. Augustine, on the eastern coast, —Fort Brooke at the head of Tampa Bay,* about thirty miles from the Gulf of Mexico,—Fort King, intermediate to these two points,—and Key West, belonging to the Archipelago, about sixty miles southwest of Cape Sable. As Fort King is situated in the interior, and the other three posts are on the coast, we have an additional illustration, even in a climate characterized by very little distinction of the seasons, of the modifying agency of large bodies of water; for the mean temperature of winter at Fort King is lower, and that of summer higher, than at the other three posts. Although Key West, which is 4° 39′ south of Fort King, has a mean annual temperature 3°·43 higher, yet the mean summer temperature is 2°·81 lower—a law which is strikingly illustrated on the map of the United States, which shows that the isotheral line of Key West cuts Savannah, Augusta, and Fort Gibson. This equalizing influence of the ocean is still further exhibited in the annual range of the thermometer, the mean of the monthly ranges, and the average difference of the successive months.† During the summer months, the morning and evening observations at Fort King and Key West are nearly the same, the disparity being caused by the exalted temperature of the former at mid-day. As is usual in southern latitudes, there is a little variation presented at Key West in the mean temperature of the same month in different years. Within the period of six years, (from 1830 to 1835 inclusive,) the mercury at Key West was never known to rise higher than 90°, or sink lower than 44°.

There is little difference between the thermometrical phenomena presented at Key West and the Havana. In the West India islands, the mean annual temperature near the sea is only about 80°. At Barbadoes, the mean temperature of the seasons is as follows: winter 76°, spring 79°, summer 81°, and autumn 80°. The temperature is remarkably uniform; for the mean annual range of the thermometer, even in the most excessive of the islands, is, according to the British army statistics, only 13°, and in some not more than 4°. Contrast this with Hancock Barracks, Maine, which gives an average annual range of 118°, Fort Snelling, Iowa, 119°, and Fort Howard, Wiskonsan, 123°!

* The old Spanish appellation was Espiritu Santo, or Bay of the Holy Ghost, the name Tampa being then restricted to an arm.

† All these various results are presented in a tabular form in the author's work on "The Climate of the United States and its Endemic Influences."

The peculiar character of the climate of East Florida, as distinguished from that of our more northern latitudes, consists less in the mean annual temperature than in the manner of its distribution among the seasons. At Fort Snelling, for example, the mean temperature of winter is 15°·95, and of summer 72°·75, whilst at Fort Brooke, Tampa Bay, the former is 64°·76, and the latter 81°·25, and at Key West 70°·05 and 81°·39. Thus, though the winter at Fort Snelling is 54°·10 colder than at Key West, yet the mean temperature of summer at the latter is only 8°·64 higher. In like manner, although the mean annual temperature of Petite Coquille, Louisiana, is 2° lower—that of Augusta Arsenal, Georgia, nearly 8°—and that of Fort Gibson, Arkansas, upward of 10° lower—than that of Fort Brooke; yet at all, the mean summer temperature is higher. Between Fort Snelling, on the one hand, and Fort Brooke and Key West on the other, the relative distribution of temperature stands thus: difference between the mean temperature of summer and winter at the former 56°·60, and at the two latter 16°·49 and 11°·34; difference between the mean temperature of the warmest and coldest month, 61°·86 compared with 18°·66 and 14°·66; difference between the mean temperature of winter and spring, 30°·83 to 8°·35 and 5°·99; and the mean difference of successive months, 10°·29 to 3°·09 and 2°·44.

The diverse climatic peculiarities of Fort Snelling and Key West are delineated in the accompanying engraving, Plate II. The contrast in the course of the mean annual temperature of these two posts, as traced through each month, is indeed striking, while the variation of temperature on each of these monthly lines is still more marked. Although the average minimum temperature of Fort Snelling in January is as low as 22° below zero, while that of Key West is 57° above; yet, strange to say, we find the mean maximum temperature of July at the former 5° higher than at the latter. The course of the seasons are equally marked in their contrasts; for while the curves of Key West are confined within a few degrees, those of Fort Snelling are so opposite that the lines of spring and autumn traverse each other at right angles, and those of summer and winter are so remote that the one is truly hyperborean, and the other tropical.

This remarkable equality in the distribution of temperature among the seasons in Florida, compared with the other regions of the United States, constitutes its chief climatic peculiarity; and the comparison, if extended to the most favored situations on the continent of Europe, and the various islands of the Mediterranean and Atlantic held in highest estimation for mildness and equability of climate, is no way disparaging. A comparison of the mean temperature, that of the warmest and coldest month, and that of successive months and seasons, results generally in favor of peninsular Florida. The mean difference of successive months stands thus: Pisa, 5°·75; Naples, 5°·08; Nice, 4°·74; Rome, 4°·39; Fort King, interior of Florida, 4°·28; Fort Marion, at St. Augustine, 3°·68; Fort Brooke, on the western side of Florida, 3°·09; Penzance, England, 3°·05; Key West, near the southern point of Florida, 2°·44; and Madeira, 2°·41. The mean annual range thus: Fort King, 78°; Naples, 64°; Rome, 62°; Nice, 60°; Montpelier, 59°; Fort Brooke, 57°; St. Augustine, 53°; Penzance, 49°; Key West, 37°; and Madeira, 23°.

The want of instrumental observations until recently to indicate with precision the actual or comparative humidity of the atmosphere in Florida is to be regretted. That the air is much more humid than in our more northern regions is sufficiently cognizable to the senses. The deposition of dew, even in the winter, is generally very great. To guard against the oxidation of metals, as for instance surgical instruments, is a matter of extreme difficulty. During the summer, books become covered with mould, and keys rust in one's pocket. *Fungi* flourish luxuriantly. The writer has known a substance of this kind to spring up in one night, and so incorporate itself with the tissue of a woollen garment as to render separation impracticable. As the rains however generally fall at a particular season, the atmosphere in winter is comparatively dry and serene. The following abstract of the monthly fall of rain at Key West is the mean of five years' observations:

January 1·82, February 1·34, March 1·98, April 1·09, May 6·34, June 2·39, July 2·84, August 3·30, September 4·35, October 3·33, November 1·49, December 1·13, Annual average 31·40.

During six months, from November to May, it will be observed that the proportion of rain is but 8·84 inches, being little above one fourth of the annual quantity. Now as in tropical climates, a portion of the year is known as the rainy season, and as the same quantity of rain descends in a considerably shorter space of time than in the temperate zone, it follows that the proportion of fair days and clear skies is infinitely in favor of the former. This is strikingly evidenced in a comparison of Fort King, in the interior of East Florida, and of our northern lakes already adverted to, the annual number of fair days at the former being 309, and at the latter only 117. On the coast of Florida, however, the average is not more than 250 days.

Thus it is demonstrated that invalids requiring a mild winter residence, have gone to foreign lands in search of what might have been found at home, viz. *an evergreen land in which wild flowers never cease to unfold their petals.* But to treat of the advantage of peninsular Florida as a *winter* residence for pulmonic and other invalids from more northern latitudes, would be incompatible with our present object.

Having completed the details relative to each division of the United States, we may now take a glance at the general laws of climate as illustrative of their harmony throughout the globe. It is an important general law in reference to both continents, that a striking analogy exists, on the one hand in the climatic features of the western coasts, and, on the other hand, in those of the eastern shores. Thus in tracing the same isothermal line around the northern hemisphere beyond the tropic, it presents on the east side of both continents *concave*, and on the west side *convex*, summits. Following the mean annual temperature of 55°·40 Fahr. around the whole globe, we find it passes on the

E. coast of old world, N. Lat. 39° 54′, E. Lon. 116° 27′, near Pekin:
E. coast of new world, N. Lat. 39° 56′, W. Lon. 76° 16′, Philadelphia.
W. coast of old world, N. Lat. 45° 46′, W. Lon. 0° 37′, near Bordeaux.
W. coast of new world, N. Lat. 44° 40′, W. Lon. 104° 0′, Cape Foulweather, south of the mouth of Columbia.

On comparing the two systems, the concave and convex summits of the same isothermal line, "we find," says Humboldt, "at New York the summer of Rome and the winter of Copenhagen; and at Quebec the summer of Paris and the winter of Petersburg. In China, at Pekin, for example, where the mean temperature of the year is that of the coast of Brittany, the scorching heats of summer are greater than at Cairo, and the winters are as rigorous as at Upsal."

The difference of climate between western Europe and eastern North America, was long since determined by Humboldt in his paper on *Isothermal Lines and the Distribution of Heat over the Globe;* but these various relations, owing of course to the paucity of his data, are not characterized by much precision.

The isothermal line of 41°, which, according to this philosopher, passes through the Bay of St. George, in Newfoundland, in the latitude 48°, if correctly ascertained, sinks as it penetrates towards the interior of the continent; for at Hancock Barracks, Maine, in latitude 46° 10′, at a distance of one hundred and fifty miles from the Atlantic, the mean annual temperature is 41°·21, and at Fort Brady, at the outlet of Lake Superior, in latitude 46° 39′, it is 41°·39; and proceeding to the western coast of America, we find that at Fort Vancouver, Oregon Territory, in latitude 45° 37′, the mean temperature, like similar parallels in western Europe, is as high as 51°·75.

As the region of the United States, however, exhibits very diverse systems of climate even on the same parallels, such comparative tables, as for instance the difference of the seasons from the equator to the polar circle, can present only the most general laws. For example, it shows that on the isothermal line of 41°, the mean temperature of winter is 14°, and that of summer 66°·20—a result obtained from observations made in lat. 48°, on the Bay of St. George, Newfoundland. Now, according to the "Army Meteorological Register," this isothermal line is again found in the comparatively equalized climate of Fort Brady, at the outlet of Lake Superior, in lat. 46° 39′, where the mean temperature of winter is as high as 21°·07, while that of summer is only 63°·18. Again, the table shows that on the isothermal line of 50°, the mean temperature of winter is 30°·20, and that of summer 71°·50; but this too gives only a partial view, as at Fort Wolcott, Rhode Island, the former is 32°·51 and the latter 69°·06, and at Council Bluffs, near the junction of the Platte and Missouri, 24°·47 and 75°·82; thus showing that the disparity in the mean temperature of winter and summer, on the same parallel of latitude and on the same isothermal line, (that of Fort Wolcott being 50°·61, and that of Council Bluffs 51°·02,) is 14°·80 greater in an *excessive* than in a *uniform* climate.

As those who first observed the climatic difference between western Europe and eastern North America, were natives of the former, they of course regarded the climate of their own country as constituting the rule, and that of America as the exception; while some, even now, as for instance Lyell, as already quoted, make Europe the exception to the general rule. But when these facts came to be generalized, it was discovered that the eastern coasts of both continents have a lower annual temperature and more contrasted seasons than the western in corresponding latitudes. These results find a satisfactory explanation in physical causes, thus demonstrating the harmony of the laws of climate throughout the globe.

Did space allow, it would be easy to show that the *rationale* of all these laws finds a ready explanation in the phenomena of the polar and equatorial currents, in connection with certain local causes. Suffice it to refer to a single explanation. The winds without the tropics have a prevailing direction from the west,—a fact which affords a solution of the problem that in extra-tropical latitudes, countries lying to the eastward of seas or other great bodies of water, have milder climates than those situated on the eastern portions of a continent. That this westerly breeze prevails with considerable regularity, is apparent from the following observations made by John Hamilton, during twenty six voyages between Philadelphia and Liverpool from 1798 to 1817, showing that the winds were more than half the time from the west. Thus, out of 2029 days, the wind prevailed

From northward, ...	208	days.
" southward, ...	167	"
" eastward, ...	361	"
" westward, ...	1101	"
Variable, ...	192	"
	2029	

We thus perceive at once the principal cause of the rise of the isothermal line on the western coast of continents in extra-tropical latitudes; for there is thus swept from the ocean, which never sinks below the freezing point, a humid atmosphere, which, in its passage over the land, has a constant tendency to establish an equilibrium of temperature, and as its vapor is gradually condensed, it also evolves its latent heat. As large bodies of water never become so cold in winter or so warm in summer as the earth, the winds that sweep from them have a constant tendency to maintain an equilibrium of temperature. Land winds, on the contrary, must necessarily bear with them the greater or less degree of cold induced by congelation, while in summer they will convey the accumulated heat absorbed by the earth; and thus is produced, in a great measure, those extremes of the seasons which characterize extra-tropical latitudes on the eastern coasts of continents.

The difference of temperature on the eastern and western coast of continents is still further increased by local causes. Europe is separated from the polar circle by an ocean, while eastern America stretches northward at least to the 82° of latitude. The former, intersected by seas, which temper the climate, moderating alike the excess of heat and cold, may be considered a mere prolongation of the old world; while the northern lands of the latter, elevated from three thousand to five thousand feet, become a great reservoir of ice and snow, which diminishes the temperature of adjoining regions. "America," says Mr. Phillips, "with little north tropical and wide polar land, gives us a case of extreme refrigeration from the pole towards the equator; Africa and the west of Europe compose a surface of wide and hot north tropical land, with free channels to a polar sea." Hence Lapland, under the 72°, experiences a less rigorous climate than Greenland under the 60th parallel. On the other hand, between the 40th parallel and the equator, the influence of land, if not very elevated, produces effects diametrically opposite; for, the surface of the earth absorbs a large quantity of caloric, which is diffused by a radiation into the atmosphere. Thus Africa, as Malte-Brun observes, "like an immense furnace, distributes its heat to Arabia, to Turkey in Asia, and to Europe." On the contrary, the north-eastern extremity of Asia, which extends between the 60th and 70th parallel, and is bounded on the south by water, experiences extreme cold in corresponding latitudes.

Another cause contributing to the same effect is the Gulf Stream, the warm air arising from which being wafted by the westerly winds mainly to the shores of Europe. But independent of the westerly winds, which transport the tempered atmosphere of the Pacific over the land, and conversely, in traversing the continent, bear upon their wings the accumulating cold towards our eastern shores, we observe, in attempting to account for the extraordinary dissimilitude in the climate of our two coasts, on the eastern side an unascertained prolongation of the continent towards the pole and an oceanic current sweeping immense masses of ice southwardly, while on the western side, the great range of Rocky Mountains shelters Oregon from the polar winds, and the projecting mass of Russian America protects it from the polar ice.

Connected with this subject is the question frequently agitated, whether the old continent is warmer than the new. Volney and others have attempted its solution by a comparison of the mean annual temperatures of different places on both sides of the Atlantic; but to this mode of determining it, the objection at once presents itself, that the points of comparison represent opposite extremes in the climate of each continent. Indeed, the question in itself involves an absurdity; for as the laws of nature are unvarying in their operation, and as similar physical conditions obtain in corresponding parallels of both continents, the same meteorological phenomena will be induced. It shows in lively colors the truth of the remark, that every physical science bears the impress of the place at which it received earliest cultivation. In geology, for example, all volcanic phenomena were long referred to those of Italy; and in meteorology, the climate of Europe has been assumed as the type by which to estimate that of all corresponding latitudes. In making a comparison of the two continents, it is, therefore, necessary that both points have the same relative position. Fort Sullivan, Maine, notwithstanding it is more than 11° south of Edinburgh, Scotland, exhibits a mean annual temperature $5\frac{1}{2}$° lower; Bordeaux, which is parallel with Fort Sullivan, has an annual temperature 15° higher; and the mean of Stockholm, in lat. 59° 20′, is about the same as that of Fort Sullivan, in lat. 44° 44′. These are not, however, legitimate points of comparison. Pekin and Philadelphia, each on the eastern coast of its respective continent, are fair examples, having the same latitude and a similar relative position, and consequently the same mean annual temperature. A comparison between western Europe and the United States would be equally improper with a comparison between it and China. "Thus at Pekin, in lat. 40° N., long. 116° 20′ E.," says Dr. Traill,* "the mean temperature of summer is 78°·8, and of winter 23°—a difference of not less than 55°·8, which gives rise to a frost of several months' duration in that part of China; yet Pekin is under the same parallel as the southern extremity of Naples, where frost is unknown, and

* Encyclopedia Britannica.

of the central provinces of Spain, in which, though at an elevation of two hundred feet above the sea, ice is an extremely rare occurrence."

Now, at Philadelphia, on an average of thirty two years, the mean summer temperature is only 73°·17 and that of winter is as high as 32°·96, making a difference of 40°·21. Hence, as this difference is nearly 15° less at Philadelphia than at Pekin, and as the same result appears in the subjoined comparison of similar parallels in Oregon and in western Europe, it follows, contrary to general, indeed we may say *universal*, opinion, that the new world, so far from having a climate more austere than the old, has in fact, if there really exists any difference, a more mild and uniform temperature.

It is only within the temperate zone, from 30° to 60° of north latitude, that the year exhibits the grateful vicissitudes of the four seasons—the varied charms of spring and autumn, the tempered fires of summer, and the healthful rigors of winter. Wisdom desires not that "eternal spring," the want of which poets affect to deplore. At the equator there is no difference between the mean temperature of summer and winter, but it increases, as a general rule, with the latitude. From Florida to Canada, the contrast in the seasons increases in proportion as the mean annual temperature decreases—a general law subject to modification on every parallel, in accordance with the varieties in physical geography. It has been already seen, that the greatest and the least contrasts of winter and summer are exhibited at Fort Snelling and Key West. Upon this point, Humboldt has, as usual, determined many important laws.

"The winters of the isothermal curve of 68°," he says, "are not found upon that of 51°, and the winters of 51° are not met with on the curve of 42°." In considering separately what may be regarded as the same systems of climate, for example, the European region, the transatlantic region, or that of eastern Asia, the limits of variation become still more narrow. Wherever in Europe, in 40° of longitude, the mean temperature rises—

To 59°·00	the winters are from	44°·60 to 46°·40	and the summers from	73°·00 to 75°·00
" 54°·50		36°·50 to 41°·00		68°·00 to 73°·00
" 50°·00		31°·10 to 37°·40		62°·60 to 69°·80
" 45°·50		28°·40 to 36°·10		57°·20 to 68°·00
" 41°·00		20°·30 to 26°·80		55°·40 to 66°·00

In the United States, if the comparison is confined to the same system of climates, as for example the posts on the ocean or lakes, or those remote from the agency of large bodies of water, the limits of variation, as in Europe, are also narrow; but if the whole extent of our domain is embraced, the results are strikingly diverse. Thus—

	MEAN TEMPERATURE.		
	Annual.	Winter.	Summer.
Fort Vancouver, Oregon Territory,	51°·75	41°·33	65°·00
Council Bluffs, junction of Platte and Missouri,	51°·02	24°·47	75°·82
Difference,	0°·73	+16°·86	−10°·82

Here, then, although there is not a degree of difference in the mean annual temperature of Fort Vancouver and Council Bluffs, yet the mean winter temperature of the latter is nearly seventeen degrees *lower*, while the mean summer temperature is nearly eleven degrees *higher*. But this contrast is exhibited in a still more marked degree by comparing the difference between the mean temperature of winter and summer, the former being 23°·67, while the latter is 51°·35.

"In tracing five isothermal lines between the parallel of Rome and St. Petersburg," continues Humboldt, "the coldest winter presented by one of these lines is not found again on the preceding line. In this part of the globe, those places whose annual temperature is 54°·50 have not a winter below 32°, which is already felt upon the isothermal line of 50°."

In the European climate, two points having the same winter temperature may differ as much as 11° in latitude. Thus in Scotland, in latitude 57°, and isothermal line 45°·50, the winters are more mild than at Milan, in latitude 45° 28′, and isothermal line 55°·80. Consequently the lines of equal winter cut isothermal lines which differ 10°. At the isle of Maggeroe, at the northern extremity of Europe, under the parallel of 71°, the winters are 7° milder than at St. Petersburg, latitude 59° 56′. In the United States, embracing the whole region between the Atlantic and the Pacific, as great a contrast no doubt exists. The mean winter temperature of Fort Vancouver, Oregon Territory, latitude 45° 37′, is found about 9° farther south at a point intermediate to Fort Gibson and Jefferson Barracks; but if the observations, like those in Scotland just referred to, were made on the coast, (Fort Vancouver being 70 miles distant from the Pacific,) the winter temperature would necessarily be still higher.

As the mean annual temperature of Fort Vancouver is 51°·75, and that of the assumed point between Fort Gibson and Jefferson Barracks is about 61°, it follows that the lines of equal winter cut isothermal lines which differ more than 9° of Fahrenheit. (See Plate II.)

In Europe a greater deviation from the terrestrial parallels is caused by the inflections of the isocheimal than by the isothermal lines; for while two points having the same winter temperature may differ as much as 11° in latitude, a difference of not more than 5° is found between any two places having an equal annual temperature—disparities which increase as the eastern coast of Asia is approached. In the United States the same law obtains; for between the isothermal line of Fort Vancouver and the same in the Atlantic region, the difference is only 4° of latitude. (See Plate II.)

The isotheral curves or lines of equal summer follow a direction opposite to that of the isocheimal lines. The region about Moscow and that about the mouth of the Loire, in France, notwithstanding differing 11° in latitude, present the same summer temperature. Although this result, as regards difference of latitude, is not discovered in the United States, yet the most extraordinary results in this respect have been demonstrated on the same parallel running from the Atlantic through the great lakes. In the United States, the heats of summer are every where intense. At Fort Snelling, notwithstanding the isocheimal line is 54° lower than at Key West, the isotheral is only 8° lower. (See Plate II.) At Fort Vancouver, the mean summer temperature is 2° or 3° higher than on the same parallel in the region of the Atlantic and the great lakes, and about 7° lower than in the excessive climates of the same region. In tracing an isothermal line around the globe, we find that the same causes which, on the Atlantic coast of North America and in the north of China, depress the curves of equal annual heat, tend to elevate the isotheral curves or lines of equal summer. Thus, in following the isothermal line of 51° around the globe, and adding the indications of the mean temperature of summer and winter at its summits and depressions, we find it marked in England, $\frac{37°·90}{62°·80}$; in Hungary, $\frac{31°·80}{70°·10}$; in China, $\frac{24°·10}{79°·20}$; in western America, at Fort

Vancouver, $\frac{41°\cdot38}{65°\cdot00}$; and in eastern America, at Council Bluffs, $\frac{24°\cdot47}{75°\cdot82}$; and at Fort Wolcott, Rhode Island, $\frac{32°\cdot51}{69°\cdot06}$.

The law, as established by Humboldt, *that the same causes which produce the greatest convexity of the isothermal line also equalize the temperature of the seasons*, has been already well illustrated in the table tracing the isothermal line of 55°·40 around the earth. The same isothermal curve, according to Humboldt, runs on the western coasts about six degrees of latitude higher than on the eastern; but to illustrate the law that at this *convex* point the seasons are most equalized, another table becomes necessary. Thus the annual mean temperature being equal to the fourth part of the total of the winter, spring, summer, and autumnal temperatures, the same isothermal line of 53°·60 shows—

		Winter.	Spring.	Summer.	Autumn.
At the *concave* summit in America, 74° 40′ W. lon.	53°·60=	32°·00+	52°·30+	75°·60+	54°·50 (÷4)
At the *convex* summit in Europe, 2° 20′ W. lon.	53°·60=	40°·10+	51°·80+	68°·40+	54°·10 (÷4)
At the *concave* summit in Asia, 116° 20′ E. lon.	53°·60=	24°·80+	54°·70+	80°·60+	54°·30 (÷4)
At the *convex* summit in America, 122° 37′ W. lon.	51°·75=	41°·33+	48°·00+	65°·00+	52°·67 (÷4)

The first three results on the same isothermal line are furnished by Humboldt. Unable to obtain the same annual temperature on our Pacific coast, it becomes necessary to take a lower isothermal line, (that of Fort Vancouver in the "Army Meteorological Register,") which of course gives a contrast in the seasons correspondently greater. It is thus seen that on the western coasts, where the isothermal curve rises, or is *convex*, the seasons are much equalized, the difference between the mean temperature of winter and summer being only about one half as great as on the eastern coasts, where the line sinks, or is *concave*. But this may be better illustrated by a tabular arrangement.

	Isothermal line.		Diff. between mean temp. of winter and summer.
Asia, eastern coast,	53°·60	At depressions, . . .	55°·80
America, eastern coast,	53°·60	" " . . .	43°·60
Europe, western coast,	53°·60	At summits, . . .	28°·30
America, western coast,	51°·75	" " . . .	23°·70

It may be well to add, that with the exception of the last, the writer is not aware of the local position of these points of comparison,—a consideration which may be supposed to be of some importance, inasmuch as the northern division of the United States presents, on the same isothermal line, a difference between the mean temperature of winter and summer, varying from 38° to 54°. This does not, however, in the least affect the law of the climatic analogy of the eastern and western continental coasts.

But this law, that the same causes which increase the mean annual temperature also equalize the seasons, does not hold good in the United States, in receding from the Atlantic; for, on comparing the climate of the coast of New England with the still more excessive climate of the interior, it is found that the mean annual temperature of the latter is higher. That the climate should become more austere, the seasons being *less* equalized, is in accordance with the laws established by Humboldt; but that the isothermal line, at the same time, should become more *convex*, is in diametrical opposition.

Forts Sullivan, Snelling, and Howard, for example, have very nearly the same latitude; the first, on the ocean, has a mean annual temperature of 42°·95, while the last two, in the opposite system of climate, have a mean respectively of 45°·83 and 44°·92,—a result the more unexpected, at first sight, as the latter are in a region elevated six hundred to eight hundred feet above the level of the sea. Comparing Fort Wolcott, on the ocean, with Fort Armstrong, West Point, and Council Bluffs, in the interior, the same relation is found. Fort Trumbull, it is true, offers an exception; but it is necessary to bear in mind that the results of this post are based on two years' observation only, while those of Fort Wolcott are calculated from ten; and in further evidence of its probable erroneousness, it may be mentioned that the mean annual temperature at Fort Columbus, which is 0° 40′ farther south than Fort Trumbull, based on nine years' observations, is 2° less. Again, we find that while at Salem, near the Atlantic, in lat. 42° 34′, the mean annual temperature, based on thirty three years' observations, is 48°·61, it is, on the other hand, at Fort Armstrong, lat. 41° 28′, Council Bluffs, lat. 41° 45′, and at West Point, lat. 41° 22′, respectively 51°·63, 50°·50, and 52°·47. Here then is actually an increase of from two to four degrees in the annual temperature, while the interior posts are only about 1° nearer the equator, which cannot, on the average, cause a greater difference of temperature than 1°·50.

Having thus shown that there is an actual increase of annual temperature, or a rise of the isothermal line, on receding from the Atlantic, it is deemed unnecessary to give any details proving, that instead of the seasons becoming from the same causes more equalized, they actually grow more contrasted, inasmuch as this law has been already abundantly established. Suffice it to compare Fort Snelling on the Mississippi and Fort Sullivan on the Atlantic. Although the former has a mean annual temperature 2°·88 higher than that of the latter, yet it has a contrast between the mean temperature of winter and summer actually 17°·45 greater! Equally striking is the contrast between the results given by posts on the lakes and those in the same region, notwithstanding not more than one, two, or three hundred miles distant. Thus, on comparing Forts Snelling and Howard with positions (Forts Brady and Mackinac) in the modified climate of the lakes, this relation is discovered; for, although the mean latitude of the latter posts is only 1° 34′ north of Fort Snelling, (and perhaps four hundred miles distant,) yet the mean annual temperature is 4°·25 lower. Now, of this difference in annual temperature, not more than one half can be accounted for by difference of latitude, being an expression of the same law that was revealed by the comparison with posts modified by the ocean; and we also find, that so far from the temperature of the seasons being more equalized at Fort Snelling, which has a higher annual temperature, the difference between the mean temperature of summer and winter is in reality 12°·84 greater than on the lakes.

Humboldt's law holds good so far as the comparison refers to the eastern and western continental coasts, each being more or less modified by the ocean; but in a comparison with an interior position remote from large bodies of water, a new element, arising from the law of the accumulation of caloric by the surface of the earth, doubtless enters into the calculation. It may be said, however, that this ought to be compensated by the augmented cold of winter; but it is found in our excessive climates, compared with the modified, that the annual temperature gains more by the continued elevation of the thermometer in summer than it loses by its depression in winter. Besides, in excessive climates, the vernal increase alone often compensates for the low temperature of winter; for example, although the mean winter temperature at Fort Sullivan is 22°·95, and at Fort Snelling as low as 15°·95, yet that of spring is higher at the latter, being as 46°·78 to 40°·11. Then follows a mean summer temperature more than 10° higher in the excessive than in the uniform clime. The season of autumn, (September, October, and November,) is not perceptibly influenced by these causes.

These contrasts would be still more striking, were the comparisons instituted between points on the same isothermal line, instead of the same parallel of latitude; for, as the isothermal curve of Fort Sullivan would strike a point at least 2° north of Fort Snelling, the extremes of the seasons there would be correspondently augmented. Sufficient, however, has been adduced to prove that Humboldt's deduction, *that the same causes which produce the greatest convexity of the isothermal line, also equalize the temperature of the seasons*, is unwarranted as a general law. And here the writer may venture to add that these conclusions pertain wholly to himself, inasmuch as they had been, doubtless, never brought to the notice of the scientific world, before they were made known by him in his work on "The Climate of the United States and its Endemic Influences."

These results, in the comparisons just made, appear the more extraordinary, as some reduction of temperature, by reason of the elevation of these interior posts, would be *a priori* inferred; for, according to Humboldt, "elevations of four hundred meters, (one thousand three hundred and twelve feet,) appear to have a very sensible influence on the mean temperature, *even when great portions of countries rise progressively.*" That high table-lands have a more exalted temperature than isolated mountains of the same height is well known; for the elevated plains on which the towns of Bogota, Popayan, Quito, and Mexico are built, have a much warmer climate than they would have, if elevation above the sea were the only element that determines the temperature when the latitude is given. That our western table-lands, rising gradually to the height of eight hundred feet, cause no diminution of temperature, has been already abundantly established.

Without attempting here to explain the diminution of temperature on the summits of high mountains, it may be remarked that these causes cannot be in operation when a large region of country rises very slowly and progressively to a height less than one thousand feet. It is only when lands are considerably and suddenly elevated, and exposed to the action of the atmosphere laterally, that a rapid conduction of heat and rarefaction of the atmosphere can take place. Our northwestern region, in those districts which are remote from the great lakes, so far from causing a diminution of annual temperature, produces, it has been seen, an augmentation.

In regard to the extremes of heat and cold in the United States, it would be natural to expect that the severest cold would be registered at the most northern, and the greatest heat at the most southern posts. It is now, however, proved by exact instrumental observations that this is not the case, as these are situated on large bodies of water; but that the western stations, Forts Snelling, Gibson, and Council Bluffs, remote from inland seas, are remarkable for extremes of temperature. It is here that the mercury rises the highest and sinks the lowest, while Forts Brady and Mackinac, the most northern stations, as well as those on the southern coast, exhibit a lesser range of the thermometer; and in accordance with the same law, we find that the mean summer temperature is greater at Augusta, Georgia, than along the coast of Florida. While at Key West, during a period of six years, the thermometer never rose above 90°, it attained at Council Bluffs, a point 17° 12′ farther north, a height every year varying from 102° to 108°. The highest temperature in the shade noted at various posts, was at Fort Gibson, on the 15th of August, 1834, being 116°. In Africa, the mercury is sometimes seen at 125°, and in British India it is said to have been as high as 130°. It has been remarked that on the coast of Senegal the human body supports a heat which causes spirits of wine to boil, and that in the northeast of Asia, it resists a cold which renders mercury solid and malleable. Although the mean annual temperature, in proceeding from the equator toward the poles, gradually diminishes, yet the thermometer scarcely mounts higher at the equinoctial line than under the polar circle. Hence it follows that the climate of the tropics is characterized much more by the duration of heat than its intensity.

(*To be concluded in our next No.*)

Art. III.—*A new Method proposed for computing Interest;* by Geo. R. Perkins, A. M.

Perhaps there is nothing which requires more to be well understood by all business men, than the principles and rules in relation to interest of money.

According to all the methods now in use in different countries, as well as the various rules adopted by our different states, there is wide room for different individuals to arrive at very different results in computing the interest on many bills of obligation. That which gives rise to the most difficulty, is the distinction between *simple* interest and *compound* interest. By the laws of this country, a loan for any period, even though it exceeds one year, is not allowed any more than simple interest; while at the same time the loan may be made for as short a period as we please, and at the end of said period the interest may be added to the principal and the result again loaned as a new principal; by which means it may be made to draw even a greater interest than the ordinary compound interest. So long as the statute of the land has laws to prevent usury, it ought to provide some certain and infallible method for computing interest.

Another common source of perplexity in computing interest, is in the case of bills of obligation, where payments are made at various periods, some being made at intervals less than one year, and others at longer intervals. So great has this source of perplexity been, that our different states have adopted their distinct rules in regard to the method of computing interest in such cases.

It is obvious that so long as the unit of time is a *finite* quantity, as one year, no rule can be devised which shall in all cases be equitable to both parties, for when the payments are made before the end of the year, they must affect the parties different from what they would when made after they became due. By the many ways in which contracts are drawn, new cases are almost daily presented, which require much labor as well as skill to determine the exact rate per cent. per annum received.

I see no way by which such difficulties can be avoided, so long as a finite portion of time is allowed before the interest is considered as due. It may be asked, why is a man's interest any more due at the end of the year, than the half of it at the end of six months? or than any fractional part of a year's interest which may have accrued at any other period? The true way, in my opinion, is to consider the interest due at any and all periods of time; or in other words, that a sum of money at interest should be constantly augmenting; that is, I would have all sums of money, whether for great or small portions of time, at compound interest, the interest being added in at the end of every *instant*. This would give a unity to all cases of casting interest; no misunderstanding could then possibly arise as to the amount of interest due. I shall show that it is not difficult to compute interest tables on this principle. The per cent. per annum may be so taken as to make the interest due at the end of one year, the same as in the case of simple interest.

I do not suppose that such a method of casting interest will ever come into general use; all I wish is, to show that such a method is practicable, and if adopted would at once settle all disputes in respect to *usury*. Interest would then be a uniformly increasing quantity, not limited to any particular epoch for receiving its increments.

Let us now endeavor to find a formula for the amount of a given sum at compound interest, when compounded at the end of every instant.

Let P=the principal.

r'=the rate per cent. per anuum.

t=the time in years.

a=the amount.

It is obvious that in the next instant dt, the increment of a, would be $ar'dt$; but by the principles of the calculus this increment of interest is represented by da. Therefore we have this condition,

$$da=ar'dt \qquad (1),$$

where a and t are considered as variable, and r' as constant. Integrating (1) and adding the arbitrary constant c, we have

$$\log. a=r't+c \qquad (2).$$

Now, when $t=0$, $a=P$, these values substituted in (2) we find $c=\log. P$. Hence condition (2) becomes

$$\log. \frac{a}{P}=r't \qquad (3),$$

or

$$a=Pe^{r't} \qquad (4),$$

where $e=2.718281828459$, &c.

The usual formula for compound interest is

$$a=P(1+r)^t \qquad (5),$$

where r=rate per cent. per annum.

If we equate the right-hand members of (4) and (5), we have

$$Pe^{r't}=P(1+r)^t \qquad (6).$$

This is readily reduced to

$$e^{r'}=1+r \qquad (7).$$

Taking the logarithm of both members of (7), we have by a slight reduction

$$r'=\frac{\log.(1+r)}{0\cdot4342944819, \&c.} \qquad (8).$$

Equation (8) gives the rate per cent. per annum r', so that the interest being compounded at every instant shall be the same as yearly compound interest at r per cent. per annum.

By giving to r successively the values 0·03; 0·03½; 0·04; 0·04½; 0·05; 0·05½; 0·06; 0·06½; 0·07; we find the following values for r':

When r=	0·03	we have r'=	0·0295587
"	0·03½	"	0·0344013
"	0·04	"	0·0392206
"	0·04½	"	0·0440169
"	0·05	"	0·0487902
"	0·05½	"	0·0535408
"	0·06	"	0·0582690
"	0·06½	"	0·0629748
"	0·07	"	0·0676587

We will now calculate the *instantaneous* compound interest for intervals of time not exceeding one year on $1, at 6·76587 per cent. per annum.

When P=1, equation (4) becomes

$$a=e^{r't} \qquad (9).$$

Putting this into logarithms, we have

$$\log. a=r't \log. e \qquad (10).$$

If for t we write $\frac{d}{365}$, this will become

$$\log. a=\frac{r'd}{365}\log. e \qquad (11).$$

Substituting 0·0676587 for r', and using the well known value of e, equation (11) becomes

$$\log. a=0\cdot00008050353\times d \qquad (12),$$

where d expresses the time in days.

By means of formula (12) I have computed the values in the third column of the following table. The first column gives the time in days; the second is the simple interest of \$1, at 7 per cent. per annum; the fourth column gives the difference between the simple interest and the instantaneous compound interest.

Days.	Amount of \$1 at simple interest, the rate per cent. per annum being 7.	Amount of \$1 at compound interest, the interest being compounded at the end of every instant,—the rate per cent. per annum being 6·76587.	Difference between the simple interest and the compound interest.
1	1·000192	1·000183	0·000009
2	1·000384	1·000371	0·000013
3	1·000575	1·000556	0·000019
4	1·000767	1·000742	0·000025
5	1·000959	1·000927	0·000032
6	1·001151	1·001113	0·000038
7	1·001342	1·001298	0·000044
8	1·001534	1·001484	0·000050
9	1·001726	1·001670	0·000056
10	1·001918	1·001855	0·000063
20	1·003836	1·003714	0·000122
30	1·005753	1·005576	0·000177
40	1·007671	1·007442	0·000229
50	1·009589	1·009311	0·000278
60	1·011507	1·011884	0·000323
70	1·013425	1·013060	0·000365
80	1·015342	1·014940	0·000402
90	1·017260	1·016823	0·000437
100	1·019178	1·018709	0·000469
200	1·038356	1·037769	0·000587
300	1·057534	1·057185	0·000349
365	1·070000	1·070000	0·000000

By the foregoing table we see that the greatest difference, as given in the fourth column, is \$0.000587, which corresponds to 200 days. If we wish to know the exact time when this difference is a maximum, we proceed as follows: The amount at simple interest of \$1 for t number of years at r per cent. per annum is

$$1+tr.$$

The amount at instantaneous compound interest at r' per cent. per annum for \$1, for the same time, is

$$e^{r't}.$$

The difference is therefore expressed by

$$1+tr-e^{r't} \qquad (13).$$

In this expression t is the only variable.

Putting the differential of (13) equal to zero, we have

$$r-r'e^{r't}=0.$$

Taking the logarithms and reducing, we find

$$t=\frac{\log. r-\log. r'}{r'\log. e} \qquad (14).$$

By using the foregoing values of r, r', and e, we find

$$t=0{\cdot}5025,$$

which is a little more than half a year. Substituting this value of t in the expression (13), it becomes \$0·000592. Hence the greatest difference which can possibly occur is when the given sum has been on interest a little more than half a year; this difference is then less than six tenths of a mill on one dollar.

If this interest is found to be too great, we may take the rate per cent. per annum as much smaller as we wish; all that I have wished to show is, that this method of instantaneous compound interest is practicable.

Before closing this subject, it may not be amiss to remark, that the values of the third column of our table could have been calculated by formula (5), by calling $r=0{\cdot}07$. This is obvious, since we determined $r'=0{\cdot}0676587$ from the condition that equations (4) and (5) should be identical.

Utica, January 27th, 1844.

Art. IV.—*Catalogue of the Fishes of Connecticut, arranged according to their natural families;* prepared for the Yale Natural History Society, by Rev. James H. Linsley, A. M.

To the Secretary of the Yale Natural History Society:

My dear Sir—In continuation of the plan of furnishing a regular series of catalogues of the zoology of Connecticut, I herewith, as next in order, transmit a list of the *fishes*, or fourth class, which I hope may prove acceptable to the Society.

The following catalogue embraces the names of all the species I have yet ascertained to be found in our waters, together with the names of a very few whose residence therein can scarcely be doubted, from the circumstance of their having been obtained in adjoining States.

I would improve this opportunity to tender my thanks to those gentlemen and friends, who have kindly furnished me specimens of rare fishes. I am under very special obligation to William O. Ayres, Esq. of East Hartford (now of New Rochelle, N. Y.) and to James H. Trumbull, Esq. of Stonington, for many interesting communications respecting the fishes found in their vicinities: particularly for the habitat of those registered *Hartford* and *Stonington*, am I indebted to the kind attention of these two gentlemen.

The fishes mentioned in the notes, as forwarded by myself to Dr. Storer, were for the acceptance of the Boston Society of Natural History, and thus transmitted through his hands. They were often accompanied with queries, for the solution of which I take this opportunity to return my thanks to Dr. Storer, as well as for a copy of his valuable "Report on the Fishes and Reptiles of Massachusetts;" to Dr. Dekay also, for the proof plates of the well executed figures of "the Fishes and Reptiles of New York," the family arrangement of which I have usually adopted, as on the whole the best I have seen.

The labor and study requisite to obtain the materials for a catalogue of this description, are much greater than can be imagined by any except a practical naturalist. But the occasional capture of some rare and interesting species, will atone for many a disappointment, as well as much toil and travel.

The declaration of no individual has been received as authority, to enter the name of a species in the following list, (which I have not examined,) unless I had the most implicit confidence in the integrity and veracity of the informer, and strong circumstantial evidence also appeared to corroborate the fact.

It is a source of no small pleasure, that the exquisitely beautiful *hair-finned Dory*, no. 51, has been seen, and for the second known instance in the world, *living* in the waters of this town. Other species of this rare genus, (or allied to it,) have occasionally favored us with their highly appreciated visits.

As regards the number of *sharks* and *rays*, said by various authors to be found on the coast of New England and New York, notwithstanding it is less than that ascribed to Great Britain, there is very little doubt but some of each will be found eventually to be mere synonyms of the same species. But time and observation only, can correct these annoying perplexities, which occur more or less in the history of every class of animals.

Other species will doubtless be found in this State, as the attention of naturalists becomes awakened to the subject. The medium through which these animals move, serves greatly to conceal their numbers, as well as their habits, from our view, and thus retard our progress in their science.

My object in this, as in the preceding classes, already furnished the Society, has been to insert the name of no species, even though in adjoining States, unless strong probability indicated its visits to our waters; and never then, unless notice of these circumstances is subjoined, or made in the note appended.

It is herewith respectfully submitted to the Society, by, dear sir, your sincere friend and servant, JAMES H. LINSLEY.

Elmwood Place, Stratford, Dec. 16, 1843.

CLASS IV. FISHES.

SUB-CLASS I. BONY FISHES.

Section 1. PECTINIBRANCHII.

Order I. SPINE-RAYED.

Family *Percidæ.*

*1. Perca flavescens, Mitchill, Yellow Perch, Hartford and Stonington.

*2. Perca granulata, Cuvier, Rough-head Yellow Perch, Canaan.

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*1. All the large streams near Hartford, (writes Mr. Ayres,) afford this species of *yellow perch.*

*2. I obtained many beautiful specimens of the *rough-head perch* by hook and line, in a stream near "Our House," in Canaan, Litchfield County, in August last. It has recently become a question with Dr. Storer, one of our most distinguished ichthyologists, whether this and the preceding species are not identical, or one and the same, although they have long been considered distinct.

The *Perca acuta,* the *gracilis,* and the *serrato-granulata* of Dekay, or as adopted by him, I suppose are justly considered very doubtful species, but I found in the ponds of Salisbury immense quantities of a perch answering to the description of the true *flavescens.* There is a great similarity in the markings or number of bars of this, and the *granulata.* The fin rays are the same in number, except the anal fin of this contains two more rays. The head is much more acute, and the fish more slender, and the back at the first dorsal invariably more arched, and the colors in all the great numbers I saw, were much lighter than any individual taken in Canaan, which is the true *granulata.* Dr. Dekay's figure 1, of his late Report on the Fishes of New York, which represents the *flavescens,* affords a good representation of this fish, except his figure is throughout too highly colored. I have sent specimens of both to Dr. Storer, and though he is at present undecided as regards their identity, he thinks this Mitchill's "yellow perch." As their localities are only four to five miles asunder, it appears to me their shape (and the much lighter coloring of this) being always so perfectly dissimilar, they can by no means be the same fish, but at least distinct and constant varieties, if not different species. Fu-

*3. Labrax lineatus, Cuvier, Striped Bass, common.

*4. Labrax mucronatus, Cuv., White Perch, common in Stratford.

*5. Centropristis nigricans, Cuv., Black Bass, Black Perch, Housatonic.

*6. Pomotis vulgaris, Cuv., Pond Perch, Bream, Sunfish, common.

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ture observation, however, may possibly decide that we have but one species of perch in New England or New York. This is already the opinion of Mr. Ayres, who has given this subject much attention.

*3. It is matter of some surprise that the *striped bass* ascends the Connecticut river above Hartford. But the *quantity* taken at Stonington is still more extraordinary. Mr. Trumbull wrote under date of January 5, 1842, that "two of our citizens during the latter part of summer and fall, took with hooks four thousand pounds in one day. Some years since a schole of bass were surrounded with seines, and a sufficient number caught to load *thirteen sloops and smacks*. About fifty thousand pounds were taken in half a day a few weeks since in the Paugatuck river, a mile from its mouth. They are usually shipped to New York market."

*4. The white perch are caught here in great numbers both by hooks and seines, and are esteemed among our best fishes for the table. They are at times taken twelve inches long by four inches broad.

*5. This is the *C. nigricans* of Jardine, of which he gives a splendid figure. One of this species was caught in the Housatonic, May 24, 1842, in the shad seine of "the Juniper Fishing Company," and sent to me as unknown, and the first ever seen in this region. The length was 19½ inches, depth 5 inches, thickness 3 inches, weight 2¾ pounds, eye ¾ of an inch in diameter and movable; said to have lived two hours on the shore and to look distinctly in the eye of every person who came near him. The *roes* were at the time about half matured and quite large for their age. I think it superior to any fish for the table that we have in our waters.

*6. The *bream* or Roach (the more popular name in Connecticut) when first taken from the water and living, is one of the most beautiful fishes we have, but at death its colors are entirely changed and all its beauty vanishes. The *appendix* with its long black strap at the termination of the operculum, is nearly as common in our waters as the *vulgaris*. One fact connected with the Roach or Sun-fish, which I have not seen noticed is, it is not very uncommon in still water to find one of these fish to occupy a hole bowl-shaped, about two feet across, dug out in clear white sand near the shore. This he guards with constant care; so much so, that if any fish approaches near it, he flies out from his hole and gives furious chase for twenty feet or more, and then returns and places himself again in the lowest centre of his large bowl, until another unsuspecting visitor approaches and is driven off again in like manner. This appears to occupy his whole attention, and the evident ferocity with which he attacks all intruders is absolutely surprising. I inquired of an intelligent trout-catcher an explanation, and was told it was the female preparing to spawn. But I found on the contrary in taking a number so situated, with a hook, it was invariably a male fish. The object of his strange conduct I have hitherto been unable to learn, and insert the facts here in order to elicit some intelligence on the point. It will doubtless be found to have some connection with the increase of the species.

7. Pomotis appendix, Mitchill, Black-eared Pond Perch, Hartford.

*8. Pomotis rubricauda, Storer, Red-tailed Pond Perch, Bridgeport and Hartford.

*9. Etheostoma Olmstedi, Storer, Ground-fish, common.

Family *Triglidæ.*

*10. Dactylopterus volitans, Cuv., Sea Swallow, Long Island Sound.

11. Prionotus strigatus, Cuv., Sea Robin, Grunter, Stratford and Stonington.

12. Prionotus Carolinus, Cuv., Web-fingered Grunter, Stratford and Stonington.

*13. Trigla cuculus, Linn., Red Gurnard, East Haven.

*14. Cottus Mitchilli, Dekay, Smooth Brown Bullhead, Norwalk.

15. Cottus Virginianus, Will., Common Sculpin, Stonington.

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*8. The *red-tailed pond perch*, first discovered and described by Dr. Storer, may perhaps eventually prove to be only a beautiful variety of the *P. appendix*, although at present Dr. S. I believe is not decided.

*9. The *E. Olmstedi*, I have found in nearly all our fresh-water streams. It is identical with Dr. Dekay's *Boleosoma tessellatum.* I think its motions altogether too slow to be called a "*darter*," but as it always appears to lie upon the ground except when aroused by fear, I prefer the name of *ground-fish.* All this genus, owing to the peculiar shape of the head, are denominated hog-fish by Dr. Kirtland in his report of the Zoology of Ohio. The fin rays of this species vary much in their number. Dr. Storer's specimens, D. 9—13, P. 15, V. 6, A. 11, C. 15. My largest specimen has, D. 8—15, P. 13, V. 6, A. 10, C. 16.

P. S. I took in September last, in Wolcott, Wayne county, N. Y., about 3 miles south of Lake Ontario, several individuals of the genus *Etheostoma*, which Dr. Storer, to whom I sent three specimens, believes with myself, to be an undescribed species.

*10. An intelligent fisherman of this town (Mr. Mitchell,) who has long made it his constant occupation, assures me that he has taken one of this *sea swallow* in Long Island Sound. It is so unlike any thing else that swims, there can be no mistake. Dr. Dekay's report says it ranges from Brazil to Newfoundland.

*13. The red gurnard was taken in a seine last spring at East Haven, by Mr. Mitchell, as he informed me, of which Dr. Dekay's figure he thinks a good one. Cuvier states that he received this species from New York.

*14. The two preceding species of *Prionotus* are very common on our shores, and are often taken by seine, of which I have had several. The *smooth brown bullhead*, about four inches in length, it is believed, I caught and preserved, while fishing near Norwalk Islands for blackfish some years since. Dr. Dekay believes it not the young of any other species, but distinct on account of the spines of the preopercle and the radial formula.

*16. Cottus Grœnlandicus, Cuv., Greenland Sculpin, Long Island Sound.

*17. Cottus æneus, Mitchill, Brown Bullhead, Bridgeport.

*18. Cottus cognatus, Richardson, (*viscosus* of Haldeman,) Star-gazer, Manchester.

*19. Cottus variabilis, Ayres, Various-colored Bullhead, Long Island Sound.

*20. Aspidophoroides monopterygius, Cuv., Bullhead, Masssachusetts.

*21. Hemitripterus Americanus, Gmelin, Sea Raven, Stonington.

*22. Scorpena bufo, Richardson, Sea Scorpion, Stratford.

23. Sebastes Norvegicus, Cuv., Norway Haddock, Massachusetts and New York.

*24. Cryptacanthodes maculatus, Storer, Spotted Wry-mouth, Bridgeport.

*25. Gasterosteus biaculeatus, Mitchill, Two-spined Stickleback, Stratford.

26. Gasterosteus Noveboracensis, Cuv., New York Stickleback, Stratford.

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*16. The *Greenland sculpin* has been taken in Long Island Sound, and Dr. Storer states that it is common along the coast of Massachusetts.

*17. This *brown bullhead* I took at Bridgeport harbor a short time since, in company with flat fish, (*Platessa plana.*)

*18. The *C. cognatus* of Richardson, which is the *Uranidea quiescens, star-gazer* of Dekay, has been taken by Mr. Ayres, about three miles from Hartford, and he observes it is found only in the earliest spring water, even so intensely cold as to be painful to the hand immersed in it. They are usually about four inches in length, and lie on the bottom and sometimes under cover of the stones.

*19. This is a new species discovered and described by Mr. Ayres, and taken in this State.

*20. The *Aspidophorus* of Cuvier is doubtless a very rare fish, but it has been repeatedly obtained in Massachusetts, from the stomachs of haddock, as indicated by Dr. Storer. As the latter fish is not uncommon in our Sound, it may at least be *conjectured* this may also be found there. It is so extremely small it would scarcely be noticed except by a naturalist.

*21. The *sea raven* was taken at Stonington, in June, 1842. Length 15 inches, circumference back of ventrals 9½ inches.

*22. Two specimens of the *sea scorpion* were caught the season past by Mr. P. Wilmot, in a tide mill-pond, between Stratford and Bridgeport.

*24. The *spotted wry-mouth* was taken in Bridgeport, by Charles Norman, in April, 1842, and presented by me to the Boston Society of Natural History through the hand of Dr. Storer. Length 13 inches.

*25. This little *two-spined* fish I have often found here, but very much doubt its being a distinct species; consider it more probably a mere variety of the New York stickle-back, which is also common here.

*27. Gasterosteus quadratus, Mitchill, Four-spined Stickle-back, Stratford.

28. Gasterosteus millepunctatus, Ayres, Many-spotted Stickle-back, Hartford.

29. Gasterosteus apeltes, Cuv., Bloody Stickle-back, Stonington.

*30. Gasterosteus pungitius, Linn., Many-spined Stickle-back, Housatonic.

Family *Scienidæ.*

*31. Leiostomus obliquus, Mitchill, Little Porgee, Branford.

*32. Otolithus regalis, Cuv., Weak Fish, Yellow-fin, common.

*33. Umbrina nebulosa, Mitchill, King Fish, Stratford.

*34. Pogonias chromis, Cuv., Big Drum-Fish, Stratford.

Family *Sparidæ.*

*35. Sargus ovis, Mitchill, Sheepshead, Stratford.

36. Pagrus argyrops, Linn., Big Porgee, Bridgeport and New Haven.

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*27. The *four-spined stickle-back* is not uncommon here, a few specimens of which I have sent to Dr. Storer.

*30. The *ten* or *many-spined stickle-back* I caught in a shrimp net in the Housatonic, about 10 miles from its mouth, July 20, 1843. The dorsal rays were 8—8. Two might have been lost by accident; none, however, appeared to be missing from the fish, and of course its rays are not always fixed to 10, either spinous or flexuous. Dr. Dekay's figure of the *G. occidentalis,* is a good representation of my fish, and perhaps may be, as he conjectures, distinct from the *pungitius.*

*31. The *little porgee* I saw taken in very great numbers from the Sound, in Branford, many years since, in a seine drawn for white fish, (*A. mehaden.*)

*32. The *weak-fish* are very numerous at the mouth of the Housatonic, in the absence of blue fish, (*Temnodon saltator.*) These two species of fishes do not agree to appear numerously at the same time, or same year; hence it is inferred, that the voracious blue-fish makes war upon the weak-fish, or yellow-fin.

*33. I have had a fine specimen of the *king-fish,* taken here in a seine. If cooked and eaten immediately after caught, it is an excellent fish, but will bear no keeping.

*34. The *drum-fish* abounds here. Sixty-two of enormous size were caught here at one haul of a net this season, (1843.) I measured what was considered a small one, that was three feet seven inches long, and one foot deep. They are salted by fishermen, and accounted by them as valuable. Their various colors have established among fishermen what they call three kinds—the *red,* the *black,* and the *beardless;* but I imagine we have but one *species* in Connecticut, although Dr. Mitchill made three.

*35. Many of the *sheepshead* were caught here in the Housatonic, in 1842. They are, however, considered rare and very valuable for the table. It has an extensive geographical range. I have seen them very large and fine in Georgia and Florida. They usually sell at about *seventeen cents per pound,* both here and there.

Family *Chetodontidæ.*

*37. Ephippus faber, Cuv., Banded Ephippus, Stratford, East Haven.

Family *Scombridæ.*

*38. Scomber vernalis, Mitchill, Spring Mackerel, Branford and Stratford.

*39. Scomber colias, Gmelin, Spanish Mackerel, Stratford.

*40. Cybium maculatum, Mitchill, Spotted Mackerel, Long Island Sound.

*41. Thynnus vulgaris, Cuv., Common Tunny, Stonington.

42. Trichiurus lepturus, Linn., Silver Hair-tail, Massachusetts and New York.

*43. Pelamys Sarda, Bloch, Skip-Jack, Stonington.

*44. Elacate Atlantica, Cuv., Northern Crab-eater, New York and Massachusetts.

*45. Xiphias gladius, Linn., Sword Fish, New Haven.

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*37. This beautiful fish, the popular name of which is *three-tailed porgee*, was taken last summer at East Haven, by a Mr. Mitchell, as he informs me. Capt. Porter of this town informs me that he has taken it at the Middle Ground in our Sound. They both readily recognized the figure. This is so unlike any other fish, it must be correct as stated.

*38. The *spring mackerel* is sometimes caught in white-fish seines in our Sound. Some years since many were so taken in Branford. Mr. Ayres has seen them also in the Sound on Long Island shore.

*39. A fine specimen of the *Spanish mackerel* I obtained from a seine drawn here in the Sound, in July, 1842. It was ten inches in length, and truly a beautiful fish.

*40. This is the *Scomber maculatum* of Mitchill, and has been taken in Long Island Sound by Mr. Ayres.

*41. Many of the *tunny* or *horse mackerel* were taken in Stonington, in 1840. It appears there somewhat periodically and with several years intervening. Its length there usually is from twenty-five to thirty inches. New York obtains her principal supply from Block Island, as indicated by Dr. Dekay.

*43. The *skip-jack* was taken at Stonington, in July, 1842.

*44. This voracious fish, (*crab-cater*,) as stated by Dr. Dekay's report, page 144, ranges from 42° N., to the shores of Brazil, as well as the shores of Europe and Africa. One of our fishermen believes he has taken it at the mouth of the Housatonic.

*45. While Yarrell observes that the edges of the sword of this fish "are finely denticulated," and Dr. Storer mentions, "his specimen was unnaturally broken at intervals, evidently by use," I would remark, that I have a very large and fine specimen, the edges of which are very regular and perfectly smooth. The fish could not have been less than fifteen feet in length. I have no hesitation in stating, that the sword of this fish is *naturally* perfectly smooth throughout its whole length, and with sharply carinated edges. I am well informed a *small specimen* was taken and eaten by fishermen at New Haven, 1843.

*46. Palinurus perciformis, Dekay, Black Pilot Fish, Stonington.

*47. Naucrates Noveboracensis, Cuv., New York Pilot Fish, Stonington.

*48. Caranx crysos, Mitchill, Yellow Mackerel, Long Island Sound.

*49. Caranx punctatus, Cuv., Spotted Caranx, East Haven.

*50. Temnodon saltator, Cuv., Blue Fish, very common.

*51. Blepharis crinitus, Ackerly, Hair-finned Dory, Stratford.

*52. Argyreiosus capillaris, Mitchill, Hair-finned Argyreiosus, Greenwich.

*53. Argyreiosus vomer, Lacepede, Rostrated Dory, Stratford.

*54. Vomer setapinnis, Mitchill, (*Brownii*, Cuv. and Dekay,) Bristly Dory, Long Island Sound.

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*46. This is what Dr. Storer in his report denominated *Trachinotus argenteus*, but subsequently preferred the above name of Dr. Dekay's.

*47. Several individuals of the *New York pilot-fish*, which is the *Scomber ductor* of Mitchill, were taken in Stonington, in 1842.

*48. As the *yellow mackerel* is abundant at New York, it can hardly be possible it does not inhabit our Sound, though I am not aware of its having been taken here.

*49. The *spotted caranx*, Mr. Mitchell informs me, he caught last spring at East Haven, in a seine.

*50. The *blue fish* is very remarkable on account of the monstrous parasite, Cymothöa, (a species which is I believe undescribed,) that inhabits its mouth. I have taken many of these parasites; the longest was 1 and 9 tenth inches in length; usually about 1¼ inches long, and from ½ to ¾ inch wide.

*51. This beautiful and most curiously constructed fish was seen for *a half hour*, by several gentlemen in 1842, in Mr. Cook's tide mill-pond, which divides Stratford from Bridgeport, but all means failed to capture it. It was particularly noticed that the *long dorsal* and *anal rays* remained perfectly quiet, while the tail or caudal fin was constantly flapping between them, to give the fish his gentle motion, in his various circular movements, as apparently to evade all attempts to capture him. Dr. Dekay remarks, that the only one ever observed, was obtained from Long Island Sound, (which was discovered by Dr. Ackerly, as published in the Journal of Science, Vol. II, page 144.) Of course this is the second. I am informed by one of the gentlemen who saw it during its whole time of appearance, that Dr. Dekay's figure is a very perfect representation.

*52. The *Argyreiosus* was taken in a gill-net, in 1842, by an old fisherman, at Greenwich, Conn., as he recently informed me, of the correctness of which, I entertain not the least doubt.

*53. The *rostrated dory* was taken in this town a few years since, in a seine set for other fishes, by Mr. A. Curtis. Length of fish about 3 or 4 inches. It was preserved in alcohol that unfortunately was too strong, and it was destroyed.

*54. The *bristly dory* was taken by Mr. Ayres, in Long Island Sound, (L. I.) It is a kind of cosmopolite, and doubtless we have a claim to its habitat on this shore also.

*55. Peprilus triacanthus, Peck, Three-spined Peprilus, Stratford.

*56. Seriola zonata, Mitchill and Dekay, Banded Mackerel, Long Island Sound.

Family *Atherinidæ.*

*57. Atherina notata, Mitchill, Small Silver-side, Stratford.

Family *Mugilidæ.*

*58. Mugil albula, Linn., White Mullet, Stonington.

*59. Mugil lineatus, Mitchill, Striped Mullet, Stratford.

Family *Gobidæ.*

*60. Murenoides guttata, Lacepede, Spotted Gunnel, Bridgeport and Stonington.

*61. Pholis subbifurcatus, Storer, Subbifurcated Pholis, Massachusetts.

*62. Zoarchus anguillaris, Peck, Eel-shaped Blenny, Long Island Sound.

*63. Zoarchus fimbriatus, Cuv. and Dekay, Bordered Eel-pout, Long Island Sound.

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*55. This fish is not uncommon in our Sound. I have had several specimens of it; it is usually called *pumpkin-seed* by our fishermen, from the similarity in shape.

*56. Dr. Dekay says he has taken this fish in Long Island Sound, in company with the big porgee.

*57. This little *silver-side* is quite common here, and is much used as bait for blue fish. I think there is another and much larger species, whose habitat is Fresh Pond, in this town, but for the last three years, I have not been able to obtain a specimen.

*58. An individual of the *white mullet* was taken at Stonington harbor, October 29th, 1842, as Mr. Trumbull informed me. It weighed 2¾ lbs. The length from extremity of caudal fin was 18½ inches. Our fishermen, he adds, had never seen it before. Dr. Mitchill mentions a specimen sent him, which weighed 2½ lbs.; and adds, this is the largest known.

*59. The *striped mullet* has been taken in a seine here in the Sound, by Mr. Mitchell, as he assures me, and during the present season. It is found in New York markets in September.

*60. This is the *Gunellus mucronatus*, *butter-fish* of Dekay. I obtained a fine specimen of this singular fish at Bridgeport, in the summer of 1842, which I forwarded in alcohol to Dr. Dekay.

*61. This fish having been found driven on shore among sea-weed at Nahant, as mentioned by Dr. Storer, it is hoped may be thus obtained in our Sound.

*62. I received a fine specimen of this fish, 28 inches in length, and weighing 2½ lbs., from Mr. Treat, of Bridgeport, which was caught near Block Island, in Rhode Island, and found a good fish for the table.

*63. Of this *eel-pout*, I received some half dozen fine specimens from Mr. Charles Youngs, of Bridgeport, taken with a hook, on what is termed Middle

*64. Anarrhicas lupus, Linn., Wolf-Fish, Long Island Sound?

*65. Gobius alepidotus, Bosc., Variegated Goby, Bridgeport.

Family *Lophidæ.*

*66. Lophius piscatorius, Linn., Angler, Wide Gab, Bridgeport and Stonington.

*67. Cheironectes lævigatus, Cuv., Smooth Mouse-Fish, Long Island Sound.

*68. Cheironectes gibbus, Mitchill and Dekay, Gibbous Mouse-Fish, Long Island Sound.

*69. Batrachus variegatus, Le Sueur, Toad-Fish, common.

Ground, in our Sound. This and the preceding species may, as some naturalists imagine, be the same. But as Dr. Dekay justly remarks, "it is invariably smaller than the other species," and I may add, it is singular that one of this species never reaches half the size of the preceding. Among all that I have seen of the latter, not one has measured over 13 inches in length, and very rarely one is less than 12 inches, while the Blenny is more than double the length, besides the difference in color. The fin rays are so variable in number that nothing can be decided from that source alone.

*64. As the *wolf-fish* is not uncommon at Long Island, and all the shores east of us, it is doubtless a resident of our Sound, but as I have not been so fortunate as to obtain one, I have inserted it with a query?

*65. The *variegated goby* was taken at Bridgeport, by Capt. Roundy, from an oyster. The latter, I believe, had been brought by vessel load from New Jersey. Dr. Dekay mentions its being taken in the harbor of New York, and its radial formula was, D. 6—14, P. 17, V. 12 or 13, A. 11, C. 19. The rays of my specimen were, D. 6—12, P. 13, V. both fins serrated, A. 10, C. 16. I sent my fish to Dr. Storer, for the B. S. N. History, and he decided it to be this species, though differing in several respects from Dr. Dekay's figure and description. It is the *G. vinidipollidus* of Mitchill.

*66. Of this monstrously deformed fish, I obtained a large specimen from Bridgeport. Length 3 feet 8 inches, width front of pectorals 22 inches, from ends of pectorals 37 inches. Although it continued alive out of water about 24 hours, I took from its stomach subsequently, a large half pail full of fishes, of various species, such as *tom-cods*, cunners, bass fry, &c.; of the latter, some were as perfect as when swallowed, notwithstanding the lapse of time mentioned. One taken at Stonington, writes Mr. Trumbull, was 45 inches in length.

*67. This *mouse-fish*, Mr. Nettleton, of Orange, informs me he has taken in this county. Its range is said by Dr. Dekay, to be from Massachusetts to Charleston.

*68. A dried specimen of this fish was brought to Bridgeport for me, (found at Montauk Point by a fisherman,) but by accident in passing through too many hands it was lost.

*69. The eggs of the voracious *toad-fish* are very unusually large for a fish so comparatively small. It is not, as is generally supposed by naturalists, confined to still water, shallow and muddy. I have seen a very large one caught with a hook, on *rocky* bottom in the middle of our Sound, at what is called the Middle Ground.

*70. Malthæa nasuta, Cuv., Short-nosed Malthea, L. I. Sound?

Family *Labridæ.*

*71. Labrus Americanus, Bloch, Black Fish, common.

*72. Crenilabrus ceruleus, Mitchill, Conner, Burgall, common.

Soft-Rayed Fishes.

Order II. Abdominal.

Family *Siluridæ.*

*73. Galeichthys marinus, Mitchill, Oceanic Cat-Fish, Long Island Sound?

*74. Pimelodus nebulosus, Le Sueur, Horned Pout, Bullhead, common.

Family *Cyprinidæ.*

75. Cyprinus auratus, Linn., Golden Carp, introduced.

76. Cyprinus carpio, Linn., Common Carp, introduced.

*77. Lebias ovinus, Mitchill, Sheepshead Killfish, L. I. Sound.

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*70. On the authority of Dr. Dekay, this *malthæa* has a wide geographical range, from the Caribbean Sea to the coast of Labrador.

*71. The greatest or most distinguished locality for the capture of the common black-fish, on our whole Atlantic coast, is at the Middle Ground already mentioned. It is south of Stratford and Bridgeport, and about the middle of Long Island Sound. The largest of this species are caught by hand, while on the top of the water, and apparently incapable of descending. Mr. Trumbull mentions several being thus taken at Stonington, some of which weighed 16 lbs., 18 lbs., and 22 lbs. Fishermen imagine they belong to very deep water, and that the bladder is so large by coming into warmer water, that when they by accident are brought to the top, they have no power to sink, and are thus taken by hand. It is indeed extraordinary that none but the very largest of the species are found floating, and thus incapable of descending. It affords an interesting query for naturalists, what really constitutes this incapacity for descent or escape from being taken by the hand, on the surface of their native element, when apparently in perfect health and vigor.

*72. There is a great variety of coloring in the *burgall*, and this has doubtless induced Dr. Dekay to constitute his new species, "*uninotatus;*" specimens of which may always be taken in any large collection of the *true species*, and of which we doubtless have but one, as indicated by Dr. Storer.

*73. The *cat-fish* is inserted here on the authority of Dr. Dekay, who states that it has been found from 23 to 41 deg., N., and is frequently abundant in New York harbor, and is, therefore, probably in the Sound.

*74. I have taken this fish in many parts of the State, and as far north as Salisbury lakes, and usually from 8 to 12 inches in length. The popular name in Connecticut is the *bull-head.* The largest I have seen were from Thatcherville Factory-pond, near Bridgeport, over 12 inches long.

*77. Mr. Ayres found this fish abundant on the shore opposite Stratford, at Old Man's Harbor. He believes, however, the *ovinus* of Mitchill to be only the young of *ellipsoides* of Le Sueur, and of course both names include but one species.

*78. Catostomus Bostoniensis, Le Sueur, Common Sucker, passim.

*79. Catostomus tuberculatus, Le Sueur, Horned Sucker, Housatonic.

*80. Abramis crysoleucus, Mitchill, New York Shiner, common.

*81. Leuciscus atronusus, Mitchill, Black-nosed Shiner, common.

*82. Leuciscus cornutus, Mitchill, Red Fin, Horned Dace, common.

*83. Leuciscus pulchellus, Storer, Beautiful Shiner, common.

*84. Leuciscus nasutus, Ayres, Broad-nosed Shiner, Hartford and Northford.

*85. Leuciscus atromaculatus, Mitchill, Black Spotted Dace, Stratford and Canaan.

86. Hydrargira nigrofasciata, Le Sueur, Banded Minnow, common.

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*78, 79. From observation and experience, I am fully satisfied that these two species embrace *all the suckers*, as yet discovered in Connecticut, or in New England. Cart loads of the former are taken here by nets in the Spring, and are probably found in every considerable stream in the State. The *horned sucker* is much more rare. It is the *Cyprinus oblongus* of Mitchill, and this name has the claim of priority, but I have preferred Le Sueur's name as much more appropriate. I forwarded a specimen to Dr. Storer of this species, which I took in the Housatonic, near Derby.

That Dr. Dekay's *Labeo elegans*, *oblongus*, *gibbosus*, *Catostomus nigricans*, *C. communis*, *C. pollidus*, and *C. tuberculatus*, are embraced under the two names of *C. Bostoniensis* and *C. tuberculatus*, I entertain no doubt; because I have seen those answering well to all his figures and descriptions above named, and which also, it is believed, belong only to the two. The colors, &c., thus varying according to different localities and peculiar state of the fishes.

*80. The *New York shiner*, of which I have taken many, I have never found except in still water, and the largest are found in our largest ponds of fresh water.

*81. The *black-nosed shiner* is found most common in our small running streams, and thus in summer is frequently left to die, by their drying away.

*82. The *red-fin* is one of the most beautiful of our fishes, found in fresh water. I have taken it in several of our large streams and in different counties of the State.

*83. The *pulchellus* and *argenteus* of Dr. Storer are now considered by himself and others, as but one species.

*84. The *broad-nosed shiner* I caught in Northford, in company with the *atronasus*, in the stream which runs at the foot of Letoket Mountain. Mr. Ayres, the original describer, found it at Hartford.

*85. The *black-spotted dace*, which is well figured and described by Dr. Dekay, is very nearly allied to the *L. pulchellus* of Dr. Storer, and may eventually prove to be the same. I have taken it in different parts of the State ; but the largest in-

87. Hydrargira ornata ,Le Sueur, Ornamented Minnow, common.

*88. Hydrargira flavula, Mitchill, New York Gudgeon, Stratford.

*89. Hydrargira pisculenta, Mitchill, Greedy Shiner, Long Island Sound.

Family *Esocidæ.*

*90. Esox reticulatus, Le Sueur, Pickerel, common.

91. Esox fasciatus, Dekay, Little Pike, Samp-Mortar River, Fairfield.

*92. Belone Truncata, Le Sueur, Garfish, Stratford and Hartford.

*93. Scomberesox Storeri, Dekay, Billfish, Long Isl. Sound.

*94. Exocetus Noveboracensis, Mitchill, Flying-fish, New Haven and Stonington.

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dividuals I caught at North Canaan. The distinct black spot at the anterior portion of the base of the dorsal fin, furnishes the name. Dr. Storer, to whom I forwarded specimens of this, and the other five preceding species, informs me that he has received very fine specimens of this from the State of Maine. Those I took at North Canaan were thickly covered with little black blotches, which could not be removed without breaking the skin. Whether caused by parasites or by disease, I was unable to determine.

*88. We have many varieties of the *minnows*, but on comparing them, I consider it doubtful whether we have more than the above named four species; the first three of which are common.

*89. The *H. pisculenta* has been taken by Mr. Ayres in L. I. Sound.

*90. A *pickerel* was taken at Hartford about a year since, (Mr. Ayres informs me,) that measured 38 inches in length, and weighed 14 pounds. This species is unquestionably the best edible fish we have in the State, the black perch (*C. nigricans*) excepted.

*92. The *gar-fish*, says Mr. Ayres, " has been obtained 60 miles up the Connecticut river. Mr. Olmsted caught it even above Hartford." It is quite common in our tide waters. My largest specimen measured 23 inches in length.

*93. The *bill-fish* I have not obtained, but am informed by our fishermen, that it is not uncommon, and is often taken in seines.

*94. An intelligent fisherman and of strict veracity, informs me, that he last spring took the New York *flying-fish* on the east side of New Haven harbor. He has not unfrequently taken this fish south of Long Island—has often seen it at sea to fly when pursued by other fishes. But the one at New Haven was the only one he had ever seen in the Sound. His impression is, that this one arose from the water and fell into his boat. *He is sure of seeing it fly a moment*, but whether pursued or not he cannot remember. He believes it 10 or 12 inches long. On my presenting figures of different species without names, he immediately pointed to this as the only one he had ever seen. Dr. Dekay mentions the same species as taken in a seine at New York, and presented to Dr. Mitchill. Dr. Storer also (page 200 of Report) mentions it as found in Massachusetts. Mr. Trumbull writes, it has been seen at Stonington.

Family *Fistularidæ.*

*95. Fistularia serrata, Bloch, Tobacco Pipe-fish, Massachusetts.

Family *Salmonidæ.*

96. Salmo fontinalis, Mitchill, Brook Trout, common.

*97. Salmo erythrogaster, Dekay, Red-bellied Trout, Housatonic.

*98. Salmo Salar, Linn., Salmon, Connecticut River.

99. Osmerus eperlanus, Artedi, Smelt, Stratford.

*100. Scopelus Humboldti? the Argentine, Massachusetts.

Family *Clupidæ.*

*101. Clupea elongata, Le Sueur, Common Herring, Stratford.

*102. Clupea fasciata, Le Sueur, Fasciated Herring, New Haven.

*103. Clupea minima, Peck, the Brit, Massachusetts.

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*95. The *tobacco-pipe-fish* is found from Brazil to Massachusetts; and is probably at times in our Sound, although I have not known of one being so obtained.

*97. It has long been known as an adage, "*as red as a Housatonic trout.*" The largest I have seen here weighed *about 2 pounds.* I then (1840) knew of but one species. Mr. Nettleton, of Orange, one of the greatest and most expert trout catchers in New England, assures me he has often taken this trout, or answering exactly to Dr. Dekay's figure and description of this species, Plate 39, fig. 2, of his report on the New York Fauna.

*98. The delicious *salmon,* once very abundant in the Connecticut river, is now nearly or quite extinct, except occasionally in the course of years one is taken in a shoal seine, near the mouth of the river. Dr. Barratt has published a very interesting paper suggesting some means for restoring this excellent fish again to this river. His plan is predicated upon the principle that this fish returns annually to the same river in which it was spawned. His plan is worthy an attempt, although it will require a different construction of all the mill-dams to afford the fish the capacity to ascend over them in its annual return.

*100. The *argentine* is undoubtedly very rare, though it has been found at Nahant, in Massachusetts, in the stomach of a haddock, and may probably be found in Connecticut.

*101. I obtained the *common herring* picked up fresh on our shore, Nov. 3, 1842, and on the same day took a perfect specimen from the stomach of the great heron, (*Ardea herodias*, Linn.,) the only individuals I have seen here.

*102. The *fasciated herring* I found in a seine in New Haven harbor, drawn for white-fish, and found it not very uncommon. The difference between these two species will easily be seen by comparing them together.

*103. The *brit* I have seen by such *myriads* pouring into the bay of Massachusetts, as would serve to render it almost impossible but some of the little fellows should reach our Sound; although scholes of the voracious *blue-fish* were devouring them by thousands and probably millions.

*104. Clupea virescens, Dekay, Green Back, Stratford.
105. Alosa vernalis, Mitchill, Alewife, common.
*106. Alosa sapidissima, Wilson, Shad, common.
*107. Alosa menhaden, Mitchill, Whitefish, common.
*108. Alosa mattowacca, Mitchill, Autumnal Herring, Housatonic.
† ——— ——— Stratford.

Order III. Jugular.

Family *Gadidæ.*

109. Morrhua Americana, Storer, Codfish, Stonington.
110. Morrhua æglefinus, Linn., Haddock, Stonington.

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*104. This fish is taken here by hundreds every autumn in seines, and usually sold at a cent apiece, under the popular name of *green-backs.* They were for a length of time a perplexity to me on account of their near alliance to the *alewife.* I was induced finally to consider them the young of that species. But on eating them I found them much less bony, and usually about half the size of that fish. Dr. Dekay's name, figure and description removed my doubts, and were very satisfactory. His plate 13 exhibits a good figure of each species.

*106. Although our *common shad* has had several specific names, as *vulgaris, indigena, alosa,* and lastly, Dr. Dekay has instituted the name of *præstabilis,* it is believed the above name, *sapidissima,* has the priority, as first given to this valuable fish. (See *Clupea sapidissima, in Rees' Encyclopœdia.*) Dr. Dekay remarks, that he is "not able to state whether the shad is found farther north than the coast of New Hampshire;" to which I would subjoin, that the largest shad I have ever seen were taken in Maine. I saw one selected from several hundreds of very large size, which weighed 10 pounds. This was on the Kennebec river, in 1812. They were then selling for four cents each, and dull at that. They extend abundantly through our whole eastern coast, as I have subsequently witnessed.

*107. The *menhaden* or *white-fish,* are caught in most *incredible numbers,* nearly the whole length of our Sound, and are sold for manure at seventy-five cents to one dollar a thousand. They are, however, vastly more abundant east of this town than west, and for a few years past have been much less abundant than formerly. Poor, or quite ordinary soil, by the application of ten thousand fish to the acre, will, with suitable cultivation, produce the finest crop of wheat.

*108. These fish were caught in considerable numbers in this place last June, at the winding up of the shad fishery. And at East Haven, two hundred were taken at one haul of a seine. They are considered "new comers." The weight was about 2 pounds. The popular name was, "a new species of shad." Dr. Dekay states, "they are called *weiseck* in the Connecticut river."

†*Alosa sardina* of Dekay, *Clupea sardina* of Mitchill, is figured and described by the former as a distinct species. It is caught in great numbers in this town by seines, from June to October; and unless I am greatly mistaken, is nothing more than the *young of our shad,* (*A. sapidissima.*) His radial formula is not more dissimilar than what occurs in almost all fishes of the same species. The want of *caudal pouches* would evidently be supplied by age, as our largest shad always present the fullest. His diversity of length, from 6 to 12 inches, serves to confirm the opinion of its being a young shad.

*111. Morrhua pruinosa, Mitchill, Frostfish, Tomcod, common.

*112. Morrhua minuta, Linn., Poor Cod, Massachusetts.

*113. Merlangus carbonarius, Linn., Coalfish, L. I. Sound.

*114. Merlangus purpureus, Mitchill, Pollock, Stratford.

*115. Merlucius vulgaris, Cuv., Hake, Stonington.

*116. Lota brosmiana, Storer, Eel-Pout, New York and New Hampshire.

*117. Lota compressa, Le Sueur, Compressed Burbot, North Canaan.

*118. Lota maculosa, Le Sueur, Spotted Eel Pout, Canaan.

*119. Brosmius flavescens, Le Sueur, Cusk, Long Isl. Sound?

120. Phycis Americanus, Schneider, American Hake, Stonington.

*121. Phycis punctatus, Mitchill, Spotted Codling, Long Island Sound.

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*111. This is the *M. tomcodus* of Mitchill, and is so diversified in its appearance as caused him to make five species of it. It is taken by hundreds in our streams emptying into the Sound. Smaller streams are often artificially narrowed by stones; at the mouth of the narrowed place a hand net is placed. A horse is then ridden into the stream, some hundreds of rods above, and made to dash violently down the stream to the net, and thus to drive into it several bushels of these fishes, sufficient at once to supply a small village.

*112. Little is known of this fish, except it has been taken at Boston, and Dr. Dekay " thinks he has seen it in New York market."

*113. The *coal-fish* is common in Massachusetts and New York, and is said to be found on both shores of the Atlantic.

*114. In the summer of 1842, young fishes of this species usually about six inches in length, were taken by seines in *scholes,* in great abundance, and were then new to our fishermen.

*115. The *hake,* 22 inches in length, was taken in Stonington, Nov. 25th, 1842. The rays of this species vary exceedingly in their number. These, as Mr. Trumbull informs me, were D. 12—38, P. 13, V. 7, A. 39, C. 28.

*116. This is the *Lota inornata* of Dekay, and has been taken in the Hudson river, and in New Hampshire, and is doubtless in our large rivers, the Connecticut, or Housatonic.

*117. I obtained a fine specimen of this *burbot* by the hand of Frederick Plumb, Esq., which was taken in a stream in Canaan, a few years since. Length, after having been in alcohol, 11¼ inches. Barbel under the chin, ½ an inch.

*118. Mr. F. Plumb, above named, informs me that several of the *spotted burbot* were caught in Canaan a few years since, and were erroneously supposed by the inhabitants to be a *hybrid,* between the *trout* and common *eel.* " The spots were derived from the trout, and the shape more from the eel." It was a stranger there.

*119. The *cusk* is not uncommon in Massachusetts, and as indicated by Dr. Storer, is taken with cod-fish. It is inserted here with a hope it may yet be obtained in the Sound. Dr. Storer now believes this a distinct species from the *vulgaris* of Europe, and considers it the *flavescens* of Lesueur.

*121. I am informed by a sensible fisherman that he has often taken this *codling* in our Sound. Dr. Dekay also says, it occurs from New York to the St. Lawrence.

Family *Planidæ.*

122. Hippoglossus vulgaris, Linn., Halibut, Stonington.

*123. Platessa plana, Mitchill, Flatfish, common.

124. Platessa ferruginea, Storer, Rusty Dab, Massachusetts and New York.

*125. Platessa dentata, Mitchill, Toothed Flatfish, Stratford.

*126. Platessa oblonga, Mitchill, Flounder, Stratford.

*127. Pleuronectes aquosus, Mitch., Watery Flounder, Bridgeport and Stonington.

*128. Achirus mollis, Mitchill, New York Sole, Stratford.

Family *Cyclopteridæ.*

*129. Lumpus vulgaris, Cuv., Lumpfish, Long Island Sound?

Family *Echeneidæ.*

*130. Echeneis remora, Linn., Common Suckfish, Long Island Sound?

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*123. Dr. Storer names this "the *flounder of Massachusetts;*" and Dr. Dekay, "the *New York flat-fish;*" but as it is one of the most common fishes on our coast, and to which we all have equal claim, I think *flat-fish,* the only name in Connecticut, quite sufficient. The radial formula of this fish varies more than that of any other species observed; that is, in the *dorsal* and *anal* fin rays. In Dr. Storer's specimen as shown in his Report, the *dorsal* rays were 65—anal 48. Dr. Dekay's were, dorsal, 67—anal, 46. In three individuals I had taken promiscuously, the rays were as follows: Dorsal, 71—anal, 52. Dorsal, 61—anal, 48. Dorsal, 60—anal, 47.

*125. Mr. G. Landon, of Bridgeport, informs me, that he took an individual of this *toothed flat-fish* in Stratford, which weighed 5 pounds.

*126. The *flounder* is caught here with the flat-fish in considerable numbers by seines. It is instantly distinguished by its *eyes* and *color*, on the opposite side. It is nearly allied to the *dentata*, but is different in its coloring upon the dark side, and rather deeper or wider in proportion to its length.

*127. The *watery flounder* is occasionally taken in seines at Bridgeport, but is not eaten. Fishermen erroneously consider it poisonous.

*128. I have obtained a very beautiful specimen of the New York *sole*, from the seines in Stratford. All the transverse bands on one side are very distinct, and the round spots on the other are equally so. Fishermen also universally reject this fish from fear of its poisoning them. This fish has been taken by Mr. Olmsted, above Hartford.

*129. The *lump-fish* is said to be a native of the northern seas, but has been found as far south as New York, which is supposed to be its southern limit. Dr. Storer indicates that it is not uncommon in Massachusetts, and is frequently found washed upon the beaches after storms.

*130. This fish has been taken from vessels in the harbor of New York, and I have a fine specimen about 6 inches, taken (it was said) from the belly of a shark. It has eighteen plates on the disk.

*131. Echeneis albicauda, Mitchill, Suckfish, L. I. Sound.

Order IV. Apodal.

Family *Anguillidæ.*

*132. Anguilla Bostoniensis, Le Sueur, Common Eel, passim.
*133. Anguilla argentea, Le Sueur, Silver Eel, common.
*134. Anguilla latirostris? Yarrell, Broad-nosed Eel, Hartford.
*135. Conger occidentalis, Dekay, Conger Eel, Stratford.
*136. Ammodytes tobianus, Bloch, Little Sand Eel, Stratford.
*137. Ammodytes lancea, Cuv., Banded Sand-Launce, Long Island Sound.

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*131. This is what Dr. Storer, in his Report, considers the *naucrates* of Cuv., but he has since ascertained it to be the *albicauda.* It is not uncommon on the coast of Long Island, and is not unfrequently taken there in seines. They are sometimes attached to vessels arriving in New England. Mr. Trumbull writes me that one has been recently taken at *Stonington*, attached to another fish taken in a seine, by which it adhered until thrown together into a boat. Length 7 to 8 inches. The generic name is derived from εχω to hold, and ναυς a ship, because they were anciently supposed, by attaching themselves to ships, to retard their progress in sailing.

The *E. quatuordecem laminatus*, Storer, has been taken in Massachusetts; and the true *naucrates* from vessels in the harbor of New York, as well as on the banks of Newfoundland, as indicated by Dr. Dekay, but I have not been able to obtain either of them in Connecticut.

*132. Dr. Dekay has given a much better description of this eel, under the name *A. tenuirostris*, than any I have seen under LeSueur's *Bostoniensis*, and Dr. Dekay's name is also more appropriate, but I have used LeSueur's only on account of its priority.

*133. The *silver eel* is, I believe, much less in size than the *Bostoniensis*, but I knew one, I supposed to be of the former species, caught in a small stream in Northford, that weighed 7 lbs. 2 oz. It was killed by a lad with a large stick because the water of the stream was too shallow for him to escape. It was about 10 miles from salt water, and above such barriers, as is believed he could not have ascended. The size of this eel, militates against the idea of Dr. Storer's and Dr. Dekay's *oceanic eel*, to which Dr. Mitchill, followed by Dr. Dekay, gave the name of *Anguilla oceanica*,—weight 9 lbs.

*134. This eel, Mr. Ayres informs me, is common at Hartford, and he considers it the *latirostris* of Yarrell, or the *macrocephalus* of LeSueur, or otherwise it is undescribed.

*135. An intelligent fisherman in this town, assures me that he caught the *conger eel* last season at the mouth of the Housatonic. Said it was easily distinguished by its under parts being so perfectly white, and thus unlike other eels.

*136. The *little sand-eel* (which is quite common) lies quiet in the sand on the shore until rising water. They then throw themselves out and are easily discovered by their floundering on the shore, and are thus often picked up as bait for blue-fish. Their dorsal and anal rays are exceedingly various in their numbers.

*137. Dr. Dekay took a species of *Ammodytes* at Sag Harbor, which he considered identical with the *lancea*, but which he has named *A. vittatus.* I have a fine

Order V. Lophobranchii.

Family *Syngnathidæ.*

*138. Syngnathus fuscus, Storer, Brown Pipefish. Stratford.

*139. Syngnathus Peckianus, Storer, Peck's Pipefish.

*140. Hippocampus brevirostris, Cuv., Short-nosed Sea-Horse, Long Island Sound?

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specimen of the *A. lancea*, from the Great Banks, that corresponds in many respects with this species. Mine, however, is 10 inches in length, seven tenths of an inch deep and the rays are Dorsal 60, P. 10, V. 32, C. 12. Dr. Dekay's specimen, D. 54, P. 15, V. 28, C. 19. The under jaw of mine extends three twentieths of an inch beyond the upper. If *his* was the species, it must have been quite young, being but half the length of an adult. Mr. Ayres, however, took the *lancea* in L. I. Sound. Vide page 58, B. S. N. H. proceedings.

*138, 139. Both these species of *pipe-fish* are often found here in oyster beds and are taken by oyster tongs. I have several specimens thus obtained. By placing them in alcohol a short time, they are easily dried by a coat of varnish easily preserved. I have a fine specimen of this *genus* from *Madeira*, by the kindness of Mr. Trumbull, that is much *thicker* and *stouter* than ours, and is probably a different species.

One of the greatest curiosities in this class of animals, is believed to be, (the well known fact,) that the *males* receive the ova of the females into pouches, or false bellies, and hatch them after they are excluded by the females; and indeed the males (it is said) often carry the young after they are hatched. (Vide Dr. Storer's valuable Report on the Fishes and Reptiles of Massachusetts, page 166.)

*140. I have a beautiful male specimen of this wonderful fish, 6 inches long, and 1 and one-tenth inches wide, caught alive at Sandy Hook, and presented to me by Mr. Sandy Layfield, of Bridgeport. It differs in several respects both from Dr. Storer's and Dr. Dekay's. But on comparing it attentively with both descriptions, I am fully satisfied that Dr. Storer's *brevirostris*, and Dr. Dekay's *Hudsonius*, and my own fish, are all the true *brevirostris* of Cuvier and Yarrell. Mine differs by being a *male*, and having a splendid horn 2 tenths of an inch in length, in the centre of the forehead, and 3 tenths of an inch back of the eyes. The body of mine is divided into 16 segments; Dr. Storer's 11, and Dr. Dekay's 12, both of which were females. Rays of mine—D. 18, P. 14, A. 0. Dr. Storer's—D. 20, P. 14, A. 3 or 4. Dr. Dekay's—D. 18, P. 15, A. 3.

Dr. Dekay supposes his fish to have a shorter tubular jaw than that of Dr. Storer's, and assigns this as a reason for making it a new species. I imagine this was an oversight of Dr. Dekay's, because in all our three animals, the jaws are about 3 tenths of an inch in length.

Dr. Dekay, however, notices one fact not mentioned by Dr. S., "on the summit of the head is a large bony protuberance terminating in five distinct points." This is also a very prominent feature in my fish, and from the correspondence about the head in other respects with Dr. Storer's, it is believed this feature was merely omitted or not regarded by Dr. S., and therefore, the *H. Hudsonius*, in my humble opinion, is not a new or distinct species.

Order VI. Plectognathi.

Family *Gymnodontidæ.*

*141. Diodon maculo-striatus, Mitchill, Spot-striped Diodon, Stratford.

*142. Tetraodon turgidus, Mitchill, Swellfish, Puffer, very common.

*143. Tetraodon mathematicus, Mitchill, Lineated Puffer, Long Island Sound.

*144. Orthagoriscus mola, Linnæus, Short-headed Sunfish, Stonington.

Family *Balistidæ.*

*145. Monocanthus aurantiacus, Mitchill, Orange Filefish, Stratford and Stonington.

146. Monocanthus Massachusettensis, Storer, Massachusetts Filefish, Stonington.

*147. Monocanthus broccus, Mitchill, Long-finned Filefish, Stonington.

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*141. I have obtained several specimens of this singular fish in this town, and have seen it also at New Haven. As Dr. Storer had not named it in his valuable Report, I forwarded him a specimen. The *D. fuliginosus* of Dr. Dekay, (plate 55, fig. 181,) is unquestionably the young of the *maculato-striatus*, as I have the adult, 7 inches in length, with "*lanceolate tail*," which he says, constitutes the distinctive mark of his new species, (*fuliginosus.*) Mr. Ayres informs me that he has relinquished *his* specific name of *nigrolineatus*, as probably only a variety of this species of Mitchill.

The *D. verrucosus* and *pilosus* of Dekay, are both probably in our Sound, but as I have not been so fortunate as to obtain them, their insertion is omitted here.

*142. The *swell-fish, puffer*, &c., has throughout our State, (I believe,) improperly the popular name of *toad-fish*, which ought to be applied only to the *Batrachus*.

*143. Upon the authority of Dr. Dekay, the *lineated puffer* is found from Rhode Island to Mexico. Dr. Storer has recently obtained it in Massachusetts, as stated in the Boston Jour. Nat. Hist., Vol. IV, page 183. The specific name of *lævigatus*, first proposed by Linnæus, and adopted by several other naturalists, appears in most respects preferable to *mathematicus*. But as there are *spines* on the abdomen the latter name is considered more appropriate.

*144. A large individual of the strangely constructed *sun-fish*, called also *short-head-fish*, was caught in Stonington, in 1841, as Mr. Trumbull informs me.

*145. The *orange file-fish* I obtained from the Housatonic, in August, 1841—length 17 inches, width 4 inches, and 1 inch thick—horn upon the head 2 inches in length, but was said to have lost one inch by accident before I received it. Two individuals taken at Stonington, same year, length of one 18 inches, and the other 19 inches, horn 4 inches.

*147. The *long-finned file-fish* is nearly allied to the preceding species. Both are common at New York, and also another species named by Dr. Dekay, *M. setifer*.

*148. Aluteres cuspicauda, Mitchill, Long-tailed Filefish, Long Island Sound?

Family *Ostracionidæ.*

*149. Ostracion Yalei, Storer, Yale's Trunkfish, Massachusetts.

Sub-Class II. Cartilaginous Fishes.

Order I. Eleutheropomi.

Family *Sturionidæ.*

*150. Acipenser oxyrhincus, Mitchill, Sharp-nosed Sturgeon, common.

*151. Acipenser ———, ———? Long Island Sound.

Order II. Plagiostomi.

Family *Squalidæ.*

152. Carcharias vulpes, Cuv., Thresher, or Fox Shark, Stonington?

*153. Carcharias obscurus, Le Sueur, Dusky Shark, Stratford.

154. Carcharias griseus, Ayres, Brook Haven, Long Island, opposite Stratford.

155. Lamna punctata, Mitchill, Oyster River and Stratford.

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*148. The *long-tailed file-fish* being found very common at New York, and is also obtained in Massachusetts, can hardly fail of being numbered among the fishes of Long Island Sound, although I have not had the pleasure of thus obtaining it.

*149. The *trunk-fish* obtained by Dr. Yale, at Martha's Vineyard, will probably be found in our waters. I have a fine specimen of the *Ostracion cornutus* of Jardine; said to have been taken in the *Atlantic*, by the sailor of whom I procured it. Length 6½ inches, width 2¼, has two horns over the eyes 4 tenths of an inch long, anal spine 3 tenths. Sailors gave it the name of *sea-rat*, on account of its imagined likeness to the common rat. Whether this is the species noticed by Pennant, on the coast of New England, I have not the means to determine. Jardine gives its *habitat* the south of France. My impression is, the sailor said he caught it when coming on to our coast, but of the fact I am not sure.

*150. This species of *sturgeon* is taken in great numbers by our fishermen, who make it a profitable occupation, to harpoon them and the *drum-fish*, which are not unfrequently found together. At the mouth of the Housatonic they are often taken very large, are soon cut up, and sold under the popular name of "*Albany beef*," as well as sturgeon.

*151. Our fishermen are sure we have two kinds of sturgeon here, and that one has a much sharper nose than the other. Not remembering to have seen it, I cannot determine upon the species. Dr. Dekay mentions the *brevirostris*, as found in the Hudson, and this may be the species alluded to as taken here.

*153. I have seen several specimens of the *dusky shark* and sent *the skin* of a male specimen to Dr. Storer, taken in this town in a seine for white-fish, in Oct., 1841; length 3 feet 11 inches. LeSueur's description of this fish is very imperfect.

*156. Lamna caudata, Dekay, Long-tailed Shark, Rhode Island and New York.

*157. Squalus maximus, Linn., Basking Shark, L. I. Sound.

*158. Mustelus canis, Mitchill, Dogfish, Stratford.

159. Spinax acanthias, Linn., Piked Dogfish, common.

*160. Zygæna malleus, Valenciennes, Hammer-headed Shark, Long Island Sound.

Family *Raiidæ.*

*161. Pastinaca maclura, Le Sueur, Broad-sting Ray, Saybrook and New Haven.

162. Raia ocellata, Mitchill, Ocellated Ray, common.

163. Raia batis, Linn., Skate, common.

*164. Raia diaphanes, Mitchill, Clear-nosed Ray, Stratford.

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*156. The *L. caudata* of Dekay, appears by his figure and description very nearly allied to the *punctata*, and is doubtless found in our waters.

*157. A shark of uncommon size was taken here a few years since, the opened *jaws* of which (after taken from the fish) passed easily over the shoulders of the largest man in town, (Mr. I. Thorp.) I saw the jaws, but not the fish. It was said to measure 15½ feet in length, and I conclude it must certainly have been this species. It is found on the whole coast of New England, as well as farther south.

*158. I obtained two of this species on our shore, taken in a white-fish seine; length over 3 feet each, and agreeing well with Dr. Dekay's description, and figure 209, plate 64. It is the most slender fish in proportion to its length of any of the family, that has fallen under my observation. The mouth and teeth much resemble those of the *Raia diaphanes.*

*160. The *hammer-headed shark* has not only been taken repeatedly in Massachusetts, and seen by sailors from this port, at Martha's Vineyard Sound, but Dr. Dekay has "seen them in Hurl-gate, 4 feet long;" and several have been captured in the harbor of New York. "It ranges from Brazil to Nantucket."

*161. This was erroneously called *Raia maclura* by Le Sueur, is changed to *Pastinica* by Dekay. Mr. G. Landon, of Bridgeport, assures me that "he once saw a *ray* taken near the mouth of the Connecticut river that weighed 900 pounds. In directing his attention to the figures in Dr. Dekay's Report, he thinks it differed from all these, but most resembled the *P. maclura.* Le Sueur mentions this species as found at Newport, R. I., and frequently measuring 15 to 18 feet in length. A fisherman informs me that he caught one of the same last summer at New Haven that weighed 200 pounds. There is indubitable evidence, therefore, that this fish inhabits our waters.

*164. Of the *clear-nosed ray*, I sent a male and female specimen to Dr. Storer, (for the Society.) I selected the male from a great number on the shore in this town, in the summer of 1842. The female was picked up at the lighthouse by Mr. Budington, the keeper, and brought to me alive, Feb. 10th, 1843. The stomach was well filled with *shrimp*, and one *Libinia caniculata*, Say. In the *ovarium* were several *eggs* resembling the unlaid eggs of a pullet before a shell is attached, and about the size of a song sparrow's eggs. One was ½ inch in diameter.

From the different *sizes* and shapes of the *egg-cases*, (popularly denominated *sailor's pocket-books,*) and by some naturalists erroneously considered *sharks' eggs,* *165. Raia Americana, Dekay, Prickly Ray, New London?

*166. Trigon centroura, Mitchill, Whip-tailed Sting Ray, Stratford.

I am induced to believe that different species of rays produce very different eggs; or otherwise, that the egg-cases themselves, after exuded by the mother, are capable to and actually do increase in size and vary in shape, until the young ray is hatched. (e. g.) Of two now before me, both inflated, one measures 1 and one-seventh inches wide, and 2 inches long, and the other 1 inch wide, and 1½ inches long, while the longest horns on the less measure 2 inches long, and the longest on the larger, only 1 inch. Yet the undivided filaments on the larger are twice the length of the divided filaments of the less. Large quantities of a *bissus* issue each side of the less; while nothing like it is found in the larger. The inside of the less case contained a substance like the yolk of an egg, the mere *embryo.* It was much less solid than those I had taken from this fish without any cases attached. There is something to me as yet inexplicable on this subject. Mr. William Plumb, of this town, who is in the habit of taking many "*skates*," (a popular name among fishermen, by which the whole family is embraced,) informs me, that two years since, he struck a female skate with a spear, and while she was dying three young skates were dischargsd at the vent, neither of which could swim. On opening her they found another still alive, and of the same size as the other three discharged. The question now is, how came they there? Or is any species of this animal, *viviparous?* Or do the young return again to the mother after hatching?

*165. Mr. Landon of Bridgeport, already mentioned, informs me, that a species of ray answering exactly to Dr. Dekay's figure of this, (plate 66, fig. 215,) is taken in great abundance at New London, and is there called *puppy-fish.* I notice in the New Edinburgh Encyclopedia, that the popular name *puppy-fish* is applied to the *Squatina vulgaris, (angel-fish.)* A species of Squatina, Dr. Dekay believes, has been found in New York, and has figured and described it in his last Report. But though this latter fish is clearly allied to the ray family, still it is so dissimilar in shape, that if there is no mistake in Mr. Landon, (who by the way is quite discriminating in subjects of zoology,) his fish may prove to be the *Raia batis*, Linn., which is common in the Sound. Hence though found by Dr. Dekay at New York, it is inserted here with some hesitation, and a query ? to be hereafter determined.

*166. Many of this species are taken here. From one about 10 feet long, and 4½ feet wide, I severed the tail near half the length of the fish, which had *three spines.* The longest from the root measured 5½ inches, and nearly ½ an inch wide. The other two were a trifle less. I forwarded this to Dr. Storer, 1843, and was pleased to find my label meet his approbation. As this fish has no fins on the tail, of course it does not belong to the genus *Raia.* As I have not seen the English *Pastinaca*, I am, perhaps, incompetent to decide; but I have yet to learn why this is not the true *Trigon pastinaca*, of Linn., as described also by Cuvier, page 294, Vol. II, (New York edition, by Dr. M'Murtrie, 1831.) If so, it has the priority to Mitchill's *T. centroura.* I have no doubt that my fish is the same species of *Trigon*, noticed by Dr. Storer in the B. J. of N. History, Vol. IV, pages 186 and 187; and from my examination of many individuals of different ages and sizes, I am induced to believe that the *long spines* at the root of the tail increase in number with age, like the number of rattles in the tail of the Crotalus. Some smaller have no spine, some one, others two to three. This is the greatest number I have found, and those are the longest fish. Cuvier, on page 295, (as above,) *note* 3, mentions a ray with *five spines.* "Voy. de Freycin, Zool., 62, f. 3." This greatly

*167. Myliobatis acuta, Ayres, Grindstone Ray, Brook Haven, Long Island.

*168. Rhinoptera quadriloba, Le Sueur, Cow-nosed Ray, New York and Stratford.

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Order III. Cyclostomi.

Family *Petromyzonidæ.*

*169. Petromyzon Americanus, Le Sueur, American Lamprey, Stratford.

170. Petromyzon nigricans, Le Sueur, Bluish Lamprey, Stonington.

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variable number of spines is mentioned here with a view to show what I have not elsewhere seen noticed; the impropriety of designating any species of ray with the specific name of "*bispinous*," or any other numerical prefix; as, in my humble opinion, such designation would probably lead to error in any species of this family yet known. I suggest this, however, with great deference to the opinion of my highly esetemed friend Dr. Storer. Vide B. J. N. History, Vol. IV, page 188, and the Society's Proceedings, page 53.

*167. This species of *Myliobatis* has been taken by Mr. Ayres, at Long Island, opposite Stratford. It is described in the Journal of Proceedings of the B. S. of N. History, page 65. *Length* 3 *feet, width* 2 *feet* 5 *inches.*

*168. Capt. Porter of this place, recently presented me with the *tail* of a Ray, taken here, very uncommonly small for its length, being three-tenths of an inch thick horizontally, five-tenths perpendicularly at the distance of 3 feet from its point. It has neither fin nor spine attached. As regards the specific marks of the fish from whence he took it, he made no observation. Dr. Dekay, however, remarks in his Report, that this is *an exceedingly common species* about New York, and this slender tail, though too long for Dr. Dekay's measured specimen, appears unlike that of any species I have seen. In view of all these considerations, it is inserted here. It is well known by "*its spade-like snout*," and its great tact in crushing our common round *clams* (*V. mercenaria*,) with its rolling-mill-teeth.

† Note. Our old fishermen here in 1840, left at the shop of Wm. Chester, what he and they, (as he informed me,) called a "*thorn-back*," and unlike any *rays* that had been known to be caught here, with a request to have him send it to me. But unfortunately it was subsequently kept too long, and being spoiled was thrown away.

Dr. Storer supposes the *thorn-back* (*Raia clavata* of Don, or *radiata* of Linnæus,) by the reports of fishermen, has been taken in Massachusetts, but I do not learn that it has ever fallen under the eye of a naturalist in New England.

Of the *Torpedo occidentalis*, of Storer, so well described in Vol. xlv, of this Journal, I am not informed that any specimen has ever been taken in America, except on the south side of Cape Cod, Massachusetts, where they have been caught for many years.

*169. I obtained a specimen of *this lamprey* in May last, taken in the Housatonic, in a seine drawn for shad. Length 2 feet. The peculiar mouth of this animal, notwithstanding its variegated color, serves to render it a very disgusting object.

*171. Petromyzon appendix, Dekay, Small Lamprey, Hartford and Stratford.

*172. Ammocœtes bicolor, Le Sueur, Mud Lamprey, Connecticut River.

*173. Ammocœtes ——— ? Ayres, Small Lamprey, East Hartford.

Although the fossil fishes of Connecticut have mostly been described in this Journal, yet it is deemed highly important that a list of such as have been found in this state should be attached to this catalogue as a short place of reference, and as we excel almost every other state in the Union in this department, it seems but just to add them here. We are greatly indebted to Mr. William C. Redfield, and his son, Mr. J. H. Redfield, and yourself, for this list.

1. Palæoniscus fultus, Agassiz, Middletown and Durham.
2. Palæoniscus latus, J. H. Redfield, Middletown and Durham.
3. Palæoniscus macropterus, Wm. C. Redfield, Middletown and Durham.
4. Palæoniscus Agassizii, W. C. Redfield, Westfield, Middlefield and Durham.
5. Palæoniscus ovatus, W. C. Redfield, Westfield, Middlefield and Durham.
6. Catopterus gracilis, J. H. Redfield, same localities.
7. Catopterus anguilliformis, W. C. Redfield, Middletown.
8. Catopterus parvulus, W. C. Redfield, Middletown.

Nos. 1, 2, 4, are found in this Journal, Vol. XLIV, p. 135. Nos. 3, 5, 6, 7, 8, are found in Vol. XLI, pp. 25–28, and so is No. 1. All are found in our new red sandstone.

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*171. I have taken in the Housatonic several of this species of *lamprey* by dredging for shells; one of which I sent to Dr. Storer. He decided it to be this recently described species.

*172. The first and only specimen of this species in New England was obtained by Le Sueur, in the Connecticut river, near Northampton. Dr. Kirtland obtained three in Ohio. Vide his Report on Zoology of Ohio, note 72, page 197.

*173. Mr. W. O. Ayres, then of East Hartford, (now of New Rochelle, N. Y.,) discovered a small lamprey in the former place which he considers new and undescribed. His delay in furnishing a description and specific name, has been occasioned by a desire to obtain fresh specimens, which would enable him absolutely to settle the point, but which last season he was unable to accomplish.

Art. V.—*Action of some of the Alkaline Salts upon the Sulphate of Lead;* by J. Lawrence Smith, M. D.

It has been for some time known, that certain neutral salts possess the property of dissolving to some extent the sulphate of lead, which property belongs neither to the acids or bases constituting these salts. By referring to Berzelius' Chemistry, it will be found that the acetate and nitrate of ammonia are among the number. "1 part of the sulphate was dissolved in 47 parts of a solution of the acetate, of sp. gr. 1·036; and in 172 parts of a solution of the nitrate, of sp. gr. 1·144." In the Annalen der Chem. und Phar. Vol. xxxiv, 235, will be found the following statement under the head of *Reactionen:* "Sulphate of lead is easily dissolved, and in a large quantity, by a solution of neutral tartrate of ammonia. A concentrated solution forms after some time a stiff jelly, like silica." This last is no doubt a double tartrate of lead and ammonia.

I had also observed some time previously, that a solution of the citrate of ammonia, when poured upon the sulphate of lead and allowed to stand, altered the character of the sulphate, and this, with the other fact above stated, led to the examination of what was really the action of these as well as other ammoniacal salts in general, upon the sulphate in question, and it was found that in every case it was decomposed.

Citrate of ammonia.—If a solution of citrate of ammonia be poured upon the sulphate of lead and shaken together, the clear solution will be found to contain the sulphate of lead, as shown by hydrosulphuric acid, and a salt of baryta—(taking care in testing with the baryta to acidulate first with pure nitric acid, to prevent the formation of the citrate of baryta.) If they be allowed to remain several weeks in contact, the solution will be found to contain more lead, the sulphate having undergone decomposition, sulphate of ammonia and a double citrate being the result; as this latter salt is not very soluble, a large portion of it remains in the form of a precipitate. The rapidity of this change is in proportion to the concentration of the solution of the citrate. If instead of performing the experiment in the cold, we boil a tolerably concentrated solution of the citrate with the sulphate of lead, a very large quantity of the latter will be dissolved, and the solution become perfectly transparent; if it be set aside and allowed to cool, in the course of a few hours an abundant white precipitate will be formed, and upon testing the clear solution, sulphuric acid, ammonia, citric acid and oxide of lead will be found present. The precipitate, when washed, affords citrate of lead and ammonia. I was at first inclined to think it simply a citrate of lead, attributing the ammonia present to some of the citrate not washed out; but from its possessing certain characters which do not belong to the simple citrate, I consider it a double citrate of lead and ammonia. It contains not the slightest trace of sulphuric acid. It was not analyzed, from the difficulty of obtaining it perfectly pure, as the water used to wash it decomposes it, and as yet this difficulty has not been surmounted. So then the result of the action of the citrate of ammonia upon the sulphate of lead is, first to dissolve it, and subsequently to decompose it, forming the sulphate of ammonia and citrate of lead and ammonia.

Tartrate of ammonia.—If a solution of this salt be added to the sulphate of lead and shaken with it in the cold, the clear solution will be found to contain both lead and sulphuric acid; and if set aside for a few weeks the precipitate will have changed its character, having assumed a crystalline nature; the solution will no longer contain lead, but the quantity of sulphuric acid present will be found to have increased. The precipitate now consists of tartrate instead of sulphate of lead, which is completely soluble in dilute nitric acid, affording no precipitate with a salt of baryta. If the mixture of the tartrate and sulphate be boiled, this change takes place more rapidly, and in a manner somewhat different from the case of the citrate; the sulphate will not be dissolved in such large quantities, and moreover by continuing to boil the solution after the sulphate has been completely dissolved, the tartrate forms during the ebullition, and is precipitated in little shining crystals. If the ebullition be continued a sufficient length of time, the whole of the lead previously dissolved will combine with the tartaric acid. This is different from what takes place with the citrate, which when boiled upon the lead salt dissolves it, and no length of ebullition will produce a precipitate. The action of the tartrate is first to dissolve the sulphate, decompose it in part, and form a double tartrate of lead and ammonia, which last salt is subsequently decomposed by continued contact with water, or still more rapidly by its solution being boiled.

Acetate of ammonia.—This salt also dissolves to some extent the sulphate of lead, but not so readily as either of the above salts. If the solution be boiled and evaporated to dryness, crystals of sulphate of ammonia are obtained, and an uncrystallizable salt of lead, probably an acetate of lead and ammonia; from the difficulty of separating the sulphate of ammonia from it, it is impossible to pronounce positively whether it is a double salt, or simply an acetate of lead. We see in this reaction the existence of a soluble salt of lead and the sulphate of ammonia simultaneously in the same solution, without a precipitate being formed.

Oxalate of ammonia.—Dissolves but slightly the sulphate of lead, owing no doubt to the impossibility of forming a double salt; but it will nevertheless decompose largely, the sulphate furnishing the oxalate of lead.

Muriate of ammonia, if boiled with the sulphate of lead will decompose it instantaneously, furnishing the chloride of lead and sulphate of ammonia.

The *nitrate of ammonia* does the same, forming nitrate of lead and sulphate of ammonia.

The *carbonate* and *succinate of ammonia* produce similar effects.

The action of most of the corresponding salts of potash and soda was examined, and with very similar results. The fact is, it would appear that those alkaline salts which dissolve the sulphate of lead, decompose it, without reference to the time occupied in the solution, as in the case of the carbonate of ammonia, which decomposes the sulphate at the very instant of its solution; and it is impossible to detect at any one time other than a trace of lead in solution, whereas the quantity of sulphuric acid is constantly increasing.

The explanation is clear: the sulphate of lead is a salt with a strong acid and feeble base; the alkaline salts used, contain feebler acids and stronger bases; they dissolve the sulphate, thus affording an opportunity for the acids and bases to act upon one another, under favorable circumstances, and to follow a natural law in chemistry, the stronger acid combined with the stronger bases, and *vice versa*.

From the above facts, some important hints might be afforded to analytical chemistry, for it will be at once seen that the presence of any of the alkaline salts in a solution from which it might be wished to precipitate lead in the form of a sulphate, would affect the accuracy of the result. What is true of the sulphate of lead, may be found also true for other insoluble salts. Moreover, this shows the importance, in the analysis of mineral waters for instance, of weighing well the relative strength of the various acids and bases therein found, in order to ascertain what salts are present, and not to be contented with evaporating the water to dryness, and considering such salts as remain to be those existing in the water, for many of them may be formed during the evaporation. It is not at all improbable, that before many years the examination of mineral waters will be based as much upon calculation as upon analysis, the former of course being guided by the latter, and by certain laws not yet developed.

Art. VI.—*Observations upon the Dip of the Magnetic Needle;* by Thomas Hobart Perry, Prof. Mathematics, U. S. Navy.

The instrument used in obtaining the following results, was made at the establishment of Messrs. E. & G. W. Blunt, of New York, in the autumn of 1837. The needles are about five and a half inches in length, and the vertical circle is six inches in diameter, and the horizontal seven; each being divided to 20′ of a degree, and the latter reading by a vernier to 20″. The needle and vertical or dip circle are protected by a cylindrical brass case with glass bases, and the whole is arranged very much in the same way as one of Dollond's, with the exception that the axes rest in agate knife-edge Ys; a contrivance which though less eligible because less delicate than planes, has nevertheless the advantage of ensuring to the pivots very nearly the same position after each reversion; and, as at sea the motion of the ship does not allow the needle to be entirely at rest, and on land care is taken to tap the case gently before each reading, and no observation is recorded without the proper reversions of the circles, axes, and magnetism,—it is hoped that the errors resulting from the form of the Ys have, with others, been eliminated.

The needle originally furnished is cylindrical, with a steel axis, and with this a few observations were attempted in 1838, at Norfolk and vicinity, and Madeira and Bahia, but without the means of reversing its poles; and as a decision of the Navy Commissioners, in May, 1838, deprived me in common with other officers of the same class, of the quarters which we had previously occupied, and which Congress have since restored, and thereby exposed my instruments to the hazards of a steerage or midshipman's mess, and consequent damage, I did not deem the previous results comparable with observations made after I had procured a magnet, and therefore rejected them altogether. At Rio Janeiro I procured two additional needles, with brass axes and steel pivots, and adjusted them by the method described in this Journal, Vol. xxxvii, p. 277, and subsequently added the adjusting screws there suggested.

No. 1, on the original needle, was distrusted after a few of the first trials, and latterly has been rejected altogether. Nos. 2 and 3 are flat; No. 3 being broadest in the plane of motion, and No. 2 in that perpendicular to it. The indications of No. 2 are found in practice to accord best with each other, although No. 3 appeared to have been best made; a circumstance probably due to the difficulty of so reversing the poles of a magnet, in the ordinary way, that the resultant of the forces, after each operation, may continue constantly parallel to the plane perpendicular to the vertical, in which it was situated previously. Otherwise a new source of error is introduced, which it is not easy to eliminate, except by multiplying observations, and one which it is to be feared is not always sufficiently regarded. It seems therefore, that in a needle for reversion the mass of magnetic matter should be, as nearly as may be convenient, in a plane passing through its axis and extremities. By this means, though we cannot destroy them entirely, we may reduce within narrow limits the errors resulting from differences in the direction of the planes of the magnetic resultants, such as are likely to be the consequence of a want of homogeneity in the metal, or any diversity or carelessness in the mode of magnetizing.

Montevideo, 2d to 4th March, 1839, mean of series with Nos. 1 and 2, upon the top of Mrs. Jane Martin's house, near the Cathedral and public square or *plaza*, . . . 33° 36′·5

Without the walls, near Mr. Fides Baracca, about one mile northwest of the Cathedral—mean of Nos. 1, 2 and 3.

Nov. 2d, 1839, ...	33° 57′·6
" 9th, "	34° 01′·7
Dec. 7th, "	34° 04′·7
Mean, ...	34° 01′·3

Cape Town, South Africa, station northwest of the Astronomical Observatory, the same as that occupied by Capt. Fitzroy of the Beagle.

April 5th, 1841,	No. 3, ...	53° 18′·3
" "	No. 2, ...	53° 30′·7
Mean, ...		53° 24′·5
April 7th, 1841,	No. 3, ...	53° 43
" "	No. 2, ...	53° 27′·3
Mean, ...		53° 35′·1
Mean resultant, ...		53° 29′·8

Magnetic observatory, Lieut. Wilmot's dip-house.

April 6th,	No. 3, ...	53° 34′·7
" "	No. 2, ...	53° 33′·7
Mean, ...		53° 34′·2

Quallah Battoo, coast of Sumatra, on the south bank of the river, near the mouth of a small creek that runs through the village; Oct. 6th, 1841.

No. 3, ...	9° 11′
No. 2, ...	9° 00
Mean, ...	9° 05′·5

Singapore, magnetic observatory in one of Lieut. Elliot's isolated observing rooms, Dec. 5th, 1841.

No. 3, ...	12° 24′·4
No. 2, ...	12° 09′·5
Mean, ...	12° 16′·9

Macao, China, on the *plaza* or common near the water, northeast side of the town, March 31st, 1842.

No. 3, ...	30° 52′
No. 2, ...	30° 53′·4
Mean, ...	30° 52′·7

Gardens of the Morrison Education Society, west side of Monte Fort.

No. 3, ...	31° 18′·7
No. 2, ...	30° 56′·1
Mean, ...	31° 07′·4

Manilla, top of Messrs. Sturgiss & Co.'s buildings, one of the stations occupied by Capt. Wilkes of the U. S. Exploring Squadron—April 12th, 1842.

No. 3, ...	16° 20′·5
No. 2, ...	16° 21′·1
Mean, ...	16° 20′.8

Canton, China, mean of numerous observations upon the top of different factories with Nos. 2 and 3, from the 14th to the 16th of May, 1842, ... 32° 26′·2

Hongkong, near the Queen's Street Baptist Mission Chapel, Sept. 21st, 1842.

No. 3, ...	30° 58′·8
No. 2, ...	31° 00′·6
Mean, ...	30° 59′·7

Wantung, Canton River, mean of observations with Nos. 2 and 3, in different parts of the island—now occupied by the new fortifications, ... 31° 49′·4

Amoy, China, (island of Koolongsoo,) gardens in the rear of the house occupied by Rev. Mr. Abeel of the American mission.

No. 2, ...	35° 28′
No. 3, ...	35° 13′
Mean, ...	35° 20′·5

Honolulu, Oahu, Dr. Wood's yard, formerly the U. S. consulate, and one of the stations occupied by Capt. Wilkes.

July 19th, No. 2, ...	42° 14′
" 20th, No. 2, ...	41° 51′
No. 3, ...	42° 18′
Mean, ...	42° 08′·1

Hilo, Byron's Bay, Hawaii, Rev. Mr. Cowan's gardens, Aug. 23d, 1843.

No. 2, ...	41° 04′·5
No. 3, ...	41° 23′·2
Mean, ...	41° 13′·8
Preferred, ...	41° 10′

Monterey, California, Mr. Larkins' gardens, Sept. 19th, 1843.

No. 2, ...	61° 56′·9
"	61° 52′·3
No. 3, ...	62° 25′·9
Mean, ...	62° 04′·7
Adopted, allowing the observations with No. 3 on account of irregularities ½ value, . . .	61° 58′·9

Art. VII.—*Astronomical Operations performed at the Imperial Observatory of Pulkova;* translated from the Bibliothéque Universelle, of October, 1843, by James Nooney, Jr., A. M.

The great Russian Observatory of Pulkova, near St. Petersburg, commenced operations about five years since, under the direction of the celebrated astronomer Struve, and important results have already been obtained from the labors of observation and calculation which have been executed there. It is proposed to present here a hasty sketch of those labors which have already been published in the Bulletin of the Imperial Academy of Sciences at St. Petersburgh, and in several numbers of the Astronomische Nachrichten.

The meridian-circle, constructed by the brothers Repsold of Hamburg, which this observatory possesses, is furnished with a telescope of 5·8 inches of aperture, and 83 inches of focal length, with which is ordinarily used a magnifying power of 246. The readings are made by the aid of four microscopes upon each of the two vertical circles of four feet in diameter, with which the instrument is provided, and which are graduated to two minutes. This instrument is to be employed principally in the construction of a catalogue of the fixed stars as far as the seventh magnitude inclusive, comprised between the north pole and the parallel of 15° of southern declination. This catalogue will contain about thirteen thousand stars. The charge of observations with this instrument is committed to Mr. Sabler. From those of 1840, he found the latitude of the observatory to be 59° 46′ 18″·65. The latitude obtained by Mr. Peters at the same time was 59° 46′ 18″·83, by double meridian zenith distances of the pole star, observed at its two transits, with a movable vertical circle of Ertel, whose diameter is 52 inches, and which has a telescope whose aperture is 5·9 inches, and focal length 74 inches. The magnifying power ordinarily used in this instrument is 215. The object-glass and eye-glass in each of these instruments are mutually interchangeable at pleasure.

Beside these, the observatory possesses, 1st, a meridian telescope made by Ertel, whose aperture is 5·8 inches, and focal distance 102 inches, and whose ordinary magnifying power is 292: 2d, an instrument for transits in the prime vertical, made by the Repsolds, whose telescope has an aperture of 6¼ inches, is 91 inches in focal length, and sustains a magnifying power of 263: 3d, a great heliometer of Mertz and Mahler, of Munich, whose aperture is 7½ inches, and focal length 10 feet. Struve has taken upon himself the charge of making observations with the second of these instruments. The first is committed to his assistant, Mr. Peters, and the third to Mr. G. Fuss.

To finish the enumeration of the principal astronomical instruments now in use in the observatory of Pulkova, there remains to be mentioned the great achromatic telescope of Mertz and Mahler, equatorially mounted, whose clear aperture is 14·93 inches, and focal length 21·85 feet. The highest power adapted to a wire-micrometer in this telescope magnifies angularly 1822 times. This instrument has been employed by Mr. Otho Struve, a son of the director of the observatory, in a review of the northern hemisphere, relative to stars of the first seven magnitudes, and to the double stars.

Toward the close of the year 1841, the third section of the northern hemisphere had been explored, and the number of stars determined in this space, as far as the seventh magnitude inclusive, was 5275, of which 1194 were of the first six magnitudes, and 4081 of the seventh magnitude. The magnifying power commonly used was 412, while Struve, the father, had adopted one of 198 in his examination of double stars with the Dorpat telescope, whose quantity of light is only a third of that of the Pulkova instrument. In the part of the heavens already examined at this last observatory, there have been found 551 double stars, of which 349 are comprised in the catalogue of Dorpat, and 202 are new. Of these last, 59 are of the first order; that is, those in which the two stars have an apparent angular distance of less than one second. The catalogue of Dorpat contains but 24 of this order, so that the number of these stars is more than trebled. The increase of numbers in the other orders of double stars is not so great, and refers mostly to cases where the companion of the star is extremely faint, while the principal star is quite brilliant. The revision of the part of the heavens just mentioned was to have been completed in 1842.

We pass now to a consideration of the most important labors of calculation which have been already executed at the Pulkova observatory.

Mr. Peters has made a new determination of the *nutation*, by applying 603 right ascensions of the pole star, resulting from observations made at Dorpat from 1822 to 1838. For this purpose he compared these observed right ascensions with the positions deduced from Bessel's *Tabulæ Regiomontanæ.* He then, from the small numerical differences arising from these comparisons, established equations of condition, of which the three principal unknown quantities are, 1st, the correction for the coefficient of nutation, the nutation being supposed 8″·977, according to Lindenau's determination; 2d, that to be applied to the coefficient of aberration, supposed 20″·255 according to Delambre; 3d, the annual parallax of the pole star.

Treating these equations of condition by the method of least squares, Mr. Peters has deduced from them, for the most probable values of these quantities resulting from the Dorpat observations:

9″·2164 for the constant of nutation.
20″·4255 for that of aberration.
0″·1724 for the parallax of the pole star.

For the determination of the same quantities, Mr. Lundahl applied, by an analogous process, the declinations of the pole star observed at Dorpat during sixteen years with a meridian-circle of Reichenbach, and found for these three numbers respectively: 9″·2361; 20″·5508; and 0″·1473; values which agree satisfactorily with the preceding. Mr. Busch had previously obtained 9″·232 for the constant of nutation, from the observations of zenith distances of stars made by Bradley from 1727 to 1747. Struve adopts the mean of these three determinations 9″·2231, as the definitive value for the present state of astronomy. The probable error of this mean does not exceed 0″·0154.

Brinkley found the constant of nutation to be 9″·25 and Robinson 9″·24. The mean of the two preceding values of the parallax of the pole star is very nearly 0″·16, or a little less than a sixth of a second, which would correspond to a distance of this star from the earth a little more than double of that of 61 Cygni, (as determined by Bessel,) or nearly to thirteen hundred thousand times the mean radius of the earth's orbit. Struve found the parallax of α Lyræ to be 0″·2613, with a probable error of 0″·0254; from which it results that the distance of this star is 771400 times the mean radius of the earth's orbit, a space which would require twelve years for the light to pass through; while it would require more than twenty years for light which moves at the rate of 192500 miles per second to travel from the pole star to the earth.

Respecting the coefficient of *aberration* of the fixed stars, Struve has obtained a new value of it, by a complete discussion of the observations made by himself at Pulkova from 1840 to 1842, with the transit instrument already mentioned, established in the prime vertical. The general result of the observations of seven stars near the zenith, gives for the aberration 20″·4451, with a probable error of only 0″·0111. The extreme values of the means arising from observations of different stars are 20″·3947 and 20″·5036. From the smallness of these differences, Struve concludes that we must admit the same constant of aberration, that is, the same velocity of light in these seven stars.

The velocity of light which results from the preceding value of the aberration by adopting the parallax of the sun obtained by Encke, and the dimensions of the terrestrial spheroid according to Bessel, is 69197 leagues of 25 to the degree, in a second of mean time, with a probable error of 38 leagues. The time which light employs in passing over the mean distance of the sun and earth is 8′ 17″·78, with a probable error of 0″·27.

Mr. Otho Struve has performed a laborious calculation in order to estimate numerically the constant of the *precession of the equinoxes*, taking into account for the first time the proper motion of the centre of gravity of the solar system in space.

Bessel, in order to determine the constant of precession, had compared the catalogue deduced by himself from Bradley's observations for 1755, with that of Piazzi, and had thence concluded for the general annual precession in 1790, the value at present admitted, viz. 50″·22106.

For the determination of the same constant, Mr. O. Struve employed the comparison of four hundred stars observed at Dorpat, from 1822, with a meridian circle of Reichenbach and Ertel, with the same stars obtained by Bradley's observations. This comparison gave him the proper motions of these four hundred stars in right ascension and declination. Now, since the remarkable work and research of Argelander, presented to the Academy of St. Petersburg, in 1837, upon the proper motion of the solar system, have made known this motion and direction beyond question, Mr. O. Struve sought to determine by means of the eight hundred equations of condition arising from the four hundred proper motions which he obtained, both the correction which should result for the supposed precession of Bessel, and the quantity of the solar motion.

Here a difficulty respecting this motion presents itself; for the effect must be in the inverse ratio of the distances of the stars from the sun, and the parallax and distances of the stars are still unknown, with the exception perhaps of a very small number of cases. To obviate this the author had recourse to the apparent brightness of the stars, which, according to optical principles, is modified with their distances. Struve, the father, in the introduction to his catalogue of double stars, established, from investigations based upon the number of stars of different classes scattered in the celestial vault, that by taking for unity the distance of the stars of the first magnitude, that of stars of the second magnitude is 1.71, and so on as far as to the stars of the seventh magnitude, whose distance is 11.34. These values though not exact must, according to him, approach near the truth in nearly all cases; though in particular instances an isolated star may be found at a very different distance from that which its apparent splendor assigns to it.

The value of the annual precession for 1790 to which Mr. O. Struve arrived by the resolution of his eight hundred equations of condition, is 50″·23449, with a probable error of 0″·00771. It is readily seen that this value differs but very little from that obtained by Bessel.

As to the motion of the solar system in space, he found that for a point situated at right angles to the direction of this motion, and placed at the mean distance of the stars of the first magnitude, the annual angular motion was 0″·321, according to the observations of right ascension, and 0″·357, according to those of declination, with a probable error of 0″·036. This agreement, for two values entirely independent of each other, is remarkable. Their mean gives 0″·339, with a probable error of 0″·025.

If we admit with Struve 0″·211 for the hypothetical value which may be assigned to the parallax of a star of the first magnitude, or to the angle which the radius of the earth's orbit subtends from this star in a direction perpendicular to the visual ray, it follows that the annual motion of the centre of gravity of the solar system in the same direction, would be a little more than once and a half this quantity, (1·54,) or about fifty-three millions of leagues of twenty-five to the degree, (147,000,000 of miles.) This is the first value, I believe, which any one has dared to assign to a motion in which we all participate; but in order to obtain a value at all probable, there is required an astonishing degree of perfection in the astronomical determinations.

By comparing the particular motions presented by stars of different classes, with this motion of the solar system, the author finds that the former, at the mean, are 2·4 times greater than that of the sun; whence it follows that this luminary may be ranked among those stars which have a comparatively slow motion in space.

Mr. O. Struve subjected the direction of the solar motion to a new examination, and found that this motion is directed toward a point in the heavens whose position, for the middle of the year 1792, was the following: R. A. 261° 23′, Dec. 37° 36′ N. Argelander had found for these elements respectively: R. A. 257° 50′, and Dec. N. 28° 50′.

The agreement in right ascensions in these two determinations is tolerably satisfactory, the difference being within the limits of probable errors. As to the values of declination which differ considerably, Struve thinks that it might result from small constant errors in the differences of declination deduced from the catalogues of 1755 and 1830. These errors cause the stars which have a smaller proper motion to give a less northerly declination than those whose proper motion is greater.

By combining these two systems of values, we arrive at the following, based upon the examination of seven hundred and ninety-seven different stars, viz. R. A. 259° 9′·4, and Dec. N. 34° 36′·5, with the probable errors of 2° 57′·5, and 3° 24′·5 respectively. The point in question is situated in the constellation Hercules.

We cannot terminate this slight sketch of the labors already performed or in process of execution at the observatory of Pulkova, without expressing our admiration for so remarkable a union which appears to exist there, between the means of observation, the most powerful and precise on the one hand, and the ability and high degree of devotion on the other, which characterize both the director of this establishment and the other astronomers who labor with him. Science will owe a debt of gratitude to the Russian emperor, under whose auspices this magnificent observatory was founded and receives its support.

Art. VIII.—*Abstract of the Proceedings of the Fifth Session of the Association of American Geologists and Naturalists.*

The fifth annual session of this Association was held at Washington, D. C. during the week succeeding the 7th of May, 1844. The sessions were held at first in the Medical College lecture-room, but after the second day were adjourned to the Unitarian Church. There were three sessions each day, of which the evening was considered as more properly devoted to general and popular discussions. The next meeting will be held at New Haven, Connecticut, on the 30th of April, 1845, and the week thereafter; Prof. William B. Rogers being chairman, B. Silliman, Jr. and Dr. J. Lawrence Smith secretaries, and Dr. Douglass Houghton treasurer.

Wednesday, May 8th, 12 M.—The Chairman, *Dr. John Locke* of Cincinnati, called the Association to order, on the motion of Dr. Houghton. *Mr. B. Silliman, Jr.* of New Haven, and *Prof. O. P. Hubbard* of Dartmouth College, were made Secretaries, in place of Dr. D. D. Owen of Indiana, not present.

On taking the chair *Prof. Locke* presented his thanks for the unsolicited honor which had been so unexpectedly conferred upon him; alluded to the completion and suspension of the state surveys, as a cause that would render it much more difficult for the members of the Association to undertake the expense of meeting; that, under these circumstances, there should be every effort made to preserve amongst the members that harmony and cordiality which should ever draw them together. He alluded to the short period allotted to the meeting, and urged essential brevity in all communications; cautioned against empty speculations and useless debates, as inconsistent with the matter-of-fact business of geology; recommended to the Association especially the study of organic remains as of absorbing importance, and suggested several problems for solution by its means. He also pointed to the services which the knowledge here acquired would enable each to render to the community in which he resides, and cautioned the members against allowing the influence of their characters to be improperly used in mining and other speculations. He finally felicitated the geologists upon the disposition now so widely evinced of a revolution, by which the talents of our country are to be turned towards the chastening pursuits of physical research, especially in the department of astronomy, and upon the influence which such a taste must exert upon that science which embodies the elements of all the departments of physical knowledge.

On motion of *Prof. Hitchcock*, it was voted that members of the Association be requested to give to the secretaries the titles of the papers they propose to read, with the number of minutes required for each paper.

On motion of *Prof. W. R. Johnson*, Mr. W. C. Redfield, Prof. Hitchcock, and Prof. Johnson, were appointed by the chairman a committee of business.

Voted to adjourn to 4½ o'clock, P. M.

Afternoon Session.—A paper was read "On the origin of the sedimentary rocks of the United States, and on the causes that have led to their elevation above the level of the sea," by *Wm. W. Mather*, Professor of Natural Sciences in Ohio University.

Part I.—On the causes of the great currents of the ocean, and their influence on the transportation and deposition of the sedimentary rocks.

(A.) *Introduction.—On the sedimentary rocks of the United States.*

General description of the extent, thickness and variety.
Various materials indicate modifications of causes.
Transport could only be effected by the aid of currents.
Causes favorable and unfavorable to marine organism.
These sedimentary rocks composed of the wrecks of older rocks.
Each of these rocks once the bed of the ocean.
Evidences that these rocks were formed in the ocean.

(B.) *Causes of transportation and deposition, and on the origin of the materials.*

Dynamical causes of the sedimentary rocks of the globe:

1. Earth a cooling body.
2. Cooling bodies diminish in volume.
3. Bodies revolving on axis if diminished in volume increase in angular velocity.

Application of these principles to the earth:

Equatorial and polar currents restore the equilibrium disturbed by varied angular velocity, and resulting from inertia and centrifugal force.

Influence of the sun upon the ocean and on the atmosphere, is to aid the flow of the polar and equatorial currents.

Reasons of the circular flow of the currents of the atmosphere and ocean.

Directions of these currents in the northern and southern hemispheres.

Causes of these currents permanent as the ocean and atmosphere.

Primary ranges of this continent the same in general position in former times as at present.

Effects of these upon the directions of the great equilibrating currents of the ocean over the territory of the United States before its emergence above the level of the ocean.

Deposition of the materials in suspension and solution in this great eddy.

Connexion of this deposite with that of the eastern British provinces, and in New England, in North America.

Evidences that even this great area of deposition of Silurian and other rocks is but a limited view of the great circular flow, and of the deposition of the sedimentary rocks of the northern hemisphere.

Various classes of facts have led from one generalization to another, until the causes of these rocks were found to require the consideration of causes affecting the entire earth.

Classes of facts mentioned as connected with such causes.

All harmonize with the effects necessarily produced by the flow of the polar and equatorial currents over such areas as they must have flowed over in times past.

Deduction that the materials of the sedimentary rocks of the United States were transported and deposited by such currents.

(C.) *Original sources of the materials of these rocks.*

Evidence during and subsequent to the drift epoch satisfactory.

Evidence preceding the drift epoch often obscure and imperfect.

Evidence of travellers in high latitudes imperfect, yet in connexion with other circumstances may be allowed to have some weight.

Conclusion as to the source of the materials that may be deemed admissible.

Part II.—On the causes of elevation of the sedimentary rocks above the level of the sea.

(A.) *Evidences that these rocks have been elevated above the sea.*

Cannot have been caused by disappearance of water.

Land has not the stability usually assigned to it.

Examples of elevation and subsidence now progressing.

(B.) *Various causes of relative levels of land and water considered.*

These causes slightly discussed and shown to be insufficient, or else referable to a more general cause.

The fourth cause, that of secular refrigeration of the earth, considered more at length.

The earth a cooling body, but in an asymptotic condition.

The increased angular velocity resulting from change of temperature considered.

The day has not varied sensibly for two thousand five hundred years.

Reasons why this cannot be adduced as evidence that the earth has not diminished in volume, and that no geological effects can have been produced dependent on such a cause.

Discussion of the various causes that might produce variation in the length of the day.

A cause adduced that would *tend* to maintain uniformity in the length of the day if the earth be contracting secularly or paroxysmally.

Influence that this cause may have had in the elevation of mountains under the tropics.

Influence of the same cause in the elevation of mountain chains, and the formation of fractures and joints around the earth in an eastward and westward direction between 40° and 50° of latitude.

Consideration of Prof. Rogers' theory of the physical structure of the Apalachian chain of mountains.

Views in part adopted. Perhaps all are true. Mode of testing the truth of them.

Another explanation offered of the folded and reversed strata and eastward dip.

This effect referred to the influence of inertia, when masses are elevated, and thus further removed from the axis of rotation, where their *proper* linear velocity is greater than their *real* velocity.

Recapitulation.—All the various causes discussed may be referred to *one general cause.*

That cause, one that is believed capable of explaining all the phenomena of geology.

This cause, the *secular refrigeration of the globe*, *combined with the effects of gravitation and the rotation of the earth on its axis.*

The discussion on Prof. Mather's paper was deferred until Thursday morning.

Adjourned to 9 o'clock, A. M.

Thursday, *May* 9.—The Association met at 9 o'clock, A. M., *Dr. Locke* in the chair.

The correspondence of the Secretary of the last meeting with gentlemen invited to this meeting was read. The Secretary read a paper communicated by *Samuel Webber*, *M. D.* of Charlestown, New Hampshire—"Observations on some appearances in the alluvial banks of the Connecticut River."

The object of Dr. Webber's paper was to show that the terraced character of the banks of the Connecticut river was due to a gradual and successive change of the river's bed in the progress of a long era; at each successive change of its bed the river flowed on a lower level, leaving its former bed as a low terrace, a little elevated above its new surface level. Dr. W. remarks that this change is going forward constantly, the river now running on the right hand of the valley, and now on the left, altering the boundaries of property, forming islands and new shores, and gradually sinking from its previous levels. In this manner, he conceives, we can account for the very elevated plateaus or terraces of the river banks, seen particularly well at Charlestown, New Hampshire, where the terrace on which the town stands is one hundred and one feet above the present level of the river. Dr. W.'s paper was accompanied by two sectional views in illustration of these phenomena.

A letter from *W. G. Lettsom*, *Esq.* of the British legation, was read, proposing to exhibit to the Association specimens from the "Washington silver-lead mine," in Davidson County, North Carolina. Also, a letter from *Prof. C. U. Shepard*, noticing a fine collection of specimens of native gold from the newly discovered mine at Lincolnton, Lincoln County, North Carolina, owned by Messrs. Cancler & Johnson, now exhibited for sale at 162 Pearl street, New York. Weights, 267, 193, 153, 106, 30 dwts.

Prof. Mather read an abstract of his paper of yesterday, when its discussion was commenced by *Prof. H. D. Rogers*, and continued by *Profs. Mather* and *W. B. Rogers*. No abstract of the remarks was submitted to the Secretary.

Prof. Haldeman read a paper—"Enumeration of the recent Fresh-Water Mollusca which are common to North America and Europe, with observations on species and their distribution."

Eight fresh-water species are enumerated as common to the two sides of the Atlantic; but as some naturalists doubt the identity of species found under such circumstances, a portion of the paper is devoted to an attempt to prove that the same species can occur in the most distant localities, whether by transportation, former connexion of the regions, distribution from more than one original centre, or by transmutation of species. The two first are the most obvious, whilst the probabilities against the third are stated to be not less than as ten thousand to one. The Lamarkian hypothesis of transmutation is reviewed at some length, and the fallacy of many of the arguments brought against it by Mr. Lyell and others pointed out.

For reasons given in the article, the Physadæ alone are taken for the determination of the per centage of fresh-water shells common to the two continents, and of these the proportion assigned is five per cent., a proportion which is stated to obtain in the Brachelytra, a division of insects.

No opinion is offered for or against the theory of transmutation, this being considered an open question, and one which has but a slight bearing upon geology; because, were species transmutable, it would be during the course of the geological periods, and that, whether organic remains be assumed as at one time identical with recent species, but now distinct, or as distinct at all times, the result and its applications to geology must be the same.

A number of authorities are cited to prove that the same animals, in a number of instances, are found to inhabit distinct regions; some physiological points are discussed, and solutions offered to several imaginary problems which have a bearing upon the subject.

After a recess, the *Hon. Nathan Appleton* presented from George Ticknor, Esq. of Boston, a letter from Baron W. Sartorius Von Waltershausen, dated Göttingen, December 18, 1843, describing his great work, called "Ætna and its Convulsions," upon which he has been occupied for seven years, and which will be published in the next six years, one portion each year. The work will be illustrated by a book of plates, containing fifty four large engravings, (beautiful specimens of which were also presented,) to appear in six numbers; a topographical map in fifteen sheets, on a scale of 1 to 50,000, and a geological map in fifteen sheets, with views, sections, and other drawings.

Extract of a Letter from Baron W. Sartorius von Waltershausen to George Ticknor, Esq. dated Göttingen, Dec. 18, 1843.

I send you some of the first proof-impressions of the copperplates intended for my work upon Ætna, upon which, as well as upon the publication of the work, I should like to give you some more minute information.

Amidst struggles of all sorts, after seven years of uninterrupted labor, in which I have avoided neither exertion nor peril, at the expense of not a small part of my fortune, without support from any European government, sustained only by the love of nature and by a strong will, I have fortunately attained my object; and having set aside and overcome all obstacles, I think myself now able to offer to science a work which in other times could have been brought forward only by royal munificence, as for example the Expedition d'Egypte under Napoleon.

Meantime six years are still necessary before the whole work can appear; in each of which years, from this time forward, one portion will come out. The description of the whole, which explains the six divisions, will, I hope, be completed in a few years. The work itself will, I think, form a thick quarto volume, called "Ætna and its Convulsions," and will contain nearly the following matter:

General scientific, topographic introduction; astronomical observations to determine the localities; base measurement and complete triangulation of the volcanoes; topographical description; trigonometrical and barometrical observations to determine heights; examination of terrestrial magnetism; mineralogy; history of eruptions from the Sicanians to the present day; the geology, and the origin, and all the changes of the volcano now to be recognized. The conclusion will consist of general remarks upon the theory of volcanoes, and a comparison of Ætna with the Ligurian volcanoes, Vesuvius, and the extinct volcanoes of the south of Europe.

The book of plates, which, as I said, will appear in six numbers, consists of about fifty four large engravings; a topographical map in fifteen sheets, and a geological map in fifteen. The remaining sheets consist of views, sections, and other drawings appropriate to the explanation of the text.

The fact that the work is of too comprehensive a nature and much too costly for a German public, is sufficient to account for my finding it impossible to procure either publisher or bookseller, and therefore I shall be obliged to meet the expenses of the costly publication with my own means.

As mutual scientific exchange has always had a great charm, and the object of grave works is, at least in part, to be read by an *intelli-*

gent public, it would be my wish that my work should be spread in America, particularly since so great a taste prevails among you for the study of geology. The accompanying plates I send you only that you may form an idea of the execution of the work, and in the course of the next year I will send you the first number.

I must add yet a few words about the engravings. They have been drawn from nature with strict accuracy, chiefly with my own hand, or at least under my supervision; and as they are proof impressions, still unlettered, I have written the titles in pencil. Then comes a proof of a small portion of the topographical map, which, surveyed and drawn by myself, like the accompanying sheet, in the proportion of 1 to 50,000, is engraved here by the master hand of Cavallari, of Palermo, a man of great talent. The whole topographical map will be, if possible, still more perfectly executed, and consists, as above mentioned, of fifteen sheets.

On motion of *Prof. Wm. B. Rogers*—

Resolved, That the thanks of the Association be presented to George Ticknor, Esq. for his very valuable present, transmitted through Nathan Appleton, Esq., of a portion of Baron von Waltershausen's "Ætna and its Convulsions."

Resolved further, That the Association tender to Baron von Waltershausen an expression of the great interest and satisfaction with which they have inspected the admirable specimens of his "Ætna and its Convulsions," and have read his letter accompanying them; and that while they offer him their cordial congratulations on the progress already made in his great work, and earnestly sympathize in the zeal with which he pursues his object in spite of obstacles, they look forward with the greatest interest to the completion of his labor.

Dr. Locke read a paper on the "Connection between Geology and Magnetism," illustrated by beautiful transparencies, topographical and magnetic. Dr. Locke remarked—

In the year 1838 I began to examine the elements of terrestrial magnetism, including dip, declination and intensity, both *horizontal* and *total*, over various parts of the United States. Every year since, I have made journeys to extend this kind of research, until now I have embraced in a general way the region from Cambridge, in Massachusetts, westward to the extreme of Iowa, and from the middle of Kentucky northward to the north side of Lake Superior. It was but natural that I should note the geology of the substratum at each station; and on reducing my observations and putting them into tabular form, I examined the properties of each group, extending over rocks of a similar kind, and found, so far as I had examined, some general indications by which classes of rocks might be distinguished, although concealed at considerable depths, the magnetical instruments in this respect answering the general purpose of a mineral or divining rod.

The method by which this became apparent was as follows: a horizontal base line was laid off, and divided say into millimetres, and upon this as a line of latitude were set up, at distances corresponding to the distances of the stations, perpendiculars, or ordinates, of such lengths as to represent the dip at the given station. This formed a line of points through which a curve was drawn, called a *curve of dip.* The scale adopted was a millimetre to every geographical mile of latitude, and a half millimetre to every minute of dip. As we desired only the differences of these ordinates to generate the curve, only the upper ends were represented—the lower portion, including the zero, or point of no dip, was supposed to be concealed, or cut off below the chart. A similar method was adopted to produce a curve of total intensity, indeed, which was drawn on the same ordinates; the total intensity at Cincinnati being reckoned a thousand, and a millimetre a unit. And here again the lower portion of the scale was omitted. The curves are exhibited on these engravings, [here a diagram was exhibited,] and here it appears that within the region of our horizontal limestone, destitute of dykes, either of trap or any other igneous rock, for many hundreds of miles, and destitute also of local disturbances, the curves ascend in proceeding to the northward with a gentle acclivity, as exhibited on the line from St. Louis along the Mississippi to Prairie du Chien, a distance of four and a half degrees; and again on a line from Lexington, Kentucky, north two degrees, to Piqua, in Ohio. Here it will be seen, that the curve of dip, although it varies its rate of ascent in many places, yet it never descends in progressing northwardly. But a curve similarly generated along the line from Baltimore to New York, where the subjacent rocks are igneous, exhibits ascending and descending undulations, like the outlines of distant primitive mountains; and on a line from west to east, along the south shore of Lake Superior, not only undulate, but so abruptly as to generate sharp needle-shaped points, so sharp indeed that it was impossible to preserve the scale and still delineate them—the horizontal scale at one place being enlarged. The subjacent rocks, in this curve, are various; but they consist mostly of trap rocks, and exhibit abundant signs of igneous action.

I have lately examined the trappean rocks in the vicinity of New York, and find the results diversified and interesting. Here the particulars were stated, and it was suggested as probably true that certain rocky portions of the earth, if they be conductors of magnetism, as trappean rocks, and especially if they be elevated, and of an oblong form, become feebly magnetized by the induction of the earth itself, and acquire a polarity contrary to that of the earth, thus diminishing in a sensible degree the effect of terrestrial magnetism in the neighborhood.

Prof. Locke also announced the fact that the point of greatest magnetic force is at or near Lake Superior. Thus there are three important poles or points nearly on the same meridian, nearly equally distant from each other, and directly north of the United States, being between the longitudes 85 to 90: first, the true north or astronomic pole; second, Ross's pole of perpendicular dip, and of magnetical convergence, twenty degrees south of the astronomic pole; third, the pole of maximum intensity of magnetic force, 21½° south of Ross's pole, 42½° south of the north pole, viz. in 47½° of north latitude.

Prof. L. added that he had made a journey to Europe and purchased the instruments for this research at his own cost; that he had travelled many thousands of miles in making the investigations, and mostly at his own cost; that now he has developed so many points of what seems to him to be a regular system of magnetism in the United States, that he feels almost irresistibly impelled to prosecute the subject, but the economy of his domestic affairs renders it inconsistent with his duty.

Prof. Johnson presented an invitation from Lieut. Gilliss to visit the Naval Observatory this afternoon at 5 o'clock, which was accepted.

The *President* presented the invitation of Capt. Wilkes to visit the collections of the Exploring Expedition at the Patent Office, and also to visit him at his residence on Saturday evening, which was accepted.

Prof. Hitchcock read a paper on "the Trap Tufa, or Volcanic Grit of the valley of the Connecticut River, with inferences as to the relative age of the Trap and Sandstone," with illustrations.

The trap tufa is a peculiar rock of volcanic origin, differing from common trap in being conglomerated, or taking round pebbles into its composition, and being stratified. It also contains one marked example of a vegetable petrifaction, appearing like organic remains in igneous rock. But Prof. H. endeavored to show that it was produced before the main ridges of trap along Connecticut river, by precursory outbursts of pumice, scoria, ashes and melted matter, flowing over the bottom of the then ocean, and mixing with the sand and gravel, and so becoming more or less stratified, and enveloping animals and plants. After this, layers of sandstone accumulated over it, and finally the main ridges of trap were protruded through the strata, tilting them up. Hence this rock, constituting East and West Rock, and the Hanging Hills in Connecticut, and Holyoke and Tom in Massachusetts, was the one of latest origin in that valley—a point which had not before been determined.

Prof. H. added some remarks on the dip of the sandstone in general in the valley, endeavoring to show that in part it was elevated by the protrusion of the trap, and in part by the elevating and lateral movements of the adjoining primary ranges, although it may have been deposited on a plane slightly inclined, but not on one so steep as 15° or 20°, which is the medium dip.

Dr. Houghton remarked on the trap and sandstone on the south shore of Lake Superior.

Prof. Wm. B. Rogers remarked upon some dynamical points not considered by Prof. Hitchcock in his paper.

Mr. Isaac Lea read a paper, "Observations on the Naiades."

Mr. Lea in the introductory part of his paper, advocated the importance and beauty of the study of the natural sciences, and pointed out the advantages of a minute knowledge of species in connection with geology. He then proceeded to state that the family of *Naiades* (fresh-water muscles) was more variously diffused throughout the rivers and lakes of the United States, than in any other country whatever. Attention had therefore been drawn to the development of this branch of zoology much more successfully here than in Europe, where there were not above a dozen real species, while the whole family consisted of about five hundred accurately made out. He mentioned some of the discoveries made here regarding their anatomy, habits, &c., and contended that the opposition made abroad to the large number of contested species, must give way to the facts as most certainly established here.

Mr. L. then went into a statement of the various parts of the *Unio*—its soft and hard parts, (the animal and its calcareous covering,) and dwelt more particularly on the secretions forming the shell, stated the discoveries made recently in England by Dr. Carpenter, and particularly of that very important one of the "basement membrane," (similar to the reticulated epithelium in the mammalia,) by which the calcified portion of the shell is formed. The apparatus of the hinge and ligament was also minutely described, and the position of the shell in its native element shown, together with the normal characters of the parts, precisely and definitely fixing in description its anterior and posterior margins, &c. He then went into the divisions and subdivisions of the *Naiades*, in accordance with the system proposed by him in his synopsis, and illustrated his remarks by specimens of the types. In describing the habits of the *Unio*, he stated that it was sometimes left by freshets on the shelving shore, down which it travelled to its native element, leaving a grooved track to mark its progress. Mr. L. then, turning to his friend, Prof. Hitchcock, who has so happily illustrated the bird-tracks of the new red sandstone of Connecticut, suggested to him that he should bear this fact in mind, as he might in his researches fall in with the path of some ancient *Unio*.

Adjourned to 7½ o'clock in the evening.

Thursday, 7½ o'clock, P. M.—A note was read from *Prof. A. D. Bache*, inviting the Association to call at the office of the Coast Survey, and see the instruments and work.

Prof. H. D. Rogers submitted a communication on the probable constitution of the atmosphere at the period of the formation of the coal. He stated that the recent researches of American geologists, by informing us of the true quantity of coal in North America, enables us for the first time to estimate with some precision its total amount on the globe, and thereby to compute the quantity of carbonic acid which the ancient atmosphere must have contained to supply this vast body of carbon. He showed that the existing atmosphere contains, in its carbonic acid, carbon enough to furnish, through vegetable action, about 850,000,000,000 tons of coal; and that the probable quantity of coal in existence, all of which must have been elaborated from the ancient atmosphere, is nearly 5,000,000,000,000 tons, that is to say, about six times that which the present atmosphere could produce. So great a reduction in the carbonic acid of the earth's atmosphere, implying, as all chemists are aware, a corresponding augmentation of oxygen, is a fact of great interest to geology, as showing that very modification in the constitution of the air which would adapt it to the development of animals progressively higher in the scale of organization, which are known to require a more rapid oxygenation of their blood.

Prof. W. B. Rogers communicated an abstract of a paper relating to the chemical equivalents of certain substances, as inferred from recent experiments by Prof. R. E. Rogers and himself. Referring to the progress of investigation in this fundamental branch of chemical science, he called attention to the recent admirable researches of M. Dumas and other European chemists, as giving strong confirmation to the doctrine maintained by Dr. Prout, of England, that the equivalent numbers of all chemical substances are whole numbers, taking hydrogen as unity. Employing the apparatus for the analysis of carbonates described by himself and Prof. R. E. Rogers in Vol. XLVI, p. 346, of this Journal, he stated the following as some of the results: the equivalent of lime 28, that of baryta 72, that of soda 31. A report of further determinations, by the same process of research, was promised for a future meeting.

Prof. R. E. Rogers described a new apparatus and processes, devised by himself and Prof. W. B. Rogers, for the determination of the amount of iron in iron ores, cast iron, &c. They are a platinum bulb and stem in place of the usual implement of glass, and, by a peculiar construction, a *revolving motion* is given to it, while hydrogen is passed over the heated assay. As a great improvement in the process of reduction, the Profs. Rogers have recently, with great success, employed lampblack in mixture with the ore in a platina crucible, the reduction being effected in a few minutes by a spirit lamp and current of air. The amount of metal is determined by the volume of hydrogen disengaged by the action of dilute acid in a graduated syphon tube.

Prof. John Locke made some oral observations on the lead regions of the Upper Mississippi. Mr. Conrad had announced that these deposits were equivalent to the Trenton limestone, as indicated by the fossils, some of them having lead replacing the animal form. Dr. L. remarked that that portion of the formation furnishing the fossils of Mr. Conrad was not productive in galena. Mr. Owen and himself had determined the principal deposits of lead to be found in a peculiar rock equivalent to the cliff limestone. He had no doubt of the accuracy of Mr. Conrad's conclusions, as far as he (Mr. C.) had had an opportunity of determining the equivalency from the specimens submitted to him.

Doctor Houghton added his assent on the determination, but did not think any peculiar limestone essential to the occurrence of lead. He had formed the opinion that it had, in all cases which had come under his observation, been segregated and sublimed by the heat of intrusive trap. In Michigan he had uniformly found native copper near and attached to the numerous trap dykes, while sulphuret of lead was lodged in cavities of the rocks more remote from the heat.

Prof. R. E. Rogers said that the fact corresponded to what might be expected where sulphurets of copper and lead were heated together ; the former would be reduced to metallic copper by the heat alone, while the galena would be driven into the cooler parts of the rock.

Doctor H. King came to the conclusion, from observations made in Missouri and elsewhere in the west, that no volcanic or igneous agency had any influence in the segregation of the lead; the subjacent beds had no dykes, dislocations, or other proofs of igneous agency. The lead was imbedded in the rock, like masses of chert; had not been volatilized, and it not unfrequently was the matrix of fossils. Further, the chief deposits are arranged in great eastern and western lines, occasionally in plates filling cracks in the rocks.

The oral discussion of the subject was continued until the hour of adjournment.

Friday morning, May 10.—The Association met at 9 o'clock in the Unitarian church. *Dr. Locke* took the chair, and presented to the notice of the Association, from *Mr. John Vancleve,* of Dayton, Ohio, a translation of the first part of Dr. Goldfuss's work on "Petrifactions," part "Zoophytes."

Prof. Hitchcock gave, by request, an oral recapitulation of the points of his paper of yesterday on Trap Tufa, &c. previous to the reading of *Mr. Silliman's* paper on the Trap and Sandstone of Connecticut.

A letter of *Messrs. Booth* and *Boyé*, of Philadelphia, in reference to the "Report on the native compounds of Lime, Magnesia, Manganese, and Iron," expected from them, was read, and it was voted that they be requested to report on the same subject next year.

Mr. B. Silliman, Jr. presented a "Report on the Intrusive Trap of the New Red Sandstone of Connecticut," which was called forth by a resolution of the Association last year, and was confined chiefly to several theoretical considerations, founded on the facts developed by Dr. Percival's able Report on the Geology of Connecticut, and on others of his own observation. The conclusions arrived at by Mr. Silliman, after a full discussion of the subject, which occupied more than an hour, were—

First.—That the sedimentary strata of the valley of the Connecticut were laid down from suspension in water in the *angular position* in which we now find them, (with an easterly dip,) and have suffered no subsequent change of dip, except in immediate connexion with and dependent on the injection of the trap rocks; and further, it was considered probable that these strata were deposited by a primeval oceanic current, setting from the southwest and west, bearing with it the ruins of the primary strata over which it flowed.

Second.—Subsequent to the accumulation and consolidation of the sedimentary rocks, and resulting by known physical laws from this accumulation, a transfer of focal energy in the internal heat was effected, which resulted in the disruption of the lower primary rocks, in the formation of long parallel dykes of trap in them, which discharged their molten contents among the sedimentary strata, distributing it among them along the lines of least resistance, up the plain of the dip, elevating the superincumbent strata parallel to the dip from the beds on which they before reposed, and producing in the superior strata fissures and transverse cracks toward the extremity of the sloping dyke, which were of course filled with molten trap. This injection was probably continued during a long period, but is all referable to the same geological epoch, and anterior to the elevation of the strata in which it occurred; and probably few of the dykes reached a point so elevated as the then bottom of the ocean.

Third.—After the period of deposition and injection ceased, and the elevation of the present continent had commenced, there occurred enormous denudation by a northerly current, whose records are every where found on the rocks in place, and in the loose detritus of rounded and transported drift which every where covers the valleys of this and adjacent regions. This current was probably due to the flowing off of the oceanic waters on the elevation of the present continent. By this denudation the softer shales, and even harder conglomerates, were removed, and the previously formed trap ranges were fully developed; many were removed entirely; the only record we have of them being in the long parallel dykes in the adjacent primary and their ruins among the diluvial materials of Long Island. This point involves the necessity of supposing the secondary once to have had an extension so great as to cover most of the now prominent trap ranges of the Connecticut valley, and that portion of the adjacent primary included between the Southbury basin on the west and the trap dykes of the eastern primary.

It was also inferred that the geological age of this region is satisfactorily determined to be the period of the new red sandstone.

This paper was illustrated by a large colored copy of Dr. Percival's outline map of Connecticut, by two ideal sections in support of the views submitted, and by a picturesque view of the Hanging Hills of Meriden, a well marked instance of the physical character of the trap and red sandstone formation of Connecticut.

Prof. Hitchcock remarked that his views on the question of the elevation of the sandstone, and on the origin of the materials composing it, were different from those expressed by Mr. S. in his paper.

An interesting oral discussion on the points brought under review by Mr. Silliman's paper, was conducted by *Profs. H. D.* and *W. B. Rogers*, *Dr. Smith*, of South Carolina, and *Prof. O. P. Hubbard*, of Dartmouth, which was not reported.

After the recess at 12 o'clock, the business committee reported the order of proceedings for the ensuing day.

Mr. James Hall then read an abstract of a paper upon the Brachiopoda and Orthocerata.

The object was to show the variation in size and form among many species of the Brachiopoda, depending upon the age and the condition of the ocean bed upon which they lived. Mr. H. remarked that, when a student in geology, and making pedestrian excursions with his associates, the motto upon their knapsacks was, "*nullum saxum sine nominum est.*" At that time there were, however, many rocks without names to them. Since that time he had learned from experience that some rocks had as many as five or six names. This remark was applicable to a species of Atrypa, which occurs in great abundance in Ohio, Kentucky and Indiana; this had been received under different names in its different stages, from the plain plicated form of the young specimens to the expanded older form, to which Mr. Conrad has affixed the name of *A. capax.*

The same fossil, in its younger or smaller form, occurs in New York, where it is known by another name.

Similar observations were made in relation to a species of Delthyris, which, under its different phases, has no less than six names applied to it. The gradations in all cases he satisfactorily traced, leaving no doubt of the propriety of referring the whole to a single species. The same was true of other species of Brachiopoda, which would be exhibited to the Association at some future time.

The remarks upon the Orthocerata regarded a peculiarity in the structure, which shows the existence of a long conical or terete tube, like a Belemnite, within the siphuncle. In some specimens there was an insertion of a second tube within the first, and in one case as many as five concentric hollow tubes of this kind. This discovery was regarded as new, and throwing additional light upon the structure of these remains.

Some species did not exhibit this structure, and it may serve as the foundation of a generic distinction.

Specimens were exhibited showing the excentric siphuncle, and these bodies, separately, were often regarded as distinct and independent fossils.

Mr. Lea called the attention of the meeting to the importance of Prof. Hall's observations on the fossil Brachiopoda, where he demonstrated that many species had been made from one, the difference of form having arisen from difference of age, locality, &c.

Mr. L. stated, it so often happened in recent species, that the young differed greatly from the adult, that he thought the geologist should be cautious in coming hastily to conclusions which he might be compelled to retract. He then exhibited specimens of the various stages of growth of the *Unio plicatus*, showing that the first and second growths were really without plications—that after the second stage the first fold began to appear, and subsequently the remainder of the shell was very much plicate. In the tuberculate species this also happens, as was shown by a specimen of *Unio pustulatus;* the first growth having no tubercle whatever; the second growth having two tubercles, and each stage of growth afterwards having two. He also instanced the genus *Melania*, as presenting difficulties in the various stages of growth, as the characters of the whorls when the individual was young or half grown was often entirely different, the marking on the superior half of the whorls being frequently totally different from the inferior half.

Friday, 2 *o'clock*, *P. M.*—The Standing Committee nominated as members: Prof. Ellet, South Carolina College, Columbia; Prof. L. R. Gibbs, Charleston College, S. C.; John Vancleve, Dayton, Ohio; Prof. Alexander D. Bache, Washington; Prof. Joseph Henry, Princeton, N. J.; Rev. John G. Morris, D. D., Baltimore; Peter Edwin Henderson, F. R. S., Civil Eng., York, England; N. P. Ames, Esq., Springfield, Mass.; Prof. Loomis, Waterville College, Maine; Wm. O. Ayres, East Hartford, Conn.: Frederick B. Leonard, M. D., Lansingburg, New York; Prof. Samuel St. John, Western Reserve College, Hudson, Ohio; Prof. Chase, Brown University, Providence, R. I.; Edmund Ruffin, Esq., Petersburg, Va.; Lieut. Maury, U. S. Navy.

Adjourned to 7½ o'clock, P. M.

Friday, 7½ *o'clock*, *P. M.*—The Association met pursuant to adjournment.

Prof. H. D. Rogers, chairman of the last annual meeting, delivered his anniversary address, "On the Progress of Geological Science during the last three years in the United States," for the first portion of which see the subsequent pages of this No.

Saturday, *May* 11.—*Dr. Locke* in the chair.

The order of business was read, and some minor points of business detail were disposed of.

Mr. Lawrence having informed the meeting that Messrs. Gales & Seaton were ready to publish the proceedings of the Association in their valuable paper, the National Intelligencer—on motion of *Mr. Silliman*, it was resolved that the thanks of the Association be presented to Messrs. Gales & Seaton, and that their offer be accepted.

The order of the day was then taken up. *Prof. H. D. Rogers* read a portion of his address, not read on the evening previous, and also a paper, by himself and his brother *Prof. W. B. Rogers*, entitled "A system of classification and nomenclature of the palæozoic rocks of the United States, with an account of their distribution more particularly in the Appalachian mountain chain."

Referring to the great extent of their field operations for the last eight years, which, besides the minute surveys of New Jersey, Pennsylvania and Virginia, have included a general tracing of the great formations from Lake Huron to Alabama, and for great distances across the chain, they called attention to the ample materials thus collected relating to the fossils as well as the mineral character of the formations; from which, and from the data collected by others, they have framed their proposed system of classification and nomenclature. They stated that the great mass of strata composing what they designate as the Apalachian system, whose aggregate thickness in some districts exceeds 30,000 feet, is made up of an extraordinary number of distinct formations, characterized by their organic remains and composition, marking a long series of events and a vast lapse of time, and constituting one uninterrupted succession of deposits, closely linked by an unbroken sequence of animal and vegetable remains. Viewing the whole as a single system, the entire record of one continuous period, they propose to deduce from the study of the organic remains of the different portions of this mass, aided by considerations of a mineral character, a classification in harmony with the natural relationship of the different members throughout the region which they occupy as to *time* and *circumstances of origin*—which, in other words, shall express the various epochs and changes in their true relative importance; and to clothe this classification in language which shall at once be suggestive of these relationships, and applicable every where throughout the vast region of the Apalachian rocks. They showed that in such a system of nomenclature wisely framed, the *primary idea* suggested by the terms should be that of the *order in time* of the successive formations, and that its language, comprising in a symmetrical form all the wider as well as more restricted groups of strata, and presenting the greater and smaller subdivisions in due subordination, should possess such *pliancy* as to admit of expressing by some simple adjunct all the modifications of type exhibited by particular divisions of the system in different or distant regions.

They then gave a sketch of the classification and nomenclature they had framed, illustrating their views by vertical sections of the strata, as exhibited in New York, Pennsylvania and Virginia. Referring to their organic remains, mineral character, and relative expansion throughout nearly the whole extent of the Apalachian chain, they exhibited lists of the fossils and tables of the comparative thicknesses of each formation in the different districts, as explored and measured by them during the last eight years in the region from New York to Alabama, pointing out the limits or area of each formation in the basin, and the range of the principal fossil species both as respects their geographical limits and their extension through successive strata.

Mr. James Hall, in reference to the paper of the Profs. Rogers, congratulated them and ourselves that they had borne such able evidence of the value of organic remains in determining the age of rocks. * * * *Prof. W. B. Rogers*, taking up the subject, said that they had not been understood on this point; they had followed out the intricate structural geology of Pennsylvania and Virginia, relying chiefly on lithological characters, and had found to their great gratification that they had been working in parallel planes to the New York geologists, whose labors among the regular and horizontal strata of that state, relying on the evidence of fossil remains, had brought out results in the main, quite consistent with the determinations of the Virginia and Pennsylvania strata.

Mr. Hall replied that the term "New York system" of rocks was considered by the gentlemen who agreed on it, as a convenient conventional term, but never as a permanent one, and that they were ready to abandon it whenever another, more general and better adapted to cover all the facts, should be presented.

In expounding their views, *Messrs. Rogers* objected strongly to any nomenclature, based upon an examination of local districts, and, referring to the vast fields of American geology now sufficiently explored to justify and require a general classification, and to the opportunity which the yet untrammeled state of the science amongst us affords for the adoption of a comprehensive nomenclature of the rocks, urged the importance of seizing the occasion for framing such a system upon an independent and enlarged basis.

Mr. James Hall and others advocated the more cautious method, in the present state of our knowledge, of adopting a provisional nomenclature, founded mainly on the labors of foreign geologists.

Prof. Hitchcock then read his "Report on Ichnolithology or Fossil Footmarks, with a description of the Coprolites of Birds, discovered recently in the Connecticut Valley," &c.

Ichnolithology is the science of footmarks in stone, and is a quite recent branch of Paleontology. The object of this paper was to give its history, and to add some new facts respecting the footmarks of this country.

Dr. Duncan gave the first trustworthy description of fossil footsteps in the sandstone of Scotland in 1828. They were those of tortoise. In 1831, Mr. Scrope described some tracks of crustaceans in the forest marble of England. In 1834 the tracks of an animal called the Chirotherium were found in Saxony, which Mr. Owen now refers to the Labyrinthodon, or a frog as large at least as an ox. In 1835 the tracks of birds were described from the Connecticut valley. With the four new species added by the author in this report, no less than thirty-four species have now been discovered in that valley, some of them sixteen inches long, or four times longer than the foot of the ostrich. In regard to the first discovery of these tracks, it was stated that the first was found in 1802, by Mr. Moody, and Dr. Dwight of South Hadley, and by them regarded as the tracks of birds, but no account of them was published. In 1835, Dr. Deane, of Greenfield, called Prof. H.'s attention to tracks of this kind, discovered in another locality. After carefully investigating the facts, the latter published, in 1836, an account of seven species, and five years afterwards of twenty seven species, in his final report on the Geology of Massachusetts. Up to that time the author maintained that he had prosecuted this subject alone, and that no other person had investigated it scientifically, and therefore he had a right to be regarded as the real discoverer of bird-tracks in stone, although others first *found* them. He thought that on this point injustice had been done him in the journals of this country and of Europe, by representing others as having preceded him in the discovery and exploration of these tracks, although he believed that such injustice was wholly unintentional on the part of those who had done it.

The author proceeded to describe other examples of fossil footmarks more recently, such as those of Chirotheria, tortoises and reptiles, at three localities in England, among which were some like those of birds; of two singular tracks in Germany; of those of fish, mollusca, and worms in England; of bird-tracks in New Jersey, of reptiles in Nova Scotia, and of supposed tracks on the slates of Hudson river.

He announced also that he had within a few months discovered the coprolites, or petrified excrements of birds, in the sandstone of the Connecticut valley. He submitted specimens to Dr. Samuel L. Dana, of

Lowell, who, by a most rigid analysis and a most ingenious train of reasoning, had found them to be not only coprolites, but the coprolites of birds; and not of birds only, but of a particular kind of birds, viz. the omnivorous, such as have deposited *guano*. These conclusions depended chiefly upon the existence in the coprolite of uric acid, which Dr. Dana had proved by a most elaborate analysis. These conclusions of Dr. Dana the author regarded as one of the most ingenious applications of chemistry to geology he had ever known, and stated that hitherto the coprolites of birds had not been discovered.

This paper was illustrated by a drawing of the Labyrinthodon, an enormous frog as large as an ox; also, by a drawing of a bird on the type of the Apteryx, intended to represent the gigantic Dinornis, or danger bird, whose bones have been lately found in New Zealand. The drawing was sixteen feet high. There was shown also a drawing of a bird-nest twenty-six feet in circumference, and two feet eight inches high, found by Captains Cook and Flinders on the coast of New Holland; and it was suggested that perhaps this might have been the nest of the Dinornis; and if so, that this enormous bird may still be alive in that country.

Several gentlemen followed Prof. Hitchcock's paper with oral remarks.

Capt. Charles Wilkes then read a paper "On the formation of the Antarctic Ice," accompanied by illustrations and a chart of the Antarctic Continent.

Capt. W. considered these bergs and floes as due for their increase chiefly to the congelation of atmospheric moisture, and that it was from this cause that they were always found to consist of pure fresh water, with which he frequently supplied his ship. His illustrations showed in a spirited way the mode of decay and the forms assumed by the bergs in melting: and he explained how powerful they were as agents in transportation of geological materials.

The Association then adjourned to assemble at the residence of Capt. Wilkes in the evening, with the view of seeing the drawings and illustrations, charts, &c., the result of the labors of the gentlemen engaged in the Exploring Expedition.

Monday morning, 9 *o'clock.—Dr. Locke* in the chair. The meeting was called to order, and the Secretary read the minutes of Saturday's session. After some minor business matters were disposed of, it was

Resolved, That this Association hold its next meeting at New Haven, in Connecticut.

Resolved, That the thanks of this Association be tendered to Prof. Henry D. Rogers for his excellent and instructive address before the

Association on the evening of Friday last; and that he be requested to furnish a copy of the same for publication.

Resolved, That Mr. B. Silliman, jr. be a committee to carry the above resolution of publication into effect.

Prof. Hitchcock called the attention of the meeting to the subject of our *transactions*, and the importance of providing some proper vehicle for the publication of the valuable papers already accumulated in the archives of the Association.

The order of business for the day was then taken up by the reading of a description of two new cabinet cases for minerals and geological specimens by *Mr. Lyman Wilder*, of Hoosick Falls, New York, illustrated by small models. The object of the plan was to bring as many specimens as possible within a given small space, by an ingenious and compact arrangement of sliding compartments, having a perpendicular movement by means of a revolving axis, each compartment coming successively into view.

A paper was read by *Dr. Douglass Houghton*, entitled "Remarks on the importance and practicability of connecting Geological Surveys with the Linear United States Surveys."

He commenced this paper with a description of the plan upon which the surveys of the public lands are at present made. It was considered a plan which, for the obtaining of mere geographical information, and for the purposes for which these surveys were more particularly designed, viz. to subdivide the public lands to be offered for sale, is all that could be required; but in other respects these surveys are defective, for the reason that the government does not appear to have in view the attaining of much beyond that single object.

He regarded this as an anomaly; a government carrying forward a vast system of land surveys without obtaining through them that information which would enable her citizens to understand their own country. This mode is entirely different from that pursued by other governments. He instanced the geological survey connected with the Ordnance survey of Great Britain and Ireland, and spoke at some length upon the policy pursued by other European governments.

He saw no good reason why geological and topographical information might not be obtained during the progress of these United States land surveys, sufficient to furnish us with minute geological maps, and that without any material additional expense. He felt certain that this could be done, for he had taken advantage of some of those surveys to aid in perfecting his geological surveys of Michigan. He had, in fact, drawn to his aid some of the deputy United States surveyors, and had tested the connexion of these geological with the linear surveys, and he could say that the plan had proved eminently successful.

He thought it of exceeding importance that the government should at once put in operation some plan for obtaining geological information through the agency of these surveys, and considered the subject in a practical point of view at some length. Its influence upon the cause of science would be of great importance—would give us information at home and character abroad.

He then referred to a report from the late Secretary of the Treasury, Mr. Spencer, made to the House of Representatives in answer to a call upon this subject. He felt much gratified with the character and spirit of this report, expressing as it does in the most decided terms the convictions of the Secretary that a connexion of these surveys "is not only practicable, but of easy accomplishment, and that it is highly desirable in every point of view in which it can be regarded."

He then stated that he had brought this subject before the Association as one which interested every one of its members, and he hoped that some means might be devised by which the object sought could be obtained.

Prof. Hitchcock remarked that he was struck with the importance of the subject presented by Dr. Houghton. He alluded to the action of the English government in connecting with the ordnance survey of that kingdom a full and minute geological survey, under the direction of Sir Henry De La Beche, and suggested whether it was not within the scope of this Association to recommend to the general government some attention to this subject.

Prof. Rogers hoped that Prof. Hitchcock would make a motion for a committee of the Association to be appointed to memorialize government on a subject so important to the great interests of the community as well as to science.

The following resolution was then adopted:

Resolved, That a committee be appointed on behalf of this Association to memorialize the proper department of the government, on the importance of connecting geological surveys with the surveys of the public lands.

Dr. Douglass Houghton, *Prof. Edward Hitchcock*, *Dr. John Locke*, *Prof. H. D. Rogers*, *Dr. Amos Binney*, *Prof. Walter R. Johnson*, *Dr. Henry King*, were appointed a committee in conformity with the above resolution.

Dr. J. Lawrence Smith, of Charleston, S. C., made a communication on some fossil bones from the vicinity of Charleston. The bones noticed were fragments of a rib, resembling that of the *Manatus*, and of a marine turtle and ray. The character of the formation in which they occur was described, it being that extensive calcareous bed which underlies a large portion of South Carolina and some of the neighboring states.

A discussion arose upon the geological age of the formation furnishing the specimens exhibited by Dr. Smith.

Prof. Bailey remarked that the Polythalamia attached to *Ostrea selliæformis* from South Carolina are not similar to those in decided eocene marls from Virginia, but appear to belong to an older period, and agree better with those of the numalite limestone of Alabama.

Prof. H. D. Rogers thought it possible that while the formation is obviously of eocene date, as proved by its larger organic remains, the Polythalamian forms may have been derived from the waste of adjacent cretaceous strata.

Mr. Tuomey said he was glad that Dr. Smith had directed attention to the tertiary strata of South Carolina. If Mr. Lyell's classification be adopted, which admits no fossil as common to the secondary and tertiary, then must the formation in question be referred to the tertiary, while the fossils enumerated by Dr. Smith are those most characteristic of the eocene division of that formation. The geologists who formerly held the opinion that the formation was secondary, have all given up that opinion. He thought, however, that the South Carolina formation will prove to be much older than the eocene of Maryland and Virginia.

Prof. Bailey stated that he had examined microscopically some specimens of marls put into his hands by Mr. Silliman, from Columbus, Mississippi, obtained in boring an Artesian well, and taken from various depths above two hundred and sixty feet from the surface. He remarked, notwithstanding the little promise which their appearance gave of their containing fossils, that they were nevertheless mostly made up of Polythalamia, some of which were the more common forms of the English chalk.

Mr. Jas. Hall then read a paper on the geographical distribution of fossils, in continuation of views presented by him at the Albany meeting last year.

The object of this paper was to present a few additional facts, upon the geographical distribution of fossils in the palæozoic strata of the United States, being a continuation of the paper of last year. In that paper the comparative conditions of the bed of the ocean on the east and west, during the period of these depositions, had been pointed out, and these were proved to have influenced the development of organic forms over the whole extent. During the earlier periods, a large proportion of calcareous matter accumulated in the western waters, while the purely mechanical deposits were largly developed at the east. As a consequence of this condition certain forms were more perfectly developed at the west and southwest, while others were in greater abundance and perfection in the eastern depositions.

Since the period of the meeting in 1843, Mr. H. had had no opportunity of investigating extensive tracts, but having had an opportunity of examining and comparing a large collection made by Mr. Logan, in eastern Canada, he presented the inferences drawn from these. In this collection certain forms were found, which though common in the Ludlow rocks of England, had not yet been detected in New York, nor so far as known to him, in the United States. These were at the same time, associated with other fossils, which occur in the Hamilton group in New York; thus offering a fact in proof of the correctness of the inference of the New York geologists, that the Hamilton group was an equivalent of the Ludlow rocks of Mr. Murchison. Similar facts were shown to be true of several forms in the Hudson River group, in Canada, which though not yet known in New York, were given in Mr. Murchison's "Silurian System," as occurring in the Caradoc sandstone of England and Wales.

Another fact of interest was, that Capt. Portlock had figured many organic remains, identical with those of the Hudson River group in New York, but which Mr. Murchison had not given in his work.

Other facts in regard to carboniferous species seemed to point to the same inference; and there seemed sufficient evidence for the conclusion that certain forms disappear in either direction—or in other words, that some species appear to be almost exclusively American, while others are exclusively European; that as we approach the eastern margin of our continent, the forms are more allied to those of England, while those discovered in Ireland, present us for the first time, on that side of the ocean, with forms before known only in the United States.

In connection with this paper, was presented a geological map of the western and middle states. Upon this map were laid down all the principal formations, showing their comparative development in the different parts of the Union. This map was not presented as the result of his own labors, but as the combined effort of others, who had very willingly aided in the perfection of the map, by furnishing the materials from their several regions. Among the gentlemen mentioned, were Dr. Owen, Dr. Houghton, Dr. Locke, Messrs. Whittelsey, Briggs and Foster of Ohio, Taylor and Ducatel, to whom and to others, full acknowledgments are made in Mr. Hall's report on the geology of western New York, in which this map is published.

Mr. Hall requested to be excused from reading his report on Zoophytes at the present meeting, and to have liberty to continue his labors to another year; which was granted.

After the recess, *Dr. John Locke* read a paper on the more important fossil remains of the country west of the Alleghanies.

The Secretary then presented a communication and letter from *Prof. J. H. Alexander* of Baltimore, accompanied by a meteorological register kept at that city from October, 1843, to May, 1844, in which some points not usually observed were noted.

Prof. J. W. Bailey exhibited to the session, a drawing of a perfect skull of *Mastodon giganteum*, found last year in Orange county, N. Y. He also mentioned Ehrenberg's recent discoveries as to the organic origin of various rocks which possess the oolitic structure, and requested that attention might be paid to any evidence of such structure in our American rocks.

Prof. W. B. Rogers stated that for many years he had been inclined to regard the oolitic structure of some of the Lower Appalachian limestone and chert, as well as the carbon of limestones of this country, as due to organic forms. He had been led to this inference by the remarkable uniformity of the spherules in the rocks, and from traces of what seemed to be septa and other indications of structure in them.

Adjourned to 4½ o'clock, P. M.

Afternoon session.—Dr. H. King in the chair.

Mr. C. H. Olmsted read a paper communicated by *Mr. W. O. Ayres*, of East Hartford, Connecticut, on the identity of the species of Cottus described by Richardson as the *C. cognatus*, by Haldeman as the *C. viscosus*, and by Dekay under the name of *Uranidea quiescens*, with the *Cottus gobio* of Europe. Mr. Ayres comes to the conclusion that they are all *one species*, and the only fresh-water fish yet determined with certainty to be common to the two continents.

The Chairman then announced for Tuesday morning the exhibition of an anemometer invented in this city.

Dr. Douglass Houghton remarked on the publication of Dr. D. D. Owen's survey of Iowa, now lying in manuscript in the State Department in this city. He suggested that the Association should make some representation to Congress recommending its publication.

Resolved, That the subject be referred to the committee appointed this morning on geological surveys of the public lands.

Dr. Amos Binney read a report "On the influence of physical causes on the distribution of the terrestrial Mollusca of the United States and their actual distribution."*

After Dr. B. had finished his paper on the distribution of the species of Helices in the United States, *Mr. Lea* objected to the theory of zoological centres to account for certain species being widely distributed. He said that he had no doubt the small species and the young of the larger were carried from one place to another by birds, on whose toes or legs they may have rested when the bird took its flight, and thus be transported by a single effort twenty, fifty, or even one hundred miles. He could see no difficulty in this.

Mr. Haldeman also made some remarks on this subject, alluding to the views advocated in his paper read on Thursday last.

The meeting adjourned to 8½ o'clock.

Evening session.—Some of the members attended the meeting of the National Institute; after which the Association came to order at 8½ o'clock, *Prof. W. R. Johnson* in the chair.

Dr. Locke favored the Association with an exhibition of his striking transparent geological drawings of organic remains and phenomena, following the order of the stratification.

Dr. Locke then resuming the chair, *Mr. W. C. Redfield* followed by reading a paper, in which he noticed some of the phenomena of the diluvial period, which had led him to infer that the abrasion of the previously existing rocks, and the transfer and distribution of their detritus, now constituting the great superficial formation which is designated as *drift*, took place during a period of both submersion and subsidence of this portion of the earth's crust.

He then proceeded to a description of some remarkably abraded rocks, having the form of isolated cliffs or island pinnacles, which are found in Rootstown, Portage County, Ohio. These pinnacles exhibit abundant memorials of having been long subjected to the lashing of great waves, while partially submersed, at a period which he conceives to have been coeval with, or subsequent to, the final distribution and deposit of the drift. These surge-beaten rocks are supposed to be one hundred feet above

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* As Dr. Binney's views are soon to appear in his elaborate volume on the Helices, now being printed, no abstract is here given.

the summit level of the Ohio canal, and about one thousand one hundred feet above the sea. A water level, to correspond with these abrasions, would now involve the submersion of the greater portion both of the Atlantic and the western states.

The Association then adjourned to 8 o'clock, A. M. of Tuesday.

Tuesday morning, 8½ *o'clock.—Dr. Locke* in the chair.

The standing committee nominated the following gentlemen as members of the Association, who were unanimously elected;

Pres. Bacon, of Columbia College; Dr. S. Reid, of Richmond, Mass.; Rev. Mr. Peabody, of Springfield, Mass.

The Secretary read the minutes of Monday's session.

Mr. Dana read a communication on the occurrence of magnesia and phosphoric acid in recent corals, as discovered by Mr. B. Silliman, Jr. and its bearing on many important geological and mineralogical questions. *Mr. Silliman*, *Dr. Smith*, *Prof. Rogers* and others, made a few observations on the subject.

Mr. Dana's paper will be found entire in the succeeding pages of this number of the Journal. No abstract is therefore given here.

Prof. Hitchcock remarked on the abstract of a paper by Dr. Webber, of New Hampshire, read on Thursday morning, and which he had seen in the National Intelligencer, on the formation of the terraced banks of Connecticut river, which was identical with the theory proposed by himself in his final reports of 1830 and 1841 on the geology of Massachusetts. He did not doubt the originality of the view in question with Dr. W., but as no notice had been taken by any writer he had seen of the views in his final reports, he thought justice to himself required the mention of the fact.

Resolved, That the next session of the Association be convened at New Haven, on the last Wednesday of April, 1845.

Resolved, That Dr. Storer be requested to continue his investigations on the subject referred to him at the last annual meeting, and to report at a future meeting.

Resolved, further, that the same request be made of Dr. Gould, Dr. Wyman, J. H. Redfield, Messrs. Booth and Boyè, Dr. Clapp, Mr. Dana, Mr. Gebhard, Mr. Couthouy, and Prof. H. D. Rogers, in relation to the subjects referred to each.

On motion of *Mr. Haldeman*, it was—

Resolved, That Dr. George Engelmann, of St. Louis, be requested to prepare a catalogue of the recent plants common to both sides of the North Atlantic ocean.

Resolved, That Prof. Silliman, Prof. Olmsted, Prof. C. U. Shepard, B. Silliman, Jr., J. D. Dana, and Edward C. Herrick, be the local committee for the meeting at New Haven next year.

The Secretary then read the acknowledgments of *J. D. Dana* and *J. P. Couthouy*, relative to a charge of plagiarism.

The members of the Association are aware of the charge of plagiarism, made by me against Mr. Couthouy, at the last meeting of the Association. Having seen his private manuscripts within a few days, I feel impelled in justice to Mr. Couthouy, to exonerate him fully from the charge. It affords me the most sincere gratification to find that my suspicions were unfounded; and a deep regret that the wrong should have been committed. As has been stated, I was actuated by no unkind feelings in making the charge, and I now take the earliest opportunity after the evidence is brought forward, to present my full acknowledgments.

JAMES D. DANA.

The frank and perfectly satisfactory withdrawal by Mr. Dana, of his charge of plagiarism brought against me at the last meeting of this Association, renders it incumbent on me to state with equal frankness, that I do upon calm reflection, most fully acquit Mr. Dana of having at any period misused the confidence I had placed in him, and that I sincerely regret having, under a hasty impulse, expressed myself to the contrary in my vindication published in the American Journal of Science.* It is moreover due to both of us, to state that the implied charge of his having done so, was based upon nothing which occurred until after Mr. Dana's return to the United States.

In the appendix to the last number of the American Journal, p. 8, I remark in allusion to my public journals—" that they contain no theories or inferences from the facts recorded in their pages, subsequent to our departure from Callao, I am very certain, inasmuch as after having had my own views therein contained, gravely quoted to me by another, as the result of *his* reflections; I determined thenceforth, while recording facts, to keep my deductions to myself till the time arrived for me to publish them." Although I have the happiness to believe that Mr. Dana himself is now satisfied that he was not the person to whom I referred, still, as my language has been so construed, I feel bound expressly to declare that I never for an instant entertained a suspicion that Mr. Dana could be capable of such a course. On the contrary our intercourse up to the hour of parting at Oahu, was one of unbroken friendship and confidence on both sides, and while I must ever regret that aught should have since occurred to interrupt it, I gladly embrace this occasion to

* Vol. XLV, p. 387.

declare that every unkind feeling which the unfortunate controversy between us may have engendered, has passed away, and that to me, by far the most gratifying consideration connected with the manly acknowledgment of Mr. Dana is, that it opens the way for a renewal of that friendship with which it was so long both my pride and happiness to be honored. J. P. Couthouy.

Washington, May 14, 1844.

Mr. Couthouy made some remarks explanatory of the extent of his views relating to the influence of temperature on the development of corals, as compared with those entertained by Jas. D. Dana, Esq.

The satisfactory withdrawal by Mr. Dana of his charge of plagiarism, made against me at the last meeting of this Association, leaves me at liberty to do what was out of my power while that was impending over me.

As I have ascertained that there exists considerable difference of opinion as to the precise nature of my views on the influence of temperature upon the distribution of corals, owing evidently to my not having expressed them with sufficient clearness, I am desirous of embracing this opportunity of defining with precision the extent of those views which were original with me. This is due no less to Mr. Dana than to myself, inasmuch as he not only arrived at similar conclusions, although at a considerably later period, but in consequence of longer continued opportunities and a series of more minute observations than it was ever in my power to institute, has been enabled to extend these conclusions far beyond any to which I attained.

I will therefore briefly remark, that the general conclusions which I published in my article on coral formations three years ago, were the natural result of a very considerable series of observations, conducted with special reference to this question of temperature as influencing the distribution of corals. Within two months after our arrival in the coral archipelago, I had from the evidence collected, assigned an approximate provisional range of temperature as that most favorable to the development of corals. At Vincennes Island, September 2d, 1839, this was assumed to be "from 77° or thereabouts to 82° or 83°;" but at Onua, October 11th, it was estimated as between 76° or 77° and 80°, as is shown in the following extract from my private MSS. containing the entry made on the spot.

"This shelf has satisfied me that the suspicion entertained on our first entering upon the Paumotus is correct, and that the growth of corals (to a certain extent) depends on temperature as much as depth. It also shows conclusively that they flourish to a depth at least three times as great as that given by Quoy and Gaimard as the result of their investigations, which is twenty-five to thirty feet for the Astræas, the only genus forming a solid surface of any considerable extent. Moreover, since our arrival in these seas, I have repeatedly observed this class of corals in the greatest abundance on the outer plateaux, at a depth of from seven to ten fathoms. How far *below* the estimated temperature here obtained, the Polypes continue to build, can only be determined by many experiments of a more accurate nature than any I have been able to make. Probably (judging from the temperature at the islands already visited) they thrive best between 80° and 76° or 77°, and gradually decrease as the water falls below this last. On some of the Paumotus, I saw branching corals growing on the surface reef where the water was as hot as 85° or 86°, and not more than eighteen inches deep, with here and there among them a few thin encrusting ones; but they had not the vigorous healthy look of those on the margin within wash of the surf, where the temperature was not over 78°. The Astræas especially, seem in such exposed places, washed by the cool breakers, to find their most congenial climate."

By these extracts it will be seen that while I had nearly fixed in my own mind upon an upward limiting temperature, and suspected that we should find the corals gradually decreasing below the temperatures of 76° or 77°, I made no pretense of determining their downward limit. I think I may venture to affirm that had there been opportunity, I should have established this point, but such it was not my good fortune to enjoy.

The two theories or principles therefore which I claim as originating from my own observation and reflection, entirely independent of any communications or suggestions from other sources, are these, expressed in my article in the Boston Journal of Natural History:*—"It is my belief, that (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° Fahr., there, *cæteris paribus*, they will be found to flourish as in the Polynesian seas"—or, as the idea is perhaps more clearly expressed in my remarks on Vincennes Island, "I am convinced of one thing, that this matter of temperature has (to a certain extent) quite as much effect on the growth of corals as depth—that is, *they will grow abundantly in ten or twelve fathoms as here, if the temperature is the same, say 77° or thereabouts to 82° or 83° Fahr., when if it was below that, even at only half the depth, they would not flourish so well.*" The second principle which I claim as original with myself, is a natural deduction from the first. "I am persuaded after a careful examination of the facts, that the absence of coral on the other side of the continent,

* Vol. IV, p. 76.

and in the wide space between it and the low islands of Polynesia, is to be attributed to the prevalence of cold currents, which proceeding northward from the polar regions are perceptible the whole distance from Cape Horn to Callao, and I presume much further to the north, in a temperature of the ocean too low for the existence of the coral animals, and that in a similar low temperature we are to seek for the cause of their absence at the Cape Verd Islands"* and the other islands designated in my article as destitute of coral formations.

That I have never intended to claim more than is here set forth, is I think manifest in the following extracts; and if I have been otherwise understood, the error is wholly attributable to my having defectively expressed ideas perfectly clear to my own mind, and thereby misled my readers.

"It appears to me, that such coincidences as the facts here submitted prove to exist, between certain temperatures of the ocean and the absence or presence of coral reefs, can scarcely be considered by any reflecting mind as merely casual; and that there are strong grounds for believing that we have here a clue to the real cause of the singular absence of recent coral formations in certain regions corresponding in every thing save temperature to those where they are most profusely scattered. In order to enable us, however, satisfactorily to determine *how far* their geographical distribution is affected by such causes, it is essential that we should be furnished with a connected series of observations on the oceanic temperatures at the surface and to certain depths, along both sides of the African continent, the coasts of Australia, and and among the coral archipelagos of the Indian seas, together with that of the seas beyond the limits of such formations, in both hemispheres."†

"While convinced in my own mind of the truth of the suggestions here offered in regard to the absence of coral formations in certain regions, I feel conscious also that the data upon which they rest, though certainly presenting a strong case as far as they extend, are after all but limited in comparison with those still deficient. I submit them for what they are worth. What this may be, time and more extensive observation must determine. Claiming only to have at least sought a more rational mode of accounting for the peculiarities here pointed out, than that of supposing them altogether fortuitous, I shall rejoice if the end show that I have contributed in the slightest degree or in a single point of view to the advancement of the great object to which we are all, according to our opportunities, devoted."‡

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* Bost. Jour. Nat. Hist., Vol. IV, pp. 159, 160. † Ib. p. 162. ‡ Ib. p. 162.

The time and opportunity for more extensive observation which were denied to me, it was the peculiar good fortune of Mr. Dana to enjoy. That they were improved to the utmost, no one acquainted with that gentleman, much less I, whose happiness it was for so long a period to be his friend and companion in many a toilsome excursion, can for a moment doubt. The data to which I allude above as deficient, at least a very large part of them, have been collected by his zealous researches, and practically applied to the determination of problems which I had no means of solving. It is for him to decide then, upon the value of the few suggestions thrown out by me as the result of my more limited examinations; and I doubt not that with a knowledge of the facts now before him, I shall receive at his hands all which is justly due them.

Entitled equally with myself to whatever credit may attach to an original discovery of the great influence of temperature or oceanic climate on the distribution of corals, Mr. Dana has been enabled by a long continued and careful series of observations, to achieve the grand object of ascertaining the entire range of temperature limiting their growth. This is a discovery solely and exclusively his own, so far at least as I am concerned, since all that I ever flattered myself with having determined on this point was the approximate range of their greatest development. *His* is the rich harvest of facts, and their application in a wide field of observation,—mine but the scanty and hurried gleanings of a limited one, a feeble and comparatively trifling contribution to the general stock of knowledge; but in casting it in, I feel proudly conscious that it was honorably acquired, and that had my opportunities continued, the results might not have been far inferior to those presented by my more fortunate fellow laborer.

Prof. W. R. Johnson presented a statement of experiments to determine the evaporative power and other properties of coal from different coal formations of the United States, and other countries.

He exhibited drawings and sections of the apparatus with which his researches had been conducted, gave a tabular list of the coals assayed, mentioned the nature of the questions sought to be determined, the classes of observations necessary to solve the several problems presented for solution, and the results of many of the experiments in regard both to the practical efficiency and the actual constitution of various coals. In analyzing the gaseous products of combustion, the actual composition of those materials gave an opportunity of determining the amount of heat expended on the air used for combustion, the moisture it contained, and the water eliminated from the coal itself, as well as that employed to evaporate water from the steam boiler. By means of the calculated results thus practically obtained, compared with the ultimate analysis of the several coals, he was enabled to bring to a test the theoretical heating power assumed by European chemists. One interesting result which these researches tended to establish was, that the evaporative power is dependent on the carbon constituent of coal. Six different coals, of very different constitution, were cited as affording proofs of this law of heating power.

He stated that those who have sought to determine the heating power of *fuel* for practical purposes, by computing separately the heating powers of pure *solid* carbon and that of hydrogen *gas*, seemed not to have taken into account the amount of heat already in a latent state in the latter material, as used by chemists to determine its heating power, while in fuel the material itself is in either a solid or liquid state. In illuminating gas, (which may be used for heating purposes,) the hydrogen has been rendered gaseous by the agency of a large amount of fuel, burned under the retort. It was stated that the practical bearing of this difference becomes the more important in cases where the products of combustion necessarily pass away from the surfaces to be heated at a temperature above boiling point. The vapor of water thus escaping has the same bulk as the hydrogen from which it has been generated, while the oxygen which had been condensed in forming it possessed only one half that bulk. On the other hand, the oxygen, which, with the carbon of fuel, forms carbonic acid, is unchanged in volume. The carbon may consequently be considered as undergoing no effective enlargement when burned into that acid. In the calorimeters of Lavoisier and Rumford, moreover, the watery vapor generated from hydrogen was condensed by employing cold surfaces, capable of absorbing the latent heat of the vapor generated in combustion, as well as the sensible heat of the gases.

On motion of *Dr. Binney*, it was resolved to rescind the resolution passed last year relative to the election of officers, and to proceed at once to the discharge of that function.

The officers for 1845 were then chosen by ballot.

Prof. William B. Rogers was chosen Chairman.

Mr. B. Silliman, Jr. and *Dr. J. Lawrence Smith*, Secretaries.

Dr. D. Houghton, Treasurer.

Mr. John L. Hayes read a "Report on the Geographical Distribution and Phenomena of Volcanoes," prepared in pursuance of a resolution of the Association of last year.

He examined minutely the geographical distribution of volcanoes with reference to determining the great lines of their geognostic position, and demonstrated that their position was determined by the configuration of the crust of the earth, and the outlines, extent and continuity of the great continental masses.

The most important phenomena of volcanoes were discussed with a view to determine the mode of formation of volcanic mountains. Several illustrations and models were presented to exhibit these phenomena. Volcanoes were shown to have that regular dome-like form which would seem to belong only to masses uplifted and formed by a single effort. It was shown that their internal structure is wholly unlike that of the superficial beds which form their lower slopes, and is that of masses originally formed in a horizontal position. Repeated instances were cited of the formation of mountain masses by elevation at the first outbreak of the volcanic power, and it was inferred that all recent eruptions sensibly uplift the mountain pile.

The mode of formation and the distribution of lava streams, and the eruption and mode of distributing the ejected incoherent materials, were described. It was shown that lava streams of recent eruptions are never arrested in large beds on the higher parts of the volcanoes, and consequently do little to increase the mountain in height. The incoherent materials do little more, but are carried by torrents and inundations to the lower slopes of the volcano.

From the consideration of all these facts, the conclusion was drawn, that all volcanic mountains are formed by the paroxysmal elevation of their rocky strata from below, and that modern ejections act only to increase their extent and to level their bases with the surrounding plains.

Dr. Henry King proposed to give a sketch of the geology of the Valley of the Mississippi, from the southern part of the State of Missouri to Wisconsin River, in the Territory of Iowa.

The limited time he had allowed himself, from a knowledge he had of other business which must come before the Association, would necessarily oblige him to do great injustice to the subject, by causing him to leave out a great many important and even necessary details.

He called the attention of the Association to the primary formation in the southern part of the state of Missouri, consisting of syenite, porphyry, and other igneous rocks, rising into low ridges, with a direction nearly north and south. A few miles south of this part of the state was the earthquake region, rendered memorable by the earthquakes of 1811–12, that nearly destroyed the town of New Madrid, on the Mississippi river, and where ever since shocks of earthquakes have been felt at very limited intervals. On these igneous rocks an upheaved stratified deposit of a very ancient character, probably gneissoid, appeared to rest. But Dr. King had not an opportunity of examining them sufficiently in detail to arrive at any very satisfactory conclusion in relation to their character.

From this point in Missouri to Wisconsin River, a section presents the evidences of one undisturbed, continuous, regular basin, with all the formations above the disturbed strata just referred to, in conformity to each other. The distance between these two points is three hundred and fifty or four hundred miles. The thickness of the formations here is very small when compared with those of the Alleghany region. Hence the basin-like curve between these points is not great, giving the appearance of almost horizontality within moderate limits.

The lower portion of this basin consists of an immense deposite of magnesian limestone ; the upper, of the limestones, sandstones, shales, and coal of the great western coal basin. The magnesian limestone deposite is subdivided by a well marked deposite of siliceous sandstone, generally light-colored, often perfectly white and friable. The lower division of the magnesian limestone is light brown, or ashy in color, very compact or hard, but decomposing with considerable rapidity when exposed to atmospheric or aqueous influences. In Missouri it presents many interesting views of precipices, caves, natural bridges, &c. At Prairie du Chien it rises about one hundred feet above the level of the river, but in Missouri, in the vicinity of Eangua River, it rises much higher. Although it resembles the upper division very much in lithological character, it differs entirely, so far as it has yet been explored, it being totally deficient of fossils and nearly or quite so of valuable minerals. Its entire thickness has never been determined. As there are reasons to believe that it belongs to a distinct geological epoch, and as an important economical purpose will be served in discriminating between it and the upper division, Dr. King proposed to call this the *Eangua* limestone ; this name being of course only provisional, and expected to yield, if on further investigation of the connexion between this and the Appalachian region it shall be found to be only a continuation of one of the latter series, or when a satisfactory systematic nomenclature shall have been adopted by American geologists generally.

Above this division of the magnesian limestone deposite, lies the sandstone already referred to, and which appears rarely to exceed fifty feet in thickness. It is remarkably uniform, being almost identically the same in thickness and lithological character at Prairie du Chien that it is in the southern part of Missouri. Above this, in Wisconsin, is a deposite of limestone, in strata of various thickness, making in all about twenty or thirty feet, remarkable for the abundance of the fossils, for its containing the seams of a brown coal-like matter, and for its marked difference in lithological character from the magnesian deposits above or below. Dr. King did not find this fossiliferous limestone in Missouri, though he was not prepared to deny its existence there. Judging from the ingenious tracings of the New York formations westward by Mr. James Hall, Dr. K. was willing to admit the high degree of probability that this limestone was the attenuated extremity of some one or more of those of that state near the Trenton limestone.

Above this fossiliferous limestone, comes the upper division of the great magnesian limestone deposite, to which the name of cliff limestone has been given by Dr. Locke. It is well supplied with fossils in Wisconsin, but less so in Missouri. Its most important character results from the valuable minerals it contains. It is the formation that furnishes the immense amount of lead produced by the mines of Missouri, Wisconsin, Illinois and Iowa. It abounds in zinc, copper, lead and other metals, and recently cobalt ore to a great extent has been found in it in Missouri. Dr. King presented a number of facts that appeared to him perfectly conclusive that the ores of these metals were deposited contemporaneously with the containing rock. The thickness of this formation in the mining regions was about five hundred feet. He showed, however, that the comparative thinness in this direction was amply made up in the horizontal extension of the formation. He found highly flattering indications of mineral deposits far west of the region to which mining has yet extended.

Above this—the upper division of the magnesian, or cliff limestone—appears a deposite of pure limestone, from thirty to sixty feet thick, which seems to be the lowest deposite of the proper carboniferous formation. Above this the first coal deposits present themselves, which in the western part of the state of Missouri exhibit at least two workable beds. These beds of coal have the usual accompaniments of shale or clay, a few feet in thickness, above and below. Next in the ascending order comes a deposite of brown or reddish sandstone, containing in many places extensive deposits of iron ore. This sandstone may be from sixty to one hundred feet in thickness.

Somewhere between the cliff limestone and this sandstone, there exist extensive sources of salt water. There is scarce a doubt that this salt water could be obtained by means of Artesian wells any where on the southern side of the coal basin, in Missouri or Illinois, within the line formed by this sandstone. This Dr. King stated was an important fact to the farming and grazing interests of these states. Above this sandstone comes a deposite of limestone, whose thickness has not yet been determined, containing at least one and perhaps several workable beds of coal. The area of this coal basin within the limits of the States and territory of Iowa, Dr. King showed to exceed that of the whole of the island of Great Britain.

Dr. King concluded with a rapid description of the character of the country whose geology he had sketched, and a few remarks on the origin of prairies, showing how the noblest forests fall before the puny power of a spear of grass.

Adjourned to 4½ o'clock, P. M.

Afternoon, 4½ o'clock, P. M.—Dr. Charles G. Page exhibited two new electro-magnetic instruments, of his invention, to produce a reciprocal motion. He calls it the axial reciprocating engine.

Dr. P. also made an oral communication on injecting the Lepidoptera with oxalic acid, to produce instant death without injury to the plumage, by using a small glass tube drawn to a fine point, piercing them under the thorax. He also recommended to destroy the grease of the insect by the use of bibulous paper, touching the insect underneath, and in contact with the lining of the cabinet case.

Mr. James Hall remarked on a supposed incorrectness of Dr. King's outline of the coal field in Missouri.

Prof. H. D. Rogers thought that the sandstone called by Dr. King carboniferous, is the Waverley sandstone of Ohio; and Dr. Troost, in his last (seventh) annual report, corrects the statement in his fifth report that it was the carboniferous.

Dr. J. L. Smith made a communication on the existence of the oxide of cobalt near Silver Bluff, South Carolina. The manner in which the oxide occurs is on the surface of a coarse gravel, and was first made known to Dr. Smith by Prof. Ellet. The oxide is associated with the oxide of manganese, and is free from arsenic. The analysis of the mixture of the oxides does not give constant results. The result of one analysis was 35 per cent. of oxide of cobalt, and 65 per cent. of oxide of manganese.

He also made a communication on the composition of fossil bones. Some remarks on the recent analyses of MM. Girardin and Preisser were made. The cause of the presence of fluorine in these bones was particularly discussed, with a view to show that the above mentioned gentlemen were mistaken in their idea that fluorine was infiltrated in the bones after the death of the animal. The principal argument was, that an analysis of two bones taken from the same locality, gave less fluorine in those bones where it was evident that most foreign matter had been infiltrated.

Lieut. Maury, U. S. Navy, read a paper on "the Currents of the Sea as connected with Geology," with a chart of the north Atlantic Ocean. Many of Lieut. M.'s remarks will be found in his interesting paper on the Gulf Stream at p. 161 of this No.

Prof. H. D. Rogers remarked on the importance of the subject presented by Lieut. Maury, and suggested the appointment of a committee who should draw up a series of points of observation, to be presented to the Secretary of the Navy, with the request that they may form a part of the instructions to our naval commanders.

The committee consists of *Lieut. Maury*, *Mr. W. C. Redfield*, *Prof. H. D. Rogers*, *Prof. Hitchcock*, *Mr. J. P. Couthouy*, *Mr. J. D. Dana*, and *Mr. Espy.*

The *Rev. Titus Coan*, of Hilo, Hawaii, Sandwich Islands, *Rev. J. H. Van Lennep*, of Smyrna, *Lieut. Johnston*, U. S. Army, Fort Washita, frontier of Texas, were elected members.

Mr. Silliman and *Dr. Houghton* made an oral communication on the connection of metallic copper with the trap of Connecticut and Michigan, being the subject of reports of those gentlemen, according to a resolution of last year.

Both gentlemen were agreed in the similarity of the phenomena in the two cases, they being much more largely developed in Michigan than in Connecticut. Mr. S. was of opinion that all the copper in the red sandstone of Connecticut had been derived from the adjacent primary. The most valuable deposite of copper in the secondary of Connecticut is found at Simsbury or Granby, on the western border of the secondary, and is immediately contiguous to the granitic basin of Bristol, (in the primary formation G. W. of Dr. Percival,) in which a great amount of variegated sulphuret of copper is found. He called attention to several large veins of barytes in the town of Cheshire, Ct., cutting across the red sandstone parallel to a trap dyke at a little distance, one of which had given abundant indications of copper, (green malachite and variegated sulphuret of copper beautifully crystallized.)

Evening session.—*Mr. Hall* read a "Report by *Prof. Emmons* on Drift." An interesting discussion then arose between *Mr. Hall* and *Prof. H. D. Rogers* on this subject.

Prof. Hitchcock then read a paper on a singular case of the dispersion of blocks of stone at the drift period, in Berkshire County, Massachusetts.

Berkshire County is traversed by several high ridges of mountains, branches of the Taconic and Hoosack ranges, running northeasterly and southwesterly. Across these ridges, and over the intervening valleys, and passing through the town of Richmond, there are two trains of angular blocks of stone, not more than thirty or forty rods wide, extending in a southeasterly direction, not less than fifteen miles, over the

Taconic ranges and Lenox mountain to Beantown mountain. They are a variety of talcose slate, which can be traced directly to a mountain in Canaan, New York, beyond which they are not to be found. Their number is very great, and they vary in size from that of two or three feet to blocks which weigh more than thirteen hundred tons. They are not at all rounded on their edges, and therefore must have been carried, not driven, *pellmell* along the uneven suface. These trains, especially one of them, are remarkably straight and well defined, looking as if artificially strewed over the surface; for they are confined to the surface, and do not mix with the rounded drift beneath. At one point the general course of the train is changed from east 34° south to east 56° south. They were first pointed out to Prof. H. by Dr. S. Reid, of Richmond.

Only one case at all analogous has fallen under the Professor's notice, and that is described by Mr. Darwin, in the Falkland Islands. Vast quantities of blocks are there strewed along the valleys, so as to form "streams of stones." But they lie in the bottom of the valleys, or in the lowest places along their track, and therefore seem to have been produced in a different manner from those in Berkshire, which pass obliquely across ridges from four hundred to seven hundred feet high.

The Professor thought that this case could not be explained by any of the prevailing theories of drift. It seemed to him absurd to imagine that currents of water, however violent, could have thus strewed in a right line so various a train of blocks not at all rounded. Icebergs could not have done the work much better: since a multitude of successive ones must pass over the same spot and along the same line. The trains do indeed resemble the *medial moraines* of glaciers, and the trains transported by packed ice on large rivers. But it seemed hardly possible to conceive how either a glacier or a river could ever have existed in this region. The case, however, is an instructive one, and forms one of the phenomena of drift, which the theorist must explain, or his theory will not stand.

A very interesting and extended discussion followed on the subject of "Drift," (which occupied the remainder of the evening,) between *Prof. Johnson*, *Profs. H. D.* and *W. B. Rogers*, *Lieut. Maury*, *Mr. Redfield*, *Mr. Tuomey*, *Mr. Hall*, *Dr. King* and *Mr. Hayes.* On motion, it was—

Resolved, That the members of the Association tender to *Dr. Locke*, their chairman, their sincere thanks for the zealous manner in which he has presided at their deliberations during this session.

Resolved, That the members of this Association deeply lament the loss by death during the past year of their esteemed associates, *Mr. Nicollet* and *Prof. Hall.*

Resolved, That the thanks of the Association be presented to the proprietors of the Unitarian Church and of the Medical College for the use of their buildings.

The Treasurer's Report was then read.

Resolved, That all persons who wish may resume their papers, and publish them where they please.

Resolved, That the local committee have power to invite persons to our next meeting at their discretion.

The Association then adjourned to meet next year at New Haven.

The present session of the Association was rendered peculiarly interesting from the opportunity presented them of viewing in detail the collections of the recent Exploring Expedition under *Capt. Charles Wilkes*, now exposed, and in good measure arranged, in the extensive and magnificent hall in the new Patent Office. More particularly were the members gratified by the courtesy extended to them by the distinguished commander in exhibiting to them at his house the drawings of the artists and naturalists in the various departments of science, as well as of national costume and scenery. A general expression of surprise was heard at the vast amount of valuable labor performed by the Expedition in all its departments; and in no field will the value of these labors be more generally felt than in the minute and accurate surveys of the groups of Pacific islands visited by the squadron, mostly regions heretofore known to mariners only conjecturally. A beautiful specimen of this sort of work was shown on the large chart of the Feejee group.

A good account of the labors of this Expedition has already been laid before the public in the pages of this Journal by one of the gentlemen of the scientific corps. The geological collections are particularly interesting as being many of them derived from regions where present causes are in the most active operation in producing geological change. Of this nature are the corals, coral chalk, and indurated limestone, as well as the extensive suits of modern volcanic rocks. The fauna and flora of New South Wales and Oregon present many new forms to the eye of the naturalist, and give new importance to several departments of palæontology.

John Locke, *Chairman.*

B. Silliman, Jr. }
O. P. Hubbard, } *Secretaries.*

ART. IX.—*On the composition of Corals and the production of the phosphates, aluminates, silicates, and other minerals, by the metamorphic action of hot water;* by J. D. DANA.*

AT the last meeting of this Association I alluded to the discovery of *magnesia* in corals, by B. Silliman, Jr., who had been engaged in chemical examinations of the corals of the Exploring Expedition. These investigations have been continued since, and lead to the conclusion that this is beyond doubt, the source of the magnesia in magnesian limestone. The analyses are not yet completed and I cannot therefore give definite results.

Besides finding magnesia, Mr. Silliman has made the more remarkable discovery of a large per centage of phosphoric acid. Analogy had taught us that corals correspond in their nature and relations to the bones of higher animals, and we have now farther evidence of this correspondence in their composition. The phosphates constituted in some instances, 9 or 10 per cent. of the whole.

These facts seem to prove, what has long been suspected, that the primary limestones and dolomites are altered sedimentary limestones, and that these limestones may be in part, of coral origin. The so frequent occurrence of phosphate of lime (apatite) in this rock, is explained by the same discoveries, and corroborates this view of its origin. The little fluorine which apatite contains, (6 or 8 per cent.) adds additional probability to these conclusions; for although fluorine has not yet been detected in these polyp remains, by analysis, fluor spar is a common mineral in fossiliferous limestones, and often occurs in the cavities of shells and other fossils, as if proceeding from animal decomposition.

The same heat, then, that crystallized the limestone, crystallized also the apatite, splendid localities of which occur in the limestone of northern New York as well as in Orange County. It is universally admitted that this crystallization took place under the pressure of an ocean, and we may believe that the heat was distributed by means of its waters, both permeating and superincumbent.

These heated waters, like the hot waters of igneous regions generally—fine examples of which are seen in New Zealand, as well as the Geysers of Iceland—contained silica in solution. Through the action of this silica on the lime and magnesia, and on the oceanic salts present, may have been formed the minerals *serpentine*, *scapolite*, *pyroxene*, *tremolite*, &c. so common in granular limestone and dolomite.

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* Read before the Association of American Geologists and Naturalists at Washington, May, 1844.

Another source of silica is found in the clay which sedimentary limestones often contain, hydraulic varieties of which sometimes include 40 per cent. A hydraulic limestone from the Helderberg, N. Y., analyzed by Prof. Beck, contained 36 per cent., 28 per cent. of which were silica; and another from Rondout, Ulster County, N. Y., contained 24·50 per cent. of clay, 15·37 of which were silica, and 9·13 alumina. This may account for the occurrence of the above minerals without the infiltration of silica; yet the large amount of silica or siliceous minerals in some granular limestones, seems to require more than can be reasonably supposed to be supplied from this source. Moreover, the occurrence of silica in solution in regions of volcanic agency, is known to be frequent, and cannot in all instances be excluded in accounting for these metamorphic changes and crystallizations.

The alumina of the same clay in impure limestones uniting with the magnesia and silica, may have given rise to the *aluminous varieties of hornblende*, and the iron often present may have contributed towards producing the *dark ferruginous* varieties. The formation of mica may be explained on the same principles.

The presence of fluorine has already been alluded to; and this, with the silica and magnesia, appears to have produced the *chondrodite*, another very abundant mineral in our dolomites.

The alumina and silica with potash or soda from heated volcanic waters might have originated crystals of feldspar, albite, &c.

Where spinels have been formed, the alumina of the altered magnesian limestone may have predominated over the silica and thus have given rise to this mineral—an *aluminate of magnesia*. Spinel is usually associated in Orange County, N. Y., with *chondrodite*. And it seems probable that the little fluorine present at once took possession of the silica, and formed fluosilicic acid,—for the attraction between fluorine and silica is known to be one of the strongest in chemistry—and thus combined, produced by uniting with magnesia the mineral *chondrodite*, which is a compound of silica, fluorine, and magnesia. Where spinels are not found, as with rare exceptions is the case in St. Lawrence County, N. Y., it may be owing to the large amount of silica present, or the absence of fluorine. Chondrodite is nearly as rare a mineral in northern New York as spinel.

ART. X.—*Address Delivered at the Meeting of the Association of American Geologists and Naturalists, held in Washington, May,* 1844; by HENRY D. ROGERS, Prof. of Geology in the University of Pennsylvania; F. G. S., &c.

GENTLEMEN OF THE ASSOCIATION,

HAVING been kindly invited by you to preside at the last annual meeting of the Society, it devolves upon me, in accordance with our rules, to bring before you on this occasion a brief history of the recent labors of American Geologists, and to take a rapid survey of the present condition of geological research in the United States. In attempting to discharge this acceptable but by no means easy duty, I am well aware that my sketch will exhibit many defects and omissions, incident in part to the dispersed state of my materials, but attributable in greater part I fear, to my own imperfect fitness for the task. Those deficiencies in this short review of American geology, which you cannot but impute to myself, your generosity will, I feel assured, indulgently pardon, but those others, which you must ascribe to the hitherto insufficient concentration of scientific effort in our country, I would not have you so lightly overlook.

On the other hand, I would here invite your attention to the difficulties, which though much abated, still beset any one who attempts either to gather into shape the scattered materials of American geology, or to prosecute extensively a connected train of research. It was in full view of these difficulties and in the hope of lessening them, that our Association was organized. And let us here congratulate each other on the success which has attended our efforts. Scattered over a country of great extent and kept asunder by distance and the claims of professional duties, the American geologists were laboring amid all the inconveniences of solitude, each hewing his lonely path through the mighty wilderness of our rocks, isolated in the worst sense of the term,—in the only sense really repulsive to the genuine student of nature,—I mean, isolated from the sympathy, the counsel, the instruction, of those engaged in the same glorious enterprise. Though fellow-laborers we were not companions, for we seldom met, knew imperfectly each others' performances, and were still less acquainted with each others' social and scientific worth. Many of you, by your published researches had made your labors to be valued, and had won the sincere respect of the rest, but how different was that respect, when from the author we came to know the man, when with opportunities of personal intercourse we learned *how* the work had been accomplished, what impediments from deficiency in books had been surmounted, and what physical hardships had been braved, but especially when we learned the spirit of the explorer, his intrepid zeal, his untiring patience, and his devotion to the cause of truth.

If we contrast our recent imperfect knowledge of each others' discoveries, with the present ample summary which we are now able to take of the geology of three fourths of the vast region between the Atlantic and the Mississippi, if we are conscious how much more intimately we now comprehend each others' published descriptions and theoretical views, and feel how much more justly we can now estimate the relative accuracy and value of each others' researches, the purity of our views, and the intenseness of our enthusiasm, and consider how much of all this change has been brought about by our annual meetings, we shall indeed see reason to congratulate ourselves on our success. To labor unostentatiously for the advancement of American geology and the collateral branches of natural science, by cultivating a spirit of mutual fellowship and generous coöperation, was the intention, has been the course, and should ever be the pride of this Association. Our ranks it is true are thin, for grievous obstacles, especially distance and want of time, restrain many from joining us who share our tastes, yet has our zeal never been daunted, for at every meeting we number among our associates some from the remotest corners of the country. The long and arduous journeys which some of you annually perform to reach our place of meeting, are a sufficient proof of the utility and attractiveness of our proceedings. Should it be asked what are the scientific and other fruits of these annual assemblages, I would point not boastfully, yet with a just pride, on the one hand to both our printed and unpublished papers, and on the other to the excellent spirit and the friendly intercourse which have invariably marked our proceedings.

Our youthful Association, though it would not presume to invite a comparison between its labors and those of the similar societies of Europe, is yet conscious of being moved by a kindred impulse, and of seeking through accordant means, the same praiseworthy objects. Those illustrious assemblages, which in England, Germany, Italy and Switzerland, have in the last ten years done so much to quicken the march of science, so much to call out from solitude and obscurity, modest learning and genius, so much to win for the student of nature, the once withheld respect of literary scholars, statesmen and governments, and best of all so much to make science and letters what they are yet far from fully being, a true republic, all were the results of the same necessities, the same intellectual and social wants, and the same high aspirations, which drew the geologists of America, a small but an enthusiastic band, together.

It is proper that before entering upon topics of purely scientific interest, we pause to pay a tribute to the memory of our esteemed and lamented associates, Prof. Hall and Mr. Nicollet, both of whom at the inexorable mandate of death, have lately left this scene of their scienentific usefulness.

Prof. Frederick Hall possessed great ardor in the pursuit of knowledge, and was one of the earliest cultivators in this country of geology and mineralogy. He was educated at Dartmouth College, became professor of natural philosophy and chemistry in Middlebury College, and afterwards president of Mount Hope College in Maryland, and at the time of his death (which took place at Peoria, Ill.) was professor of chemistry in the medical department of Columbia College, Washington. Of a generous and enthusiastic nature, he manifested the sincerity of his zeal for science and the cause of education, by the very handsome contribution in money and a rich cabinet, which he made a few years ago in aid of the establishment of a new professorship in the respected institution from which he imbibed his strong love for learning. Though not a practical geologist, he was a successful teacher of the science, evinced a lively interest in its advancement, and earnestly encouraged this our present effort to promote its progress in our country.

Mr. Nicollet was born in Savoy between Geneva and Mont Blanc. Repairing to Paris about the age of twenty, he applied himself with great enthusiasm to improve, through the resources of that capital, the defects of his early education, and gifted with a fine mathematical ability, and enjoying the lessons and friendship of Laplace, he soon rose to distinction as a mathematician and astronomer. In his assiduous devotion to his favorite branches, he became the author of several works of merit, and of many papers and memoirs which procured him a well deserved reputation in the scientific world. He was a member of *Bureau des Longitudes*, and one of the principal examiners in the French navy.

About ten years ago, Mr. Nicollet came to the United States, and impressed with the abundance of the scientific harvest which he saw spread out before him, he entered forthwith upon the rich field of his subsequent labors. With extraordinary ardor, he applied himself to the study of the physical geography of the great valley of the Mississippi. By a widely planned series of astronomical and barometrical observations, he collected in a few years, data for exhibiting with an accuracy never before attempted, the features and the physical relief of that enormous basin. To these researches, he united an extensive system of observations upon terrestrial magnetism, and though not a trained geologist, he was successfully gathering a body of very instructive facts respecting the stratification and organic remains of the region of the Missouri and upper Mississippi. Exploring, in solitude and with indomitable courage, the vast and inhospitable wilderness of the far west, with none to bear testimony to his hardships and earnest zeal, but the equally isolated and fervent Indian missionary, or the wild aboriginal, whose profound veneration and affection he enlisted by his commanding powers, kind manners and humane and gentle character, he came among us after the lapse of a few years, somewhat broken in health, but loaded with scientific treasures.

He was then engaged by the war department, directed by Mr. Poinsett, to revisit the far west and to embody his observations in a general report and map for the use of the government. Of that portion of his labors, which relates to the region traversed by the head waters of the Mississippi and extending to the Missouri, this Association has already had a glimpse, through the interesting verbal communications of Mr. Nicollet himself, made at our two last meetings. His beautiful and elaborate map, seen by us at Albany, is finished, but exhaustion, sickness and death, the result of over toil arrested the completion of his report. As he had given much attention to the customs, polity and vocabularies of our Indian tribes, and had gathered a diversified mass of valuable materials, it is greatly to be lamented, that for the cause of science, he was not spared to give his work to the public. All those whose privilege it was to listen to his clear, ingenious and eloquent descriptions of the majestic country, the scene of his researches, must be conscious how much American science has lost in the premature departure of our gifted associate.

RECENT PUBLICATIONS ON THE GEOLOGY OF THE UNITED STATES.

In attempting to trace the progress thus far made in developing the geology of the United States, I shall first present a concise sketch of the publications embodying original researches made during the last two or three years. You have already been furnished by my predecessors in the chair, Prof. Hitchcock and Prof. Silliman, with a clear and instructive history of the contributions to American geology, from the first valuable labors of Maclure in 1809, to the period of 1841. In the interval which has since elapsed, a copious mass of publications has appeared, enlarging prodigiously our acquaintance with the exact geographical distribution of our strata, with their relationship to each other, and to the rocks of Europe, and with their organic remains, and their other contents. Many of these embody the results of years of previous systematic research, prosecuted in obedience to legislative enactment in several of our states, while others are the fruits of individual investigations, conducted by members of our Association, and others again, the contributions of distinguished foreign geologists, liberally assisted with materials by our own explorers.

These publications consist chiefly of the printed volume of Transactions of this Society, published about a year ago, the five large quarto volumes on the Geology and Mineralogy of the state of New York issued by that state at intervals during the last two years; also the first Report on the Geological Survey of Connecticut, and those papers read at our last annual meeting, which have appeared in the American Journal of Seience and Arts, together with memoirs submitted to the Geological Society of London, by Owen, Lyell and Logan.

By these reports and memoirs, our knowledge has been greatly enlarged in relation to almost every class of our rocks and every period in our geological chronology. Respecting the primary crystalline masses, we have received most valuable additions to the details previously in print, through the minute description of Connecticut by Dr. Percival, the interesting and instructive account of the district between the St. Lawrence and Lake Champlain, by Prof. Emmons, and that of the southern counties of New York by Prof. Mather. We have also been presented with an interesting article on Tin veins in New Hampshire by Dr. C. T. Jackson.

Concerning the *Palæozoic* strata of the United States, the publications have been of an interest commensurate with the magnitude and grandeur of the formations. Besides the accounts already given of the range of these rocks in New England; principally by Hitchcock and Jackson: a thorough and minute analysis of these as developed throughout the wide state of New York, has been furnished by Emmons, Hall, Mather and Vanuxem in their respective reports on the survey of that state. The identity of some of the western strata with those of New York, is well set forth in a memoir by Mr. Hall published in our volume of Transactions, while a valuable contribution has been made to the geology of the western states, by Dr. Owen in a paper on that subject, read to the Geological Society of London. The extension of some of the

New York strata through the western part of Upper Canada and the adjacent districts of Michigan and Ohio, was briefly sketched in a paper laid before the American Philosophical Society in 1842, by my brother and myself; while to Dr. Troost's last Annual Report on Tennessee, published in 1841, we are indebted for some interesting additions to our knowledge of the Palæozoic rocks in that state. A short communication made by Dr. Clapp of New Albany in Indiana, to the Academy of Natural Sciences of Philadelphia, has also contributed to assist in the identification of the western with the eastern states.

The Appalachian coal strata of Pennsylvania, Ohio, Maryland, Virginia, Kentucky and Tennessee, bituminous and anthracitic, have been compared and traced, and a theory of their origin ventured upon in a paper by myself, submitted to this Association; and my brother and myself have also presented a memoir on the Physical Structure of the Appalachian chain.

The areas occupied by the palæozoic strata have been already carefully delineated in geological maps, for a large portion of the United States, this side of the Mississippi. Thus we are in possession of an excellent map of the state of New York by the state geologists, and one on a small scale of Ohio, Indiana, Illinois and Kentucky by Lawrence. A map of the last named states, including also middle and western Tennessee, has likewise been carefully compiled, principally from personal observation by Dr. Owen, and gives us the most exact picture we yet possess of the distribution in that region of the several groups of our palæozoic strata under their western types. The information conveyed by it has been fully imparted, though the map has not, I believe, been yet published. The palæozoic formations of New Jersey and Pennsylvania have been mapped by myself and my assistants, those of Maryland by Dr. Ducatel, and my brother and myself, while those of Virginia have been minutely delineated by my brother and his corps, in the survey of that state. But the geological maps of Pennsylvania, Maryland and Virginia have not yet been published. To Mr. Conrad we owe the approximate tracing of the southern border of these formations in Alabama. At our last meeting a valuable paper was read by Mr. James Hall, on the geographical distribution of the fossils of the palæozoic rocks of the United States, and another on the geological and geographical distribution of the Crinoideæ of the older rocks of New York.

Our knowledge of the *Mesozoic* strata of the United States has been considerably advanced by publications issued during the two or three past years. Thus Dr. Percival in his report on Connecticut, has described and mapped the mesozoic red sandstone of that state in detail, affording much minute local information concerning the numerous trap dykes which intersect it. Prof. Mather has in like manner in his report on New York shown its boundaries and structure in Rockland county. To Prof. Hitchcock we are indebted for a paper printed in our Transactions, describing five new and interesting species of ancient bird tracks in the same formation in the Connecticut valley, together with several obscure fossil plants, and to Mr. William C. Redfield the thanks of the Association are due, for a communication made last year at Albany on the fishes and bird tracks discovered by him in the same formation at Pompton, New Jersey. From Prof. William B. Rogers we have learned the existence of the *Posidonia Keuperi* in this rock in Virginia, a fact almost decisive of the European affinity of the formation. He has also in a paper printed in our Transactions settled with considerable exactitude the age of the interesting coal rocks of eastern Virginia. The limits of the mesozoic red sandstone (new red) in North Carolina have been described and mapped by Prof. Elisha Mitchell of that state.

Mr. Lyell will probably enlarge our knowledge of the foreign affinities of the *Cretaceous* strata of New Jersey, the Carolinas and Georgia, by a comparison of the fossils procured by him in this country with those of the cretaceous groups of Europe. Dr. Morton to whose patient and skillful investigation of the cretaceous fossils of this country we are indebted for the chief part of our facts respecting this interesting division of our Palæontology, has recently laid us under new obligations, by figuring some striking forms, collected by the lamented Nicollet in the remote and vast cretaceous region of the Missouri. At the last meeting of the Association Mr. Nicollet himself, read an interesting sketch of the cretaceous beds, examined by him on the Missouri river.

Our acquaintance with the *cainozoic* or tertiary strata of the United States has also been materially advanced in the interval under consideration. Mr. Conrad in a communication to the National Institute, made in 1841, has presented a clear and interesting synopsis of his researches among the tertiary beds of some parts of Maryland, and of the southern states, and Mr. Lyell in two papers to the Geological Society, has contributed various useful facts concerning the same strata in the Carolinas and Georgia and in Martha's Vineyard. Mr. Hodge has also given the Association his observations on some parts of the southern tertiary and cretaceous strata. The list of miocene fossils of Virginia, has been considerably enlarged by Mr. Henry Lea of Philadelphia, in a paper read to the American Philosophical Society, describing a number of minute species; and Mr. Conrad has figured some interesting species collected by Mr. Hodge in North Carolina and by himself in Maryland. Prof. William B. Rogers has during the last two years traced extensively the tertiary infusorial stratum of Virginia, while Profs. Bailey and Ehrenberg have discerned in this material a great multitude of curious microscopic forms. Mr. J. Hamilton Couper of Georgia has written on the bones of certain fossil mammalia in that state; and Mr. Lyell has published a paper (Proceedings of the Geological Society of London) on the position of the Mastodon giganteum and its associated fossils in Kentucky and elsewhere.

Several papers on topics in geological dynamics and in chemical geology have also appeared. Thus the complicated and interesting question of the origin of the vast drift formation of this continent, has been discussed by Dewey, Emmons, Hall, Hitchcock, Jackson, Mather, Vanuxem, and by my brother and myself, so that it may be said, that we are now in possession of the observations and theoretical views of most of the investigators of this curious deposit. The laws of earthquake motion, the explanation they offer of the origin of anticlinal flexures and folds in strata, and the elevation of mountain chains, and the aid which the same views afford in accounting for the facts connected with our coal strata, with the drift and other formations, have been in several shapes submitted to the geological world by my brother and myself. Mr. J. D. Dana has read at our last meeting a paper containing a theory of the metamorphism of rocks, attributing their schistose structure to crystallization, and ascribing their alteration by heat to "the heated waters of a surrounding ocean." Two papers were read on the same occasion, by Prof. Lewis C. Beck, one "on certain phenomena of igneous action chiefly observed in the state of New York," in which the author arrives at the interesting conclusion that certain rocks have been subjected to a high temperature subsequent to the formation of the minerals in them, by which these were softened and deranged in shape. The other paper was on the ancient climates of the globe. Our volume of Transactions contains a paper, by Mr. Vanuxem, on the origin of mineral springs, and one by Prof. William B. Rogers on the connection of the thermal springs of Virginia with anticlinal flexures and faults. To Prof. Bailey we were indebted last year, for a paper on the crystals formed in the tissues of dicotyledonous plants, and a valuable report was submitted to the Association at the same meeting, by Mr. John L. Hayes, on the transportation of detrital matter, by icebergs, affording useful facts connected with the theory of drift. At the preceding meeting Mr. Couthouy it will be remembered read an interesting paper, descriptive of some of the phenomena of icebergs as witnessed by himself at various times. This paper, published in our proceedings, contains important statements in relation to the partial rotation of icebergs when aground.

Among the chemico-geological communications recently made to the Association, is one by Prof. Lewis C. Beck on the bituminous matter in several of the limestones and sandstones of New York, and another by Dr. Charles T. Jackson, on the organic matters of soils.

PRESENT STATE OF OUR KNOWLEDGE OF THE FORMATIONS OF THE UNITED STATES.

I shall now review as briefly as practicable, the geology of this country, and show the development which it has reached through the researches above cited, through those of earlier date and those not yet in print. In taking such a survey we shall find that, while the materials already gathered, form a valuable accession to the positive geology of our times, some of the conclusions arrived at, and many of the questions presented, bear upon some of the most fundamental generalizations of the science.

Let us enquire in the first place, what we know touching our *palæozoic strata*, the sediments of that enormous sea, which filling once the whole interior of our continent, has its history, despite of all catastrophes, beautifully recorded in the vast sheets of matter, which from beyond the lakes to Alabama, and from the Atlantic slope to the far Missouri, tell of its depths and changes, its earthquakes, its intervals of long repose, and the structure and mode of life of its inhabitants. As the most expanded, and by far the most complicated in its outcrops of all the systems of strata this side of the Mississippi, it may be well to ascertain over how wide an area we have succeeded in tracing and mapping its numerous subordinate formations, and as the repository of a host of organic relics, leading us back to the extreme dawn of animal and vegetable life, and forward through a long series of successive creations, it becomes of the highest interest to learn how far we have advanced in exploring its fossils, and in framing in accordance with their distribution, a classification of the formations which shall be widely applicable.

The outcrops then of these vast formations, commencing at the north-east in Vermont, have been traced through the western border of Massachusetts by Hitchcock, and westward through New York by Emmons, Hall, Mather and Vanuxem; through Ohio by Briggs, Lawrence, Locke and Owen; through Upper Canada, north of Lake Erie, by my brother and myself; through Michigan by Houghton and his corps; through Indiana, Illinois and part of Wisconsin by Owen and Locke; through Kentucky and Middle Tennessee by Owen and Troost, and southward along the broad Appalachian chain and the coal fields west of it in New Jersey and Pennsylvania, by myself and corps; in Maryland by Prof. Ducatel, my brother and myself; in Virginia by my brother and his corps, and in East Tennessee to Alabama, by my brother and myself. From Lake Champlain therefore, westward to the mouth of the Wisconsin river, a distance of at least eleven hundred miles, and southward to Alabama over a still larger and very complicated tract, and throughout the entire triangular area included between these limits, the boundaries of each of our Palæozoic Appalachian formations have been determined, and with very considerable precision. To a result so practically useful, so fraught with scientific benefit, so creditable to our young country, the numerous geological surveys by state authority, either completed or begun, have principally contributed.

Although but seven or eight years have elapsed since most of the surveys of the wide region before us were instituted, so diligent have those engaged upon them been, that we are now almost ready to unite the whole of our lines, into one comprehensive and huge map of the entire Appalachian basin. If there were a general map of the United States, or a series of state maps based on a common scale, it might even now be practicable, combining the published data with the yet unpublished materials for Pennsylvania, Maryland, Virginia and East Tennessee, in the possession of my brother and myself, to produce a geological picture of the wide surface mentioned, upon which should appear each special formation in all its separate belts or outcrops. The real condition of topographical geology, is indeed already such, that our treasures are beyond the size of the caskets which should contain them; and much that has been deciphered, the most beautiful structural features, it is feared must go unwritten, not for want of *scribes* certainly, but because of the narrowness of our *tablets*, because of the insurmountable impediments from bad geographical maps. A tolerably correct and clear delineation of the limits of the strata is practicable enough, with imperfect maps, where the rocks are nearly horizontal, and their outcrops are wide and not tortuous, and, therefore, from the Mohawk westward, the formations are depicted with sufficient precision in the geological map of New York, and in the unpublished one of the states farther west, compiled by Dr. Owen, but when as in the long and complicated Appalachian chain, almost innumerable anticlinal flexures of various lengths and forms, throw the strata into nearly countless belts, some of them of extreme narrowness, and which wind about in strictest conformity to the topography, their delineation is much more difficult, and an accurate and ample map basis, on which to lay them down, becomes of the first importance. Impressed with this conviction and reluctant to see the errors and distortions of the existing map of Pennsylvania, falsify the more accurately drawn lines of the strata, furnished by a seven years' laborious survey, I have with the aid of my assistants, especially Messrs. Jackson, Henderson, Lesley, McKinley and Whelpley, constructed a more accurate general map of the Appalachian mountains of the state on a scale of two miles to the inch, upon which I am able to depict every outcrop and every axis of importance. Though far from possessing the precision of a thorough map, it is the only approximately exact picture we yet have of any part of our great mountain chain, so peculiar in the symmetry of its structure, so instructive as to the close dependence of topographical features upon the hardness, thickness and dip of the strata, and so interesting in a yet more scientific light, as revealing the nature of the grand and wonderful movements through which the great flow of an ancient sea, rose and became wrinkled into this stupendous zone of long parallel mountain crusts.

Availing ourselves of some of the abundant materials for a detailed geological map of the United States, my brother and myself have constructed for our own convenience a general map of the strata, fourteen feet by twelve feet, painted on canvas by R. Smith. It aims not at minute accuracy, nor does it embrace all the narrow belts of the Appalachian formations in their southeastern outcrops, each of the great natural groups of the strata only being represented in this part of the map.

While we thus see how much has been done towards tracing the limits of each formation, we must remember that accurate mensuration of the strata has also for most districts, been carefully attended to. Having at an early stage of their researches, ascertained the order of superposition of the stratified masses, and analyzed them, at least approximately, into groups and formations by aid of their organic remains, and their well-marked lithological limits, the geologists in charge of the various surveys have, while tracing the boundaries of each stratum, been patiently estimating its variations of thickness and its changes of aspect, texture, and composition. The labors of Emmons, Hall, Mather, and Vanuxem, in New York, and of Hall, Locke, and Owen, especially of the latter, in the Western States, have taught us many curious facts concerning the modifications which the more persistent strata undergo in this westward range from the Hudson to the Mississippi. The changes which the same mineral deposits experience in their long course from the Hudson to Alabama, have been examined by my brother and myself, and have already exposed to us some beautiful general laws of variation, which we believe will throw light on the character of the physical conditions and movements that accompanied the diffusion of these ancient sediments. As the strata generally reascend to the surface several times across a wide belt in the mountain chain, especially in Pennsylvania and Virginia, it is practicable to study the variations of type, not in one direction merely, as from N. E. to S. W., also from S. E. to N. W., or *seawards* as respects the coast of the ancient Appalachian ocean. In so many places, both in the mountains and the great plains of the Western States, are the shore and the deep sea deposits of that ancient ocean lifted to the surface by artificial flexures, so entirely in each of its many successive flows with the whole of the once living tribes that they supported, brought into contact with our hands for exact measurement, within reach of our closest vision for minute inspection, or even microscopic analysis, that the systematic and patient study of its contents, its sediments and organic fossils, now in progress, cannot fail to reward us with a full disclosure of all that was striking in its history.

Let us now turn to the progress recently made in developing the palæontology of this great Appalachian basin, in classifying and naming the formations, and in determining the relationship in age between these and the palæozoic strata of Europe. By the researches of Mr. Conrad, Prof. Emmons, and Mr. Hall, in New York, and of Hall, Owen, Troost, Locke, and Clapp, in the Western States, we are already made familiar with the forms and the positions in the strata of perhaps five hundred well characterized marine fossils, embraced in numerous genera of trilobites, testacea, and corals. The labors of Hall and Conrad, in bringing to light the chief part of these remains, have been especially valuable. As Mr. Hall is devoting himself with great zeal and signal success, to the fascinating study of the organic remains of New York, and as the geologists of other States are availing themselves of its clearly defined specimens for those comparisons, without which little real progress can be achieved in the more refined enquiries connected with our palæontology, we may anticipate the speedy development of highly curious generalizations, in relation to the limits of these extinct races, and the conditions which have controlled their distribution.

Guided by the ascertained boundaries of the principal groups of fossils, and also influenced by the great natural lithological horizons, the geologists in the different quarters of the wide Appalachian field have sedulously aimed at making such an analysis and classification of the strata as would best accord with the special type which they present in their respective district. Seeking at first for a classification founded on local and not general characteristics, the subdivisions hitherto instituted only admit of extension to districts beyond those for which they were framed, in so far, as the strata retain with more than usual constancy these local features. Hence a general scheme of grouping, applicable, if possible, to the whole region occupied by the strata, and expressed in terms significant of general and not local and restricted relations, is yet to be supplied.

The most elaborate classification of our Appalachian palæozoic strata, hitherto published, is that of the New York geological survey. From the very considerable amount of palæontological research connected with this survey, from the diversity of formations in the State, and the clear typical characters which some of them possess, this classification merits much attention. It embraces under the title of the New York system, the entire body of strata from the bottom of the lowest fossiliferous rocks, to the base of the red sandstones of the Catskill Mountains, the whole having in their maximum expansion a thickness of about six thousand feet. This large mineral mass has been subdivided by the gentlemen of the New York survey in conformity, chiefly, to the horizons established by organic remains into twenty eight special formations, or subordinate masses, and these twenty eight are thrown into four series or divisions named from the districts where they are best developed. Observing the ascending order, these are the Champlain division, the Ontario division, the Helderberg division, and the Erie division. Referring you to the ample and well illustrated volumes on the geology of New York, for the views which have induced the gentlemen of that survey to adopt the above classification and nomenclature of the rocks, and for many valuable details connected with the organic remains, I will embrace this opportunity to bring to your attention some points of general interest to American geology, which the perusal of these works suggests.

In geological, as in all archæological research, the earliest periods seem most to enlist our attention. Ascending the stream of time, the structures we *first* meet with have too much the features of the present day deeply to move our minds; but far towards its sources, on the very confines of the great desert of the forgotten past, monuments are found, that, by their strange outlines and dim inscriptions, calling conjecture to the aid of thought, stir in us a curiosity which belongs to the profoundest part of our intellectual and moral nature. The whole history of geology is the record of a series of successive incursions into remoter and remoter provinces of time. Pushing research to its limits, we seem nearly to have reached the bounds of the accessible past, in the diminution, and at last the total disappearance of fossils in the earlier strata. Our science has reached the point just attained by geography. No conjectural continent is left for a geological Columbus to discover —no great region remains unvisited, and no principal boundary undrawn. The business of the present and future generations of geologists, is to establish with all the precision admissible, by science, the exact limits which divide the many districts of ancient time into which they have penetrated; to define the position, so to speak, of each known coast, and to bring to light such lesser districts as may yet lie undiscovered within their more conspicuous borders. This work, more difficult by far than that of mere first discovery, since it demands a very thorough knowledge both of palæontology and of structural geology, is advancing at this time with extraordinary speed in Europe, in the hands especially of Phillips and De la Beche, Murchison and Sedgwick, De Beaumont and D'Orbigny.

Let us inquire how far we in the United States have proceeded in the same labor, of firmly establishing some of the more important limits between the several portions of geological time, as recorded by our strata and their organic remains. And first, let us examine the conclusions reached regarding the commencement or dawn of the whole fossiliferous period. The fixing of a base for the palæozoic rocks of the United States, is a problem scarcely less difficult than that of determining the lower limit of the corresponding system in England, to which the admirable sagacity of Sedgwick has been so usefully directed. Do we possess in the so called *Taconic system* of rocks lying to the southeast of the unequivocally fossiliferous strata at the base of the New York or Appalachian system, an independent mass of formations, of an unquestionably earlier date, or are these on the other hand, but well known lower Appalachian strata, disguised by some change of mineral type, and by igneous metamorphosis. These Taconic rocks under the form they assume along the eastern boundary of New York, and the western side of Vermont and Massachusetts, have been carefully studied by Emmons, Hitchcock and Mather, all of whom appear to have arrived at different conclusions concerning them. Since the same or a very analogous group of strata ranges at intervals, holding the same relative position, the whole distance from Vermont to Georgia, the question of their age, while it has a wide bearing on any general classification of our formations, ought certainly to admit, sooner or later, of settlement, when so many and such noble transverse sections are opened to inspection by the river gorges which cut the Blue Ridge.

Prof. Emmons considers the granular quartz, slate and limestone of the Taconic hills and the Stockbridge valley, as constituting a distinct group of strata, neither appertaining to the true gneissoid or mica schist system on the east, nor to the palæozoic fossiliferous rocks of the Champlain and Hudson valley on the west, but holding an intermediate place in the scale of time. His principal argument in defence of this view, is that the order of succession of the component members of the group, is essentially different from that witnessed in the sandstone, limestone and slate of the Champlain division, and he denies that the theory of plication of the beds, advanced originally by myself and my brother, and applied to this very region, can reconcile the seeming want of agreement. Now it is true that the *apparent* order of superposition in the Taconic belt, is in discrepancy with the well known succession of the Champlain formations, but this is precisely what should arise from the introduction of those complete folds or doublings together of the strata which we have conceived to exist; and I would add that the sections furnished by Prof. Emmons and Prof. Mather in their reports, if resolved by the introduction of the flexures supposed by us, will all of them display, for their western portions at least, the normal order of superposition of the Champlain rocks. This identity of the so named Taconic system, with the formations of the Hudson and Champlain valley, was announced by my brother and myself, in the beginning of 1841, to the American Philosophical Society. By aid of a section from Stockbridge towards the Hudson river, we showed the existence of numerous close anticlinal and synclinal folds, and thus explained the apparent inversion of the dip, which other geologists had ascribed to one general overturning of the whole series. The plication was shown to be greater along the Berkshire valley and the ridges east, the granular Berkshire marble was identified with the blue limestone of the Hudson valley, but metamorphosed by heat, and the associated micaceous, talcose, and other schists were referred in the language of the communication, to the slates of the lowest formation of the Appalachian system, while the semivitrified quartz rock of the western part of the Hoosac mountains was stated to be nothing else than the white sandstone (Potsdam sandstone) of the same series slightly altered.* I am gratified to find from Prof. Mather's report, that these views of identity are embraced by him, as they now are, if I mistake not, by Prof. Hitchcock. Prof. Mather indeed says that he has traced the slate (Hudson slate) through all its gradations into talco-argillaceous and talcy slate, and into graphic and plumbaginous slate, the limestone from compact sandy and slaty, to sparry, slaty talcose, and crystalline limestone, within short distances, and the Potsdam sandstone, to a hard compact and granular quartz rock. It is true, Prof. Emmons has presented in his report, a series of sections of the strata exhibiting an unconformity at the passage of his Taconic into the rocks of the Champlain division, but I must take the liberty of expressing my disbelief of the existence of any such unconformity, and of observing that in the prolongation southwestward of this altered and plicated belt as far as the termination of the Blue Ridge in Georgia, a distance of one thousand miles, no interruption of the general conformity of the strata, has ever met the observations of my brother or myself.

It would appear thus that the Potsdam sandstone forms the base of the palæozoic strata in the latitude of Lake Champlain, or at least in the region of the lake and of the Mohawk river. Is this formation then the lowest limit of our Appalachian palæozoic masses generally, or is it the system expanded downwards in other districts, by the introduction beneath of other conformable sedimentary rocks? From the Susquehanna River southwestward, a much more complex series of strata comes in below the bottom of the lowest limestone, than is any where seen north east of the Schuylkill. In some portions of the Blue Ridge belt there are at least four independent and often very thick deposits, constituting one general group, in which the Potsdam, a white sandstone, is the second in descending order. The uppermost of these is an arenaceous and ferriferous slate, many hundred feet thick, in which the only fossil is a peculiar fucoid. Beneath this lies the Potsdam sandstone, and under this again a mass of coarse sandy slate and flaggy sandstone, amounting sometimes to six hundred or seven hundred feet, below which occurs in Virginia and east Tennessee a series of heterogeneous conglomerates. Neither of the two lowest of these masses, has yet rewarded research with a single fossil, and therefore the white or Potsdam sandstone is yet

* See Proceedings of American Philosophical Society, Jan. 1, 1841.

the most ancient depository of organic life hitherto discovered in our strata. Adjoining this great mass of arenaceous strata towards the southeast, we find throughout much of the broad belt of the Blue Ridge, especially in its prolongation southwestward from the Potomac, a wide expansion of metamorphic strata intersected by innumerable veins and dykes of greenstone and other igneous materials, and displaying almost every grade and variety of alteration in texture and mineral contents. These after long and careful observation, we have been led to consider as a group of sedimentary beds still older than the preceding; but forming a part of one and the same unbroken series. Thus then in the great group of strata, at the base of our lowest fossiliferous series we are presented with similar and perhaps more striking results of igneous modifying powers than even in that portion of the Champlain system whose metamorphosed beds constitute the Taconic group. Although no relics of either vegetable or animal life have hitherto been met with in this group, we cannot confidently infer their entire absence, since from the effects of cleavage and chemical change their remains could not fail to be greatly obscured and for the most part quite obliterated. It is most probable, however, that the same barrenness of fossils remarked in the slates and sandstones immediately beneath the Potsdam sandstone, prevails throughout the whole of the continuous series of subjacent strata.

Respecting the phenomena presented in the long belt of rocks here referred to, the question suggests itself whether the so called Taconic system, instead of belonging exclusively to the Champlain division, may not along the western border of Vermont and Massachusetts, include also some of the sandy and slaty strata here spoken of as lying beneath the Potsdam sandstone.

Applying for the present the term palæozoic only to the strata commencing in the ascending order with the conglomerates at the base of the group, including the Potsdam sandstone, since below this there is little probability of our finding traces of organic beings, the next inquiry of general interest relates to the natural divisions, groups and formations, into which we should arrange the whole enormous body of sedimentary deposits, between this horizon and the top of the Coal rocks. Throughout this great mass of strata, whose aggregate thickness exceeds in some districts thirty thousand feet, made up of an extraordinary number of distinct formations, characterized by peculiar organic remains and composition, and marking a long series of events and a vast lapse of time, we behold one uninterrupted succession of deposits, closely linked by an equally unbroken sequence of animal and vegetable remains.

We are therefore constrained to view the whole as constituting a single system, the entire record of one immense continuous period, the collected gatherings of one prodigious sea. To deduce from a study of the organic remains of the different portions of this mass, aided by considerations of mineral type, a classification which shall be in harmony with the natural relationships of the different members throughout the entire basin, as to time and circumstances of origin, one which, in other words shall express the various epochs and changes in their relative importance; and to clothe this classification in language which shall be at once suggestive of their relationships and generally applicable, furnishes a complex problem of much difficulty, but one perhaps which at the present time has strong claims to the attention of American geologists.

As the necessity for a general nomenclature for these rocks, founded on a wide survey of their fossils, is getting to be recognized, as many and grave objections exist to the adoption of all local and partial classifications, and as it is believed that through the diligence of our geologists, we are already prepared with a sufficiently ample body of data for the construction of a system which shall unite the chief requisites here mentioned, I venture, craving the indulgence of the Association, to present the outlines of a scheme of grouping and naming our Palæozoic strata, which my brother Prof. W. B. Rogers and myself, have been carefully maturing during the last three years.

We propose to distribute the whole great body of strata from the base already designated to the top of the coal measures in *nine* distinct *series*, the products of as many great successive *periods*, and resorting to the analogy between these periods and the nine natural intervals into which the *day* is conveniently divided, we have named them in ascending order, the *primal*, *matinal*, *levant*, *premedidial*, *medidial*, *postmedidial*, *ponent*, *vespertine* and *seral* series, the deposits of the dawn, morning, sunrise, forenoon, afternoon, sunset, evening and twilight periods of the great Appalachian Palæozoic day. Subdividing each *series* in obedience to natural and obvious relations of the organic remains and mineral boundaries, we have named each ultimate subdivision or *formation*, calling the time during which each formation was produced an *epoch*, and between the series and formations, we have constructed *groups* in all cases where the natural affinities of the formations require that two or more of these latter shall be united into associations subordinate to the series.

Our *Primal series* embraces the four great rocks between the base of the Palæozoic strata and the base of the first limestone, the calciferous sandstone of New York. Of these the primal white sandstone, would seem to be the only formation existing in New York, or according to Owen, on the northwestern margin of the basin in the western states.

The *Matinal series* includes all the strata from the horizon of the base of the calciferous sandstone, to that which marks the top of the Hudson River slate in New York, and the top of the blue limestone of the western states. This series in southwestern Virginia and East Tennessee, embraces a thick and important middle group consisting of three formations, not extending northeast of the New River, and only imperfectly represented in some portions of the western states.

The *Levant series* includes all the formations between the horizon terminating the Matinal rocks and one running through the top of the water lime formation of New York, the top of the non-fossiliferous and "pitted rock" of Lake Huron, and through a plain low in the cliff limestone of the western states. It takes in, therefore, the Medina, Clinton, Niagara and Onondaga salt groups, and water lime of the New York survey.

The *Premedidial series* embraces the strata between the top of the *water lime* and the top of the *Oriskany sandstone* of New York, and includes, therefore, the *Pentamerus* and *Catskill* shaly limestones of that State as its oldest formation, and the Oriskany Sandstone as its newest; and besides these, a middle formation not there seen but well developed in Pennsylvania, with characteristic fossils.

The *Medidial series* ranks in it all the strata between the top of the Oriskany or Premedidial sandstone and the *Marcellus black* slate of New York, or the black bituminous slate of Ohio, Indiana, Kentucky and Middle Tennessee. It, therefore, includes the *Schoharie grit* and *Onondaga* and *corniferous* limestones of New York, and the upper division of the cliff limestone of the west.

The *Postmedidial series* embraces that very natural assemblage of formations commencing with the black slate just named, and crossing with the horizon which marks the base of the *Catskill red sandstone.* It contains, therefore, for New York the Marcellus shades, the Hamilton group, the Tully limestone, the Genesee slate, the Portage group and the Chemung group, and for the west all the strata between the top of the cliff limestone and the bottom of the carboniferous limestone.

The *Ponent series* includes all the rocks between the base of the Catskill red sandstone and the top of the overlying conglomerate. (Formation X, of the Pennsylvania and Virginia annual Reports.) It usually embraces but two formations, the Ponent red sandstone and the Ponent conglomerate, though the former of these requires for some districts a triple subdivision.

The *Vespertine series* comprehends the interesting formations above the horizon of the Ponent conglomerate, and below that at the base of the great conglomerate under the coal measures. In Pennsylvania it is composed of the thick red shale deposit of the coal regions, and in Virginia of a much more complex set of strata, including a lower red shale or variegated marl, next a great thickness of carboniferous limestone, and then an upper set of shales with alternating sandstones. In the western states, on the other hand, it consists almost exclusively of the carboniferous limestone and its subordinate chert.

The *Seral series* embraces one vast and multiform body of coal strata, the thickness of which in Western Pennsylvania and Virginia exceeds three thousand feet, being in the anthracite basins probably still greater. The lowest or oldest subdivision of this series is the Seral conglomerate, and the true coal formation overlying this is divided into four distinct members—the *older coal measures*, *older shales*, *new coal measures* and *new shales;* these last terminating the entire succession of one thick and wide-spread Appalachian strata.

The whole body of rocks here grouped into nine series, contains upon the most careful analysis which we have been able thus far to institute, about forty eight formations, few if any of which are co-extensive with the present limits of the great Palæozoic basin in which they lie, or even with that part of it included between the Blue Ridge chain, the Mississippi River, and the great Lakes. Those which were most widely deposited are the Matinal magnesian limestone, the Levant older (or Niagara) limestone, the Vespertine (or carboniferous) limestone, and the older coal measures. Others occupy a relatively circumscribed area, yet none are called formations which are not the products of distinct formative actions operating during epochs characterized by distinct groups of races.

My brief limits will not allow me to present here even the general scheme of names by which we propose to designate the divisions of this extensive system of strata; but I will explain succinctly the principles upon which the names are chosen. The title given to any formation is composed first, of the name of the *period* to which it appertains, and secondly, of a word or words descriptive of the *ruling mineral character* of the rock; and to these is appended, when we wish to specify the type under which the formation is referred to, the name of the district or place where it is so developed. Let me exemplify this by one or two instances. The well characterized formation called in the New York survey the Marcellus shales, is named by us the *Post-medidial older black slate*, while the Genesee slate is called *Postmedidial newer black slate*, and a member of the Clinton group of New York, occuring there as a thin bed of brown and ponderous sandstone, (seen on the Sequoit,) but expanded in Pennsylvania and Virginia into an important mass having characteristic fossils and a maximum thickness of two hundred feet, we propose to call the *Levant iron sandstone.*

The *nomenclature* here employed for the designation of the strata, is recommended we conceive by several features of obvious utility. Being a nomenclature based on considerations of geological time, it suggests at once in the names themselves the relative *ages* of the different strata, thus defining the fundamental relationship of *succession in time*, the only relationship between rocks which never varies. It has thus the advantages of a numerical or *ordinal* designation, combined with the descriptiveness which that has not. While it conveys the fixed relations of time, it expresses the accidental or local character of each formation, and what is of much more importance, it signifies under what particular type any special stratum is referred to, by introducing into the name that of the district where the rock assumes the phases treated of. Thus by the mere name assigned to each formation we are reminded of all its most essential attributes, its *age*, its *region*, and its *mineral composition;* or in other words, what place among the other strata it occupies—in what district we are describing it, and what its composition is under its normal or typical development. One of the most obvious defects in any nomenclature is a want to ready adaptibility to new or abnormal relations and conditions of the objects named, and in this unfortunate rigidity in the terminology of some of the sciences we may discern a most influential barrier to their progress. In divising the system proposed, we have aimed at uniting the power of representing the fundamental or permanent relations of the objects named, and of pliantly expressing their special deviations and gradations. In the phenomena of mineral and Palæontological deviations of type lie concealed, we should remember, the very *arcana* of our science, secrets which interpreted will give us the only insight we can hope ever to procure into the actual state of our earth's surface in periods long remote. In the above system we try to give to geological language the capacity of expressing the facts and laws of gradations in strata which are so constantly appearing.

As an illustration of the necessity for some such general applicable scheme for our Palæozoic rocks, it may be mentioned, that while some of these are remarkable for adherence to their typical characters, others are conspicuous for their protean variations. The value of our particular mode of designation, may be seen in the instance of the group of formations we have called the Matinal series. Being remarkably well separated by great natural features from the Primal series below, and from the Levant series above, from both of which it is insulated as well by clear mineral characters as by dissimilarity of organic remains, it yet exhibits within itself, when widely traced, several important modifications of type, which no geographical or other artificial nomenclature can possible indicate or express. Thus it consists in New York of five rather well defined formations, the calciferous sandstone, black limestone, Trenton limestone, Utica slate and Hudson river group, each possessing an easily distinguishable mineral aspect, and its own organic remains with little intermixture. But how different is the condition of the whole mass in Ohio and Kentucky, and again how different in southwestern Virginia and East Tennessee. In the anticlinal district of Cincinnati and Lexington, where the series bears the name of the blue limestone, not one of the special subdivisions or formations, so easily recognized on the Mohawk, can be distinguished, but the whole displays a fusion or blending of the different portions, and also of the mineral materials which baffles every attempt at tracing an exact equivalency between any of the parts and the New York formations. It is true the investigations of Prof. Locke, Dr. Owen and other geologists, show that certain species observe certain horizons, but it should be observed that these horizons in many cases, do not bear to each other the same relations which those of the same fossils do on the Mohawk. In place of the five rather clearly marked masses of New York, the whole exposed portion in Ohio, is about one thousand feet in thickness, composed of attenuating calcareous shales and thin layers of limestone.

Turning to Virginia and East Tennessee, we have a still different state of things; the Mohawk type, with no very important modifications, continues from New York through New Jersey, Pennsylvania, and Maryland, extending into Virginia as far as the north branch of the Shenandoah, some seventy or eighty miles southwest of the Potomac. The principal change in this distance is in the Black

River limestone, which expands greatly in thickness, becomes more highly magnesian, and acquires some fossils not seen to the northeast. But beyond the neighborhood of New Market in Virginia, the changes are much more important; for with the magnesian limestone is associated a vast body of chert, and the Utica slate as a stratum disappears; the place of that rock and the Trenton limestone and Hudson River slate, being occupied somewhat as in Ohio, with a blended calcareo-argillaceous mass, in which neither formation is distinctly recognized, and where the fossils of each are to a considerable extent intermingled; the *Lingula rectilateris*, for example, deemed characteristic of the Utica slate in New York, being found throughout all the middle and southwestern region of Virginia associated with the Cypricardiæ, so distinctive of the Hudson River group, and always very high in the series. Entering East Tennessee, but especially in the region of Knoxville, the middle part of the series represented by the Trenton limestone and Utica slate of New York and Pennsylvania, and by the simple attenuation of layers of limestone and slate with Trenton, Utica, and some Hudson slate fossils at Cincinnati and in the interior of Virginia, becomes a complex group of several distinct formations no where developed farther north.

Thus the entire Matinal series in East Tennessee embraces in the ascending order, 1st, a great magnesian limestone between two thousand and three thousand feet thick, with high beds of chert, often oolitic, and layers of white and even red sandstone; 2d, a knotty argillaceous limestone containing Maclurea, Isotelus, and a great profusion of Calamapora, Polymorpha, and other corals, seven hundred feet thick; 3d, a white encrinal sparry limestone five hundred feet; 4th, yellowish calcareous slate several hundred feet; 5th, sandy gray limestone very ferruginous, three hundred feet? 6th, yellowish or buff slates several hundred feet; 7th, red and greenish coralline and encrinal marble four hundred? feet; 8th, calcareous gray and yellowish slates several hundred feet.

Upon comparing these Matinal rocks of East Tennessee, with those of the northeastern portions of the basin, the difficulty of establishing an equivalency in the ordinary sense of the term, will be found to be insuperable; for while the Tennessee strata in the middle part of the column, possess a number of well known Trenton limestone fossils, the mingling of these with Hudson slate species, the total disappearence of others, and the introduction of new forms, preclude every attempt at *identification.*

In a case of this kind a nomenclature on the plan here proposed, will be found to obviate all confusion. When speaking of the strata, as they exist in New York, we would call the Trenton limestone and Utica slate, for example, Matinal newer limestone, and Matinal older slate, but when these become blended, as in Virginia, the portion of the series which they occupy, the Matinal argillaceous limestone group, or Matinal middle group, while the strata on the same general horizon in East Tennessee, would be called the Matinal encrinal limestone, or Matinal coral marble, as the case might be, appending when necessary to each such name, that of the district in which the rock or group attains its fullest development. In those instances where any thing short of a minute analysis of local application, is impracticable, as is the case perhaps of the Cincinnati blue limestone, the general term *Matinal series* will be most appropriate. If that mass should in any district acquire a binary or ternary subdivision, the respective parts may be called the Matinal older limestone, &c., of Ohio, a mode of naming which is amply descriptive as to each requisite of time or plan in the series, of composition and locality, and which furthermore takes nothing for granted.

Before leaving this important but difficult subject of classification and nomenclature, permit me very briefly to indicate why I conceive that the prevailing method of naming rocks without regard to their relationships of age, but solely by titles drawn from the spots or districts where they occur, must seriously retard our science. If the names be derived from a single district, even an extensive one like New York, however rich in strata and however ably these should be explored, only a portion of the names, generally a small part of them, will really be assigned to formations which are there fully or typically developed, for we well know, that no single corner of a vast basin can ever contain but a few of its deposits in a condition of maximum expansion, and richness in organic remains. If geographical terms are to be employed it would be more appropriate to select those belonging to districts any where in the basin where the formations are found to be most amply represented. On this plan the names would be found to possess a wider significance, than when all are chosen from one region, yet as geographical names, they would lack suggestiveness when applied to any one local district. If on the other hand, the rocks are to receive different local titles in the different territories explored, then will our descriptive geology be rendered almost unintelligible, by a crowd of synonyms, and every attempt at extensive comparison, every effort to read in this ample page of nature's great history, some of her higher laws, will be made doubly arduous.

To be concluded in our next No.

Art. XI.—*Remarks on the Gulf Stream and Currents of the Sea;* by Lieut. M. F. Maury, U. S. N.*

[Lieut. Maury introduces his paper by stating that he has no intention of recapitulating the little that we know with regard to the Gulf Stream, and of its currents; sometimes conflicting—sometimes colder and at other times warmer than the seas in which they are found.

That we should obtain a better knowledge of these currents and of the laws that govern them, is equally interesting to the navigator and to the philanthropist, and it is therefore recommended to the National Institute, "to devise and set on foot a plan for multiplying observations and extending our information upon these interesting phenomena."—Eds.]

The shoals which endanger navigation off the capes of Carolina, appear to owe their existence entirely to currents. The soundings and form of the Hatteras and other shoals, clearly indicate that they are caused by a current from the north. A comparison of present charts with Jeffry's Atlas published in 1775, shows not only that these shoals are increasing, but that the chain of islands alluded to is in process of gradual formation. Currituck and Roanoke Inlets which are now sand bars, were once navigable; Ocrocoke Inlet had then seventeen feet of water; it now has eight. Besides these, there were, between Beaufort, N. Carolina and Charleston S. Carolina, twenty five or thirty others, many of them then navigable, and most of them now closed and appearing only as dry land.

Whence comes the sand that forms these islands? Separated from the main land by standing pools of water, moved only by the tides from the ocean, it cannot be brought from the shore. It can only be upheaved by geological agencies, with the general elevation of the coast, or it is cast up from the bottom of the ocean by the Gulf Stream and the waves, or brought down from the north by the current on the coast. Investigation might settle the question. If marine currents considered as physical agents, are interesting to the geologist, they are far more so to the navigator; but their force, their fluctuations and their general laws

* Condensed from a paper read before the National Institute, Tuesday, April 2d, 1844.

are so far unknown, that we search for them in vain in our nautical books or in the history of navigation.

Proceeding into the Atlantic we find a vast stream of warm water running northeast from the straits of Florida to the Banks of Newfoundland, and thence to the shores of Europe. What are its breadth and its depth we know not. We are told indeed that even at the same place, it sometimes runs at the rate of two knots the hour and sometimes at five, and we know that it may be always found within certain broad limits, varying in this too at the same place, from one hundred and forty to three hundred and forty miles. With this, our knowledge of it ends; although more accurate information as to it and its offsets would have saved many shipwrecks, and would contribute not a little to the speedy and safe navigation of the Atlantic.

Though navigators had been in the habit of crossing and recrossing the Gulf Stream almost daily for the space of near three hundred years, its existence even, was not generally known among them, until after Dr. Franklin discovered the warmth of its waters, about seventy years ago;—and to this day, the information which he gave us, constitutes the basis—I had almost said, the sum and substance—of all we know about it.

When he was in London in 1770, he happened to be consulted as to a memorial which the Board of Customs at Boston sent to the Lords of the Treasury, stating that the Falmouth packets were generally a fortnight longer to New York, than common traders were from London to Providence, R. I. They therefore asked that the Falmouth packets might be sent to Providence instead of New York. This appeared strange to the doctor, for London was much farther than Falmouth; and from Falmouth, the routes were the same, and the difference should have been the other way. He however consulted a Nantucket whaler, who chanced to be in London also, who explained to him that the difference arose from the circumstance that the Rhode Island captains were acquainted with the Gulf Stream, while those of the English packets were not. The latter kept in it, and were set back sixty or seventy miles a day, while the former avoided it altogether. He had been made acquainted with it by the whales, which were found on either side of it, but never in it. At the request of the doctor he then traced on a chart the course of this stream from the Straits of Florida. The doctor had it engraved at Tower Hill, and sent copies of it to the Falmouth captains, who paid no attention to it. The course of the Gulf Stream as laid down by that fisherman, from general recollection, is retained on our charts at the present day, almost without an alteration.

If philosophy can vindicate a claim to property in discovery, the present case is of that description. A river in the ocean! American in its source and origin, it is found in the waters of America, and closely concerns its navigating interests; first discovered by the fishermen of New England, it attracted the attention of the great American philosopher; he determined its most remarkable characteristic; and he left it to his countrymen as a field to be reoccupied by them at some future day, and with a like spirit of philosophical research.

Therefore the Gulf Stream offers a field of investigation peculiarly American, and we the Americans are in duty as we are in honor bound to show ourselves curious as well as diligent in whatever there may be about it of undiscovered mystery or of philosophic interest to navigation.

To the philosopher, every newly discovered fact in physics, however trifling it may seem to others, is a gem. Our knowledge of nature and her laws, is only a collection of such facts, grouped together and made the basis of induction. As they accumulate, they reflect light upon each other, and each generation becomes wiser and wiser; for every fact in physics is but another clew placed in our hands, which, if carefully followed up, will lead us farther and farther out of the labyrinths of ignorance, and bring us nearer and nearer to the doors of knowledge.

Dr. Franklin's discovery of the Gulf Stream temperature was looked upon as one of great importance, not only on account of its affording to the frosted mariner in winter a convenient refuge from the snow storm, but because of its furnishing the navigator with an excellent landmark or beacon for our coast, in all varieties of weather. It was at that time, not uncommon for vessels to be as much as 10° out in their reckoning. He himself was 5°. Therefore in approaching the coast, the current of warm water in the Gulf Stream, and of cold water on this side of it, if tried with the thermometer, would enable the mariner to judge with great certainty, and in the worst of weather, as to his position.

Jonathan Williams afterwards, in speaking of the importance which the discovery of these warm and cold currents would prove to navigation, pertinently asked the question, "If these stripes of water had been distinguished by the colors of red, white and blue, could they be more distinctly discoverable than they are by the constant use of the thermometer?" And he might have added, could they have marked the position of the ship more clearly?

The manner in which the Florida Straits open out into the Atlantic, inclining to the westward of north—the contour of the adjacent coast, the deep indent in the shore here, between St. Augustine and Savannah—all seem to indicate a close pressure of the Gulf Stream, and its counter current upon this part of the coast.* If so, the course of the Gulf Stream as it disembogues in the Atlantic, must be a little to the west, instead of a little to the east of north, as our charts represent it. My own information, derived from the observations of an intelligent brother officer, goes to confirm this opinion; should it be proved correct, it will explain the anomaly often remarked upon by navigators with regard to a stronger current in the Gulf Stream off Cape Hatteras than farther south; for then this circumstance may be accounted for by the chart course leading them on the outer edge of the Gulf Stream until it reaches the Carolina capes, where they again get into the strength of it.

The Gulf Stream, as it issues from the Straits of Florida, is of a dark indigo blue; the line of junction between it and the green waters of the Atlantic, is plainly seen for hundreds of miles. Though this line is finally lost to the eye as the stream goes north, it is preserved to the thermometer several thousand miles; yet to this day, the limits of the Gulf Stream, even in the most frequented parts of the ocean, though so plainly marked, are but vaguely described on the charts. Thousands of vessels cross it every year; many of them make their observations upon it, and many more, if invited, would do the same. But no one has invited coöperation, consequently there is no system, and each one who observes, observes only for himself; and when he

* This opinion is still further confirmed by Mr. Hodgson in his paper upon the megatheroidal fossils of the coast of Georgia, in which he states that the islands on that coast bear marks of abrasion from the sea.

quits the sea, his observations go with him, and are to the world as though they had not been.

Capt. Manderson, of the royal navy, published many years ago "An examination into the true course of the Florida Stream," which he ascribed to the Mississippi and the floods of the other rivers emptying into the Gulf. But judging from what we see daily going on in the Mediterranean, the waters from the rivers, especially in summer, when they are at low stages, and when the Gulf Stream runs at the greatest velocity, are not sufficient to supply the water of evaporation. Taking the hypothesis of the English officer for granted, it was asserted by another writer that the velocity of the Gulf Stream might be determined by the freshets in the Mississippi. Capt. Livingston put these theories at rest by showing that the volume of water discharged through the Gulf Stream exceeds what is emptied from the Mississippi by more than three thousand times.

Upon the ruins of this hypothesis, which Capt. Livingston so completely overturned, he advanced the opinion that the velocity of the Gulf Stream "depends on the motion of the sun in the ecliptic, and the influence he has upon the waters of the Atlantic." To this day our books on navigation quote this opinion without comment. The most generally received opinion, however, is that the Gulf Stream is caused by the trade winds. This, doubtless, is one of the causes; but is it of itself adequate to such an effect? To my mind, the dynamical laws of the ocean, as at present expounded, appear by no means to warrant the conclusion that it is, unless the aid of other agents is also brought to bear. We know of instances in which the water has been accumulated by the force of winds on one side of a lake, or in one end of a canal at the expense of the other; but they are rare, and the effect of violent, sudden, and particular causes, such as the trades never afford. And this piling up is only known to take place in *shallow* basins, or against *strong* currents.

Supposing the pressure of the waters that are *forced* into the Caribbean Sea by the trade winds to be the *sole* cause of the Gulf Stream, that sea and the Mexican Gulf should have a much higher level than the Atlantic. Accordingly the advocates of this theory require for its support "a great degree of elevation." Major Rennell likens the stream to "an immense river descending from a higher level into a plain." Now, we know very nearly the average surface breadth and velocity of the Gulf Stream in the Florida Pass. We also know with a like degree of approximation the velocity and breadth of the same waters off Cape Hatteras. Their breadth here is about seventy five miles against thirty two in the Narrows of the straits, and their mean velocity is three knots off Hatteras against four in the Narrows. This being the case, it is easy to show that the depth of the Gulf Stream off Hatteras is not so great as it is in the Narrows of Bemini by nearly one half; and that consequently, instead of *descending*, its bed represents the surface of an inclined plane from the north, *up* which the lower depths of the stream *must* ascend. If we assume its depth off Bemini to be two hundred fathoms, (which are thought within limits,) the above rates of breadth and velocity will give one hundred and fourteen fathoms for its depth off Hatteras. The waters, therefore, which in the straits are below the level of the Hatteras depth, so far from descending, are actually forced up an inclined plane, whose submarine ascent is not less than ten inches to the mile.

The Niagara is "an immense river descending into a plain;" but, instead of preserving its character in Lake Ontario as a distinct and well defined stream for several hundred miles, it spreads itself out, and its waters are immediately lost in those of the lake. Why should not the Gulf Stream do the same? It gradually enlarges itself, it is true; but instead of mingling with the ocean by broad spreading as the "immense rivers" descending into the northern lakes do, its waters, like a stream of oil in the ocean, preserve their distinctive character for more than three thousand miles. Moreover, whilst the Gulf Stream is running to the north from its supposed elevated level at the south, there is a cold current coming down from the north with great velocity. Meeting the warm waters of the Gulf midway in the ocean, it divides and runs by the side of them right back into those very reservoirs at the south, to which theory gives an elevation sufficient to send out entirely across the Atlantic a jet of warm water said to be more than three thousand times greater than the Mississippi River!

Now this current from Baffin's Bay has not only no trade winds to give it a head, but for a great part of the way it is below the surface and far beyond the propelling reach of any wind. And there is every reason to believe that this polar current is quite equal in volume to the Gulf Stream. Are they not the effects of like causes? If so, what have the trade winds to do with one more than the other?

It is a custom often practised by seafaring people to throw bottles overboard with a paper stating the time and place at which it is done. In the absence of other information as to currents, that afforded by these mute little navigators is of great value. They leave no tracks behind them, it is true, and their routes cannot be ascertained. But knowing where they were cast, and seeing where they are found, some idea may be formed as to their course. Straight lines may at least be drawn, showing the shortest distance from the beginning to the end of their voyage, with the time elapsed.

I have a chart representing in this way the tracks of more than one hundred bottles.* From it, it appears that the waters from every part of the Atlantic tend towards the Gulf of Mexico and its stream. Bottles cast into the sea midway between the old and new worlds, near the coasts of Europe, Africa, and America, at the extreme north and farthest south, have been found either in the West Indies or within the well known range of the Gulf Stream.

Midway of the Atlantic, in the triangular space between the Azores, Canaries, and the Cape de Verd Islands, is the Sargasso Sea, covering an area equal in extent to the Mississippi valley. It is so thickly matted over with gulf-weed, (*Fucus natans*,) that the speed of vessels passing through it is often much retarded. When the companions of Columbus saw it, they thought it marked the limits of navigation, and became alarmed. Patches of the weed are always to be seen floating along the Gulf Stream. Now if bits of cork, or chaff, or any floating bodies be put in a basin, and a circular motion be given to the water, all the light substances will be found crowding together near the centre of the pool, where there is the least motion. Just such a basin is the Atlantic Ocean to the Gulf Stream, and the Sargasso Sea is the centre of the whirl. Certain observations as to its limits, extending back for fifty years, assure us that its position has not

* Of many thousands that have been cast into the sea, these are all that have been found and recorded.

been altered since that time. This indication of a circular motion by the Gulf Stream is corroborated by the bottle chart and other sources of information. If therefore this be so, why give the endless current a higher level in one part of its course than another?

Nay, more; at the very season of the year when the Gulf Stream is rushing in greatest volume through the Straits of Florida and hastening to the north with the greatest rapidity, there is a cold stream from Baffin's Bay, Labrador, and the coasts of the north, running to the south with equal velocity. The two meet off the Grand Banks, where the latter is divided, one part of it runs under the Gulf Stream, as is shown by the icebergs which are carried in a direction tending across its course. The probability is that this "fork" continues on towards the south, and runs into the Caribbean Sea; for the temperature of the water at a little depth there has been found far below the mean temperature of the earth, and quite as cold as at a corresponding depth off the arctic shores of Spitzbergen. The other fork runs between us and the Gulf Stream as already described. As far as it has been traced, it warrants the belief that it too runs *up* to seek the so called *higher* level of the Mexican Gulf.*

The power necessary to overcome the resistance opposed to such a body of water as that of the Gulf Stream running several thousand miles, without any renewal of impulse from the forces of gravitation or any other known cause, is truly astonishing. It so happens that we have an argument for determining this resistance towards the east with a degree of accuracy. Owing to the diurnal rotation, the waters of this stream are carried round with the earth on its axis, *towards the east*, with an hourly velocity one hundred and fifty seven† miles greater when they enter the Atlantic, than when they arrive off the banks of Newfoundland. In consequence of the difference of latitude between the

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* More water cannot run from the equator or the pole than to it. If we make the trade winds cause the former, some other wind must produce the latter. But there are no trade winds about the poles, and currents from these regions are for the most part (as the Labrador current after it meets the Gulf Stream) *below* the surface, and therefore beyond the influence of any wind whatever. Hence it would seem that trade winds have but little to do with the *general* system of aqueous circulation in the ocean.

† In this calculation, the earth is treated as a perfect sphere, with a diameter of 7925·56 miles.

parallels of these two places, its rate of motion around the axis of the earth, is reduced from nine hundred and twenty five* to seven hundred and fifty eight miles the hour.

Therefore this immense volume of water, in passing from the Bahamas to the Grand Banks, meets with an opposing force in the shape of resistance, sufficient in the aggregate, to retard it two miles and a half the minute, and this only in its eastwardly rate. There is doubtless another force qnite as great retarding it towards the north, for its course shews that it is the resultant of two forces acting in different directions. If the former resistance be calculated according to received laws, it will be found equal to several atmospheres, and by analogy, how inadequate must the pressure of the gentle trade winds be to such resistance, and to the effect assigned them? If therefore in the proposed enquiry, we search for a propelling power, no where but in the higher level of the gulf, we must admit, in the head of water there, the existence of a force sufficient at least to overcome the resistance required to reduce from two miles and a half, to a few feet per minute, the velocity of a stream that keeps in perpetual motion one fourth of all the water of the Atlantic Ocean.

The facts from observation on this interesting subject, afford us at best but a mere glimmer of light, by no means sufficient to make my mind clear as to a higher level of the Gulf, or as to the sufficiency of any other of the causes assigned for this wonderful stream. If it be necessary to resort to a higher level in the Gulf, to account for the velocity off Hatteras, I cannot perceive why we should not, with like reasoning, resort to a higher level off Hatteras also, to account for the velocity off the Grand Banks, and thus make the Gulf Stream a *descending* current, and by the *reductio ad absurdem*, show that the trade winds are not adequate to the effect ascribed.

When facts are wanting, it often happens that in their stead hypothesis will serve all the purposes of illustration. Let us therefore suppose a globe of the earth's size, having a solid nucleus and covered all over with water two hundred fathoms deep, and that every source of heat and cause of radiation are removed, so that its fluid temperature becomes constant and uniform

* Or 915·26 to 758·60. On the latter parallel, the current has a set to the east of one mile and a half the hour; making the true east velocity seven hundred and sixty miles the hour.

throughout. On such a globe, the equilibrium remaining undisturbed, there would be neither wind nor current; and the poet's picture would apply to every sea:

> "Still as a slave before his lord,
> The ocean hath no blast—
> His great bright eye most silently
> Up to the moon is cast."

Let us now suppose the globe to receive diurnal motion, and that all the water within the tropics, to the depth of one hundred fathoms, suddenly becomes oil, the aqueous equilibrium of the planet is thereby disturbed, and a general system of currents and counter-currents is immediately commenced—the oil, in an unbroken sheet on the surface, running towards the poles, and the water, in an under-current, towards the equator.

Thus, *without wind*, we should have a perpetual and uniform system of tropical and polar currents.

In consequence of diurnal rotation of the planet on its axis, each particle of oil, were resistance small, would approach the poles on a spiral turning to the east, with a relative velocity greater and greater, until finally it would reach the pole, and whirl about it at nearly the rate of one thousand miles the hour. Becoming water, and losing its velocity, it would approach the tropics by a similar but inverted spiral turning towards the west. For this reason, all currents, from the equator to the poles, should have an eastward tendency, and all from the poles towards the equator, a westward.

Let us suppose, further, that the solid nucleus of this hypothetical globe assumes the exact shape and form of the bottom of our seas, and that in all respects, as to figure and size, it represents the shoals and islands of the sea, as well as the coast lines and continents of the earth. The uniform system of currents just described would now be interrupted by obstructions and local causes of various kinds, such as unequal depth of water, contour of shore lines, &c., and we should have, at certain places, currents greater in volume and velocity than at others. But still there would be a system of currents, and of counter-currents, to and from either pole and the equator. Now, do not the cold waters of the north, and the warm waters of the Gulf, made specifically lighter by tropical heat, which we see actually preserving such a system of counter-currents, hold, at least in some degree, the relation of the supposed oil and water.

Apparently in obedience to the laws here hinted at, there is a constant tendency of polar waters towards the tropics and of tropical waters towards the poles. The Exploring Expedition crossed one of these hyper-borean currents two hundred miles in breadth at the equator. There is also one near our own coast, another on the west coast of South America, as well as several others elsewhere known to exist; but for obvious reasons, they are for the most part submarine and but little understood.

Counter to these are the Gulf Stream and the Lagullus and Australia currents, besides numerous others more gentle and partial, and therefore less marked in their character. But why one of these currents should always run from the Gulf of Mexico, and the other along the coast of Africa and Australia, seems to demand the presence of other agents. Perhaps these may be found in local causes, such as the contour of coasts, the constant force of trade winds, high temperature of the Gulf, &c. These would give the first impulse, and may be adequate to the initial velocity of the Gulf Stream.

Assuming its maximum velocity at five knots, and its depth and breadth in the narrows of Bemini as before, the vertical section across this stream would present an area of two hundred millions of square feet moving at the rate of seven feet three inches per second. The difference of specific gravity between the volume of gulf water that crosses this sectional line in one second, and an equal volume of water at the ocean temperature of the latitude, is fifteen millions of pounds. If these estimated dimensions (assumed here as throughout this paper they have been, from the best authority, but merely for the purposes of illustration) be within limits; then, the force per second operating here to propel the waters of the Gulf towards the pole, is the equilibrating tendency due to fifteen millions of pounds in the latitude of Bemini.

In drawing up a plan for investigating the currents of the seas, such agencies should be taken into account. As a cause, I doubt whether this one is sufficient of itself to produce a stream of such great and continued velocity as that of the Gulf; for assuming its estimated discharge to be correct, the proposition is almost susceptible of mathematical demonstration, that to overcome the resistance opposed in cosequence of its velocity, would require a force at least sufficient to drive at the rate of three miles the hour, *ninety thousand millions* of tons up an inclined plane, having an ascent of three inches to the mile.*

Yet the very principle from which this motive power is derived, is admitted to be one of the chief causes of those winds which are said to be the sole cause of this current.

But in addition to this, may there not be a peculiar system of laws, not yet fully understood, by which the motion of fluids in such large bodies is governed when moving through each other in currents of different temperature? That currents of sea water having different temperatures do not readily commingle, is shown by the fact already metioned, that the line of seperation between the warm waters of the Gulf and the cold waters of the Atlantic is perfectly distinct to the eye for several hundred miles; and to the thermometer even at the distance of one thousand miles, though the two waters have been in constant contact and continued agitation for many days, the thermometer shows that the cold water on either side still performs the part of river banks in keeping the warm waters of the stream in their proper channel.

In a winter's day off Hatteras, there is a difference between these waters of 20°.† Those of the Gulf being warmer, we are taught to believe they are also lighter; they should therefore occupy a higher level than those in which they float. Assuming the depth here to be one hundred and fourteen fathoms, and allowing the usual rate of expansion, figures show that the middle of the Gulf Stream here, should be nearly two feet higher than the contiguous waters of the Atlantic. Were this the case, the surface of the stream would present a double inclined plane, from which the water would be running down on either side as from the roof of a house. As this ran off at the top, the same weight of colder water would run in at the bottom; and thus, before this mighty stream had completed half its course, its depth would be brought up to the surface, and its waters spread out over the ocean. Why then does not such a body of warm water, flowing and adhering together through a cold sea, obey this law and occupy a higher level? That it does not, we may infer from the silence of navigators on the subject, as well as from other circumstances. If it did, the upper edges of its cold banks would support a lateral

* Without friction.

† Off the Grand Banks there is sometimes a differeuce of 30°.

pressure of at least one hundred pounds to the square foot; and vessels in crossing it, would sail over a ridge, as it were; on the east side of which, they would meet an easterly current, and on the west side a westerly current. The resolution of the forces of each of these currents with a northwardly set of the stream itself, would induce navigators to report a northeastwardly current as they ascend the other side of this ridge, and a northwest current as they descend on this. Yet never was it heard that the Gulf Stream runs northwest.

Should this roof-like current be too superficial to be felt by a vessel, the gulf weed and all the floating substances borne by this current accross the Atlantic, would run off either on one side or the other. But there is little or no gulf weed along its western edge, and its prevalence on the eastern side may be accounted for by the operation of an entirely different law.

Why this warm water therefore should not appear lighter than colder water, is a curious phenomenon that as far as I know, has never been considered. It is worthy of investigation. Nor should the paradox as to a higher level,—a double inclined plane in the Gulf Stream itself,—escape attention.

Dr. Lardner assures us, and such too is the doctrine not yet exploded from our popular* works, that sea water expands according to the laws of fresh, and from this circumstance, he and many others have argued that the fish in polar seas are preserved by the cold water on top and the warmer below. Deep sea soundings do not confirm this. With a surface temperature of 34°, northern voyagers have obtained a temperature at several hundred fathoms of 25°, which according to the received laws of expansion, should have the specific gravity of water at 55°. And a thermometer thrust down the throat of a fish caught in polar seas, has been said to stand at zero. The journals of arctic cruisers assure us of the fact, that the deeper we go down in the northern seas of America the colder the water, while off the shores of Northern Europe, the warmth increases as we go down, thus showing a warm stratum of the water to be lighter than the cold in one part of the ocean, and in another contiguous to it, to be heavier.

But to return to the mingling of the waters; we know from a familiar experiment,† that oil placed upon water in a state of rest,

* Dr. Marcet has shown that sea water contracts till it freezes.

† If a bit of paper cut in the shape of a comma (,) be dipped in oil and laid on water, the paper will spin round, while the oil runs off.

exerts an obvious and sensible force to put itself in motion. May there not exist between warm and cold sea water a tendency somewhat similar to that between oil and water? Seeing how great is the resistance encountered by the Gulf Stream in its eastward motion, and how insufficient is any head of water in the Gulf to give it its northward tendency, may there not exist between the tepid waters of the stream and their fluid banks always heaving and moving to the swell of the sea, a sort of *peristaltic* force, which, with other agents, assists to keep up and preserve this wonderful system of ocean circulation? We know that undulatory motion varies with temperature in certain other substances, and why should it not vary in water also?

Sir Isaac Newton demonstrated the velocity of waves to be in the sub-duplicate ratio of their breadths. Therefore, if two vessels, in a calm, one in and the other on the outside of the Gulf Stream, would each count the waves that pass, or the times that the vessel rolls from the one side to the other in a given time, we should have an argument for determining whether the oscillation of a wave in the Gulf Stream be shorter or longer, whether the rise and fall be greater or less, or whether there be any difference whatever between the warm wave and the cold one from which it is generated. That the waters of the Gulf Stream are more troubled than those of the Atlantic is well known by the ugly seas which are so much dreaded there by navigators.

But we want facts, and not theory. We have not enough of the former to build up any theory at all; nor should I undertake the structure if we had. In planning a system of observations in this magnificent field, instruction should cover the whole ground, and the attention of observers should be directed to every point from which it is possible or probable that light may come. Therefore, in throwing out these suggestions, sailor-like, I have but cast over my bottles; perhaps they may be picked up at some distant day—perhaps they may never be heard of again.

In its course to the north, the Gulf Stream naturally tends more and more to the eastward, until it arrives off the Banks of Newfoundland, where its course is said to become due east. These banks, it has been thought, deflect it from its proper course, and cause it to take this turn. Examination will prove, I think, that they are in part the effect, certainly not the cause. It is here that the frigid current from the north, already spoken of, is met, and its icebergs melted by the warm waters from the Gulf. Of course, the loads of stones, earth, and gravel brought down upon them, are here deposited. Captain Scoresby, far away in the north, counted five hundred ice-bergs setting out from the same vicinity upon this cold current for the south. Many of them, loaded with earth, have been seen aground on the banks. This process of transferring deposites for these shoals has been going on for ages, and with time, seems altogether adequate to the effects ascribed.

The maximum temperature of the gulf stream is 86°, or about 9° above the ocean temperature due to the latitude. Increasing its latitude 10°, it loses but 2° of its temperature; and after having run three thousand miles towards the north, it still preserves the heat of summer. With this temperature, it crosses the 40th degree of N. latitude, and there overflowing its liquid banks, it spreads itself out for thousands of square leagues over the cold waters around, and covers the ocean with that mantle of warmth which tends so much to mitigate, in Europe, the rigors of winter.

Moving now more slowly, but dispensing its genial influences more freely, it finally meets the British islands. By these it is divided, one part going into the polar basin of Spitzbergen, the other entering the Bay of Biscay, but each with a warmth considerably above ocean temperature. Such an immense volume of heated water cannot fail to carry with it, beyond the seas, a mild and moist atmosphere, and this it is which so much softens climate there.

We know not what the depth of the under temperature of the Gulf Stream may be; but assuming the temperature and velocity at the depth of two hundred fathoms to be those of the surface,* and taking the well-known difference between the capacity of air and water for specific heat, as the argument, a simple calculation will show that the quantity of heat discharged over the Atlantic from the waters of the Gulf Stream, in a winter's day, would be sufficient to raise the whole column of atmosphere that rests upon France and the British islands from the freezing point to summer heat.

Every west wind that blows crosses this stream on its way to Europe, and carries with it a portion of this heat to temper there

* Which probably is not the case.

the northern winds of winter. It is the influence of this stream upon climate that makes Erin the Emerald Isle of the sea, and clothes the shores of Albion with evergreen robes, while in the same latitude, on this side, the shores of Labrador are fast bound in fetters of ice. In a valuable paper on currents,* Mr. Redfield states, that in 1831, the harbor of St. John's, Newfoundland, was closed with ice as late as the month of June, yet who ever heard of the port of Liverpool, on the other side, though two degrees further north, being closed with ice even in the depth of winter? Baron Humboldt's isothermal curves shew that the genial influence of this current is felt in Norway, and even on the shores of Spitzbergen, in the polar basin. The mere sweeping of the winds over a large tract of ocean, without any such warm stream, is not sufficient to produce such effects upon climate, as is fully shewn by comparing the climate of Spitzbergen with that of places similarly situated in the South Seas, with regard to winds and water, but not with regard to currents.

Nor do the beneficial influences of this stream upon climate end here. The West Indian Archipelago is encompassed on one side with its chain of islands, and on the other by the Cordilleras of the Andes, bending through the Isthmus of Darien, and stretching themselves out over the plains of Central America and Mexico. Beginning on the summit of this range, we leave the regions of perpetual snow, and descend, first into the *tierra templada,* and then into the *tierra caliente,* or burning land. Descending still lower, we reach both the level and the surface of the Mexican seas—where, were it not for this beautiful and benign system of aqueous circulation, the peculiar features of the surrounding country assure us we should have the hottest, if not the most pestilential climate in the world. As the waters of these two caldrons become heated, they are borne off by the Gulf Stream, and are replaced by cooler currents through the Carribbean sea—the water, as it enters here, being 3° or 4° cooler at the surface than when it escapes from the Gulf, and at considerable depth, being as much as 40°† below the surface temperature. Taking this

* Am. Journal of Science, vol. xlv., p. 293.

† Surface temparature, 85° Sept. 84° July.
Temp. in depth, 48°, 240 fathoms. 43°, 450 fathoms. } In the Carribbean Sea.
Surface temp. 83°, 86½° Musqueto shore.
Depth, 42°, 450 fathoms 43°, 500 fathoms. } From the Journal of M. Dunsterville.

difference in surface temperature only as the degree of heat accumulated there, it can be made to appear that the quantity of specific heat daily discharged through the Gulf Stream, from those regions, is sufficient to raise mountains of iron from zero to the melting point, and to keep up from them a molten stream of metal greater in volume than the waters daily discharged from the Mississippi river. Who, therefore, can calculate the benign influence of this wonderful current upon the climate of the south?

At the depth of two hundred and forty fathoms, the temperature of the currents setting into the Carribbean Sea has been found as low as 48°, while that of the surface was 85°. Another cast, with three hundred and eighty-six fathoms, gave 43° against 83° at the surface. The hurricanes of those regions agitate the sea to great depths: that of '80 tore rocks up from the bottom in seven fathoms, and cast them on shore. They, therefore, cannot fail to bring to the surface portions of the cooler water below.

These cold waters, doubtless, come down from the north to replace the warm water sent through the Gulf Stream, to moderate the cold of Spitzbergen; for, within the arctic circle, the temperature at corresponding depths off the shores of that island is only one degree colder than in the Carribbean Sea; while, on the coasts of Labrador, the temperature in depth is 25°, or 7° below the freezing point of fresh water. Captain Scoresby relates that, on the coast of Greenland, in latitude 72°, the temperature of the air was 42°, of the water 34°, at the surface, and 29° at the depth of one hundred and eighteen fathoms. He there found a current setting to the south, and bearing with it this extremely cold water, with vast numbers of ice-bergs, whose centres, perhaps, were far below zero. It would be curious to ascertain the routes of these under-currents, on their way to the tropical regions, which they are intended to cool. One has been found at the equator two hundred miles broad, and 23° colder than the surface water. Unless the land, or shoals, intervene, it no doubt comes down in a spiral curve.

Perhaps the best indication as to these cold currents may be derived from the fish of the sea. The whales first pointed out the existence of the Gulf Stream by avoiding its warm waters. Along our own coasts, all those delicate animals, and marine productions, which delight in warmer waters, are wanting; thus indicating by their absence the cold current from the north now known to exist there. In the genial warmth of the sea about the Bermudas on one hand, and California on the other, we find, in great abundance, those delicate shell-fish and coral formations which are altogether wanting in the same latitudes along the shores of South Carolina. The same obtains on the west coast of South America; for there, the cold current almost reaches the line before the first sprig of coral is found to grow. A few years ago, great numbers of bonita and albercose, tropical fish, following the Gulf Stream, entered the English Channel, and alarmed the fishermen of Cornwall and Devonshire by the havoc which they created among the pilchards there.

It may well be questioned if our Atlantic cities and towns do not owe their excellent fish markets, as well as our watering places their refreshing sea-bathing, in summer, to this stream of cold water.

The temperature of the Mediterranean is 4° or 5° above the ocean temperature of the same latitude, and the fish there are very indifferent. On the other hand, the temperature along our coast is several degrees below that of the ocean, and from Maine to Florida our tables are supplied with the most excellent of fish. The sheep's head, so much esteemed in Virginia and the Carolinas, when taken on the warm coral banks of the Bahamas, loses its flavor, and is held in no esteem. The same is the case with other fish—when taken in the cold water of that coast, they have a delicious flavor, and are highly esteemed; but when taken in the warm water, on the other edge of the Gulf Stream, though but a few miles distant, their flesh is soft and unfit for use. The temperature of the water at the Balize reaches 90°. The fish taken there are not to be compared with those of the same latitude in this cold stream. New Orleans, therefore, resorts to the cool waters of the Florida coasts for her most choice fish.

The same is the case in the Pacific, the current of cold water from the south sweeps the shores of Chili, Peru and Equador and reaches the Gallipagos Islands under the line. Throughout this whole distance, the world does not afford a more abundant and excellent supply of fish.

Yet out in the Pacific at the Society Islands where coral abounds, and the water preserves a higher temperature, the fish though they vie in gorgeousness of coloring with the birds and plants and insects of the tropics, are held in no esteem as an article of food.

Therefore let those who are curious as to the migratory habits of fishes, join hands in the proposed system of observations upon currents, for the few facts which we have bearing upon the subject seem to suggest it as a point of the inquiry to be made whether the habitat of certain fishes does not indicate the temperature of the water, and whether these warm and cold currents of the ocean do not constitute the great highways through which migratory fishes travel from one region to another.

Navigators have often met with vast numbers of sea nettles (medusæ) drifting along with the Gulf Stream. They are known to constitute the principle food for the whale; but whither bound, by this route, has caused much curious speculation, for it is well known, that the habits of the whale are averse to the warm waters of this stream. An intelligent sea captain informs me that two or three years ago, in the Gulf Stream, on the coast of Florida, he fell in with such a "schole of young sea nettles as had never before been heard of." The sea was covered with them for many leagues. He likened them in appearance on the water to acorns floating on a stream. He was bound to England, and was five or six days in sailing through them. In about sixty days afterwards, on his return, he fell in with the same schole off the Western Islands, and here he was three or four days in passing them again. He recognized them as the same, for he had never before seen any like them; and on both occasions he frequently hauled up buckets full and examined them.

Now the Western Islands is the great place of resort for whales. And at first there is something curious in the idea, that the Gulf of Mexico is the harvest field, and the Gulf Stream the gleaner, which collects the fruitage planted there, and conveys it thousands of miles to the hungry whale at sea. But how perfectly in unison is it with the kind and providential care of that great and good Being, which feeds both the young ravens and the sparrows!

Our information as to the Sargasso sea is most barren. Whence comes the weed with which it is covered, or where its place of growth may be, is matter of dispute among learned men. But as for the office which it performs in the economy of the ocean, conjecture even is silent. Certain it is however, that sea of weeds was not planted in the middle of the ocean without design. The marks of intelligence, displayed throughout the whole system of terrestrial adaptations, forbid the idea.

The system of observations proposed will be of high interest also to the meteorologist, whose science at this time is attracting so much attention. The fogs of Newfoundland which so much endanger navigation in winter, doubtless owe their existence to the presence in that cold sea, of immense volumes of warm water brought by the Gulf Stream.

Sir Philip Brooke found the air on each side of it at the freezing point, while that of its waters was 80°. "The heavy warm damp air over the current produced great irregularities in his chronometers." The excess of heat daily brought into such a region by the waters of the Gulf Stream, if suddenly stricken from them, would be sufficient to make the whole column of superincumbent atmosphere ten times hotter than melted iron.

With such an element of atmospherical disturbance in its bosom, we might expect storms of the most violent kind to accompany it in its course.

Accordingly, the most terrific that rage on the ocean, have been known to spend their fury in and near its borders.

Connected with, and dependent upon, this great artery of the ocean, are numerous veins in the shape of eddies, counter-currents, drifts, and sets, all of the highest importance to navigation, and to the neglect of which many frightful disasters among sea-faring people are to be ascribed.

In 1804, his Majesty's ship Apollo left the Cove of Cork with sixty-nine sail, under convoy for the West Indies. They encountered a current in the tail of the Gulf Stream, of which they were not aware, and on the seventh day out, the Apollo, with forty of the convoy, were wrecked on the coast of Portugal.

In 1825, eight hundred sail of British shipping were lost at sea. And, upon an average, one American and two British ships are wrecked every day the year round. Most of these losses are owing to the effects of unknown currents. I doubt whether a subject more important in its bearings has ever before been presented to the Institute.

The field of investigation is most magnificent. When rightly understood, this system of marine currents and ocean circulation cannot fail to open to us a beautiful and harmonious arrangement. Though I have only glanced at some of the currents in the Atlantic, there are others, both there and in the Pacific, not less interesting, and scarcely less important than these.

Among them, circumstances seem to invite enquiry as to the probability of a "gulf stream" in the North Pacific. The resort of whales about Japan, as about Newfoundland, in the same latitude—the European-like climate of Oregon—the thunder storms encountered in high Pacific latitudes out of season, and the absence of whales in the same region, all appear to indicate, between northwestern America and northeastern Asia, the presence of a large body of warm water. The probability of such a current there seems to become stronger, and its similarity to the Gulf Stream more striking, from the fact, that the extreme cold of northeastern Asia corresponds with our northeastern climate, indicating thereby the existence of a cold current corresponding to that on our Atlantic coast, where certain cold water fish are known to resort.*

We have seen how useful currents are in the economy of the ocean, and how important to a safe and speedy navigation is the knowledge of them.

In the beautiful system of cosmical arrangements and terrestrial adaptations by which we are surrounded, they perform active and important parts; they not only dispense heat, and moisture, and temper climates, but they prevent stagnation in the sea; and by their active circulation, transport food and sustenance for its inhabitants from one region to another, and people all parts of it with life and animation. Yet, on this interesting subject former observations have thrown only just light enough to make visible the darkness through which we are groping.

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* Captain, now Admiral Dupetit Thuars found a surface temperature of 79° (Far.) one thousand miles east of Japan, which clearly indicates such a stream. And its existence is further confirmed by Mr. Redfield, who has formed his conclusions from information derived from our whale-men. This being the case, we should have a cold current from Beehring's Straits, to answer to the one on this side from Baffin's Bay.

Art. XII.—*Abstract of the Proceedings of the Thirteenth Meeting of the British Association for the Advancement of Science.**

(Concluded from Vol. xlvi, p. 401.)

Section C. *Geology and Physical Geography.*

A paper was read, "*On the Phenomena and Theory of Earthquakes, and the explanation they afford of certain facts in Geological Dynamics,*" by Profs. H. D. and W. B. Rogers. [The facts and principles contained in this paper were communicated in detail to the Association of American Geologists and Naturalists, at their meeting in Albany in April, 1843, and an abstract was given in this Journal, Vol. xlv, p. 341.] In the discussion which followed the reading of this paper, Major S. Clerke remarked, that having witnessed the great eruption of Vesuvius in 1822, it occurred to him to ascertain whether any reciprocity existed between that volcano and the small crater of the Solfatara, near Pozzuoli, distant about ten miles, in a line across the bay of Naples. On proceeding to the latter place he was informed by the *custode*, that since Vesuvius had been in activity the Solfatara had subsided. Major Clerke, on a personal examination of the aperture forming the vent of the Solfatara, found this to be the fact, the usual eruption and loud noise having ceased. When Vesuvius relapsed into quiet, the Solfatara resumed its activity.

"*An account of the Earthquake at the islands of Antigua and Guadaloupe on the* 8*th of February*, 1843," by Hon. Capt. Carnegie. This earthquake was felt generally among the Leeward Islands, but more particularly at Antigua and Guadaloupe. At both these islands the shock took place at twenty minutes before 11 o'clock, A. M., and it does not appear to have been preceded by any of the usual signs of earthquake; the weather was clear and fine, the sea-breeze blowing as usual, and the inhabitants engaged in their daily avocations. At Antigua the earth heaved and undulated suddenly; the hills oscillated, and huge masses of rock were detached from their summits and precipitated into the valleys; large fissures opened in the ground and closed immediately; and in the space of two minutes and a half all Antigua was laid in ruins. In this island only eight persons lost their lives, owing to the black population being employed among the canes, but the loss of property was immense. At Point-à-Pitre, in the island of Guadaloupe, the effects were much more fearful. In magnitude, this was the second town in the West India islands; it was situated upon a piece

* Condensed from the Report in the London Athenæum.

of low ground, surrounded on three sides by the sea, and entirely built of stone to avoid the effect of hurricanes. At the time of the earthquake, most of the inhabitants appear to have been at their late breakfast, in consequence of which 4,000 perished among the falling houses, or in the fire which broke out immediately after; the destruction of the whole town was so complete, as to present, after the earthquake, the appearance of a vast stone quarry. The landslips were very numerous, and all the springs in the vicinity of Point-à-Pitre were instantly dried up. The shock was felt slightly as far north as Washington and Bermuda, aud southward to Demerara, travelling in a N. N. E. and S. S. E. direction; several slight shocks were subsequently felt at different periods.

"*On the important additions recently made to the Fossil Contents of the Tertiary Basin of the Middle Rhine*," by R. I. Murchison, Esq. After a sketch of the geographical limits and geological relations of the tertiary deposits which occupy the valley of the Rhine and Mayne, around the towns of Mayence, Frankfort, and Darmstadt, Mr. Murchison gave an account of the recent discoveries made by M. H. von Meyer, M. Kaup, of Darmstadt, and M. Braun, of Heidelberg. Of the animals of this tertiary basin, M. von Meyer had catalogued and was preparing for publication 68 mammifers, 30 reptiles, 13 birds, and 8 batrachians—nearly all being undescribed species, and most of them of small dimensions. Amongst the new animals discovered by M. Kaup, were mentioned the Chalicotherium, a genus allied to Anophotherium and Lophiodon; the Hippotherium, differing from the recent Equus in the possession of an additional metacarpal bone; and a minute Saurian, named *Pisodon Colei.* M. Kaup had determined from an examination of the remains of various species of rhinoceros, tapir, &c. occurring in this deposit, that the Fauna of the period presented a close affinity to the types of the Indian and Sumatran archipelago, and were entirely distinct from all known European mammalia. He had also collected a large series of mastodontoid remains, which completely proved the views of Prof. Owen, respecting the identity of the American Tetracaulodon with the true Mastodon. The invertebrata of the deposite have been examined by M. Alexander Braun, and have been found to comprise 450 species, 306 of which are mollusca, and 103 shells, —of which, 10 species only were identical with living forms. Many of the shells approach closely in form to those in the *calcaire grossier* of Paris, and this circumstance, together with the occurrence of the Anthracotherium, and of an animal intermediate between the Anoplotherium and Palæotherium, makes it probable that the deposit belongs to the same age as the gypsum beds of Montmartre, and the Ryde and Binsted strata of the Isle of Wight. These tertiary beds are covered with gravel, sand,

and *löss*, containing 96 species of shells, 56 of which are terrestrial, and 40 fluviatile. Of these, seven belong to species now living, and nine others are probably varieties of existing species—the most abundant species are very rare in a living state, whilst those now common are of unfrequent occurrence in the *löss*. With the shell are associated the remains of mammoth, rhinoceros, tichorinus, &c., the bones of which have evidently received very little injury from diluvial action; and from the frequent occurrence of entire skeletons, Mr. Murchison infers that these superficial deposits were formed by very tranquil operations, and that the great mammalia inhabited tracts immediately adjacent to the spots where they are now entombed. Mr. Owen stated, that the Mastodon of the Mayence basin was identical with the species found in the Norwich Crag, which was likewise a fluvio-marine deposit. He had not seen any bones in the English tertiary or drift, which could be distinguished from the ordinary horse or zebra, excepting a few teeth, which were more curved than usual, and might possibly have belonged to the Hippotherium.

Major L. Beamish, F. R. S., read a paper "*On the apparent fall or diminution of water in the Baltic, and elevation of the Scandinavian coast.*" During a journey to Stockholm in the early part of the summer of 1843, Major B. had occasion to see and hear much respecting the diminution of water in the Baltic, a practical and personal evidence of which he experienced in the harbor of Travemunde, on the 4th of May, by the sudden fall of water at the port, which took place very rapidly and to a great extent; by this cause the steamer which ought to have left Travemunde on the 18th was detained until the 21st. It is well known that although without tide, the Baltic is subject to periodical variations of depth, but the water has fallen during the present summer to a degree far below these ordinary variations; and the fact was considered so remarkable as to be thought worthy of being brought before the Swedish Academy of Sciences, by Baron Berzelius. This fall or diminution of water was already perceptible in the summer of 1842, since which the Baltic has never returned to its average mean height; but on the contrary, has diminished, and there seems now no probability that the former level, or the height of 1841, will be again attained. Meantime, no perceptible change has taken place in the waters of the North Sea, and the unscientific observer asks, what has become of the waters of the Baltic? The answer is probably to be found in a simultaneous phenomenon apparent on the Swedish coast, the gradual elevation of which, has been satisfactorily proved by the personal observation of Mr. Lyell. Recent observation, however, would seem to show, that this elevation does not proceed at any regular or fixed rate, but, if he might use the expression, *fitfully*, at uncertain periods, and at a rate far greater than was at first supposed. At the meeting of the Swedish Academy just referred to, a communication was made from an officer who had been employed on the southwest coast of Sweden, giving evidence of the recent elevation of that part of the coast, and stating, that during the present summer, fishermen had pointed out to him near the Maelström, at Offroust, shoals which had never before been visible. The elevation of the Swedish coast forms a striking contrast with the unchanged position of the contiguous coast of Norway, which, as far as observation has been hitherto extended, has suffered no change within the period of history, although marine deposits, found upon the Norwegian hills, at very considerable elevations above the level of the sea, prove that those parts were formerly submerged. Dr. Langberg, of Christiana, confirmed the statement made by Major Beamish, as to the unchanged position of the Norwegian coast, within the period of history, as far as observation has hitherto extended. Mr. Lyell observed, that if the rate of upward and downward movement of the land in Scandinavia could be proved to take place irregularly, and sometimes with considerable rapidity in a short time, it would remove many difficulties in the explanation of these phenomena, by the hypothesis of a surface movement. He then alluded to the buried hut at Södertelge, near Stockholm, above which regular deposits, twenty four feet in thickness, were found, containing marine and fresh water shells, such as now inhabit the brackish waters of the Baltic. The position of this hut can only be reconciled with the belief that Scandinavia was peopled within the last 5000 or 6000 years, by supposing a greater rate of movement in the land than that experienced within the last few centuries near Stockholm.

Section D. *Zoology and Botany.*

Rev. W. Hincks called attention to two living specimens of the *Neottia gemmipara* of Smith. This very rare plant had been discovered by Mr. J. Drummond in a salt marsh near Castleton, Bearhaven, in the county of Cork, in 1810. From an imperfect specimen, Sir J. S. Smith had described and figured it, and it had not been seen again till 1841, when it was re-found by Dr. Sharkey. Only one specimen was again obtained, and it was with difficulty identified with the original specimen in the Linnæan herbarium in London. Dr. Wood and Dr. Harvey had, during the past week, both gathered living specimens, which were now on the table. The original plant was not a Neottia, as had heen supposed by Dr. Smith, but was now referred to Spiranthes. Mr. Babington stated, that he had carefully examined the plant on the table, and believed that it was a genuine Spiranthes. It was a matter of great interest, as probably this plant was one of the rarest in the world. There was no record of its having been found any where but in the locality from whence these specimens were brought. Mr. Hincks stated, that among some Californian plants received from London, had been found what appeared to be a *Spiranthes gemmipara.*

Mr. Strickland read to the Section *a Catalogue of the Birds found in Corfu and the Ionian Islands*, by Capt. H. M. Drummond, 42d R. H. The total number of species in the list is 198; of these 159 are British, 38 are found on the European continent, but not in Britain, and one, the *Calamoherpe olivetorum*, Strickland, has hitherto been found only in the Ionian Islands, where it is very common during the summer.

The next paper was a similar list by the same gentleman, of *the birds of Crete*, made during a visit to that Island in May and June, 1843, in company with Capt. Graves, of *H. M. S. Beacon.* From the shortness of the author's stay in Crete, this list is less complete than the last; the total of species observed was 105, of which 86 are British and 19 continental. It is remarkable that the common sparrow of Crete is the *Fringilla cisalpina*, while in the Ionian Islands it is replaced by the F. *domestica.*

Prof. Owen read a continuation of his *Report on the Fossil Mammalia of Great Britain.* In the previous parts of the Report he had treated of the Carnivora and Marsupiata: in this he confined himself to an examination of the vegetable-feeders. Remains of the mammoth, of which he believed there was only one species, the *Elephas primogenius* of Cuvier, had been found in abundance in various parts of Great Britain, and not less than three thousand teeth had been found in various places. These teeth varied in appearance with age, and had thus led to the supposition that there were other species of mammoth. The remains of the mastodon found in Suffolk and Yorkshire, were undoubtedly identical with those of the miocene beds of France, thus making but one European species, the *Mastodon angustidens.* Bones of an extinct species of rhinoceros had been found in various parts of Great Britain. It possessed two horns, and was undoubtedly different from existing species. The teeth also of a species of hippotamus had often been found, as in the cave at Kirksdale and other limestone rocks. In the Isle of Wight two distinct genera of pachyderms had been found; the Palæotherium and Anoplotherium. Of the former genus, there appears to have been six or seven species, and of the latter three. The teeth and fragments of bones of more uncommon forms still, had been found in the Isle of Wight and in Suffolk, which had been referred to animals of distinct genera, and called Chæropotamus and Hyræotherium. Remains of species of the genus Sus had also been found by Dr. Buckland. Of the Ruminantia, the Irish elk, as it has been called, has been found in England. In structure this animal resembled the fallow deer, but was larger. The remains of a cervus, not differing from the red deer, were frequently found with the mammoth and other animals. Remains also of the roebuck, and the goat and sheep, had been found associated with those of the mammoth. Remains of the aurocks or European bison, had been found in various parts of England, and also of Ireland. According to Mr. Ball, these remains should be looked upon as those of an extinct species. In concluding his Report, Prof. Owen stated, that he never should have commenced these labors, nor could he have continued them, but for the suggestions and assistance of the British Association.

Art. XIII.—*A Notice of some of the more important Articles contained in Berzelius's Rapport Annuel Sur les Progrèe de la Chimie;* presented to the Royal Academy of Sciences at Stockholm, March 31, 1843.*

Are the Atomic Weights multiples of the equivalent of Hydrogen?—It is known that the opinion advanced by Prout, that the atomic weights of all simple bodies are entire multiples of the equivalent of hydrogen, was abandoned when Turner arrived at a negative result after a series of experiments, with which he had been charged by the British Association for the Advancement of Science. It is also recollected that M. Dumas revived this opinion by his experiments to determine the exact atomic weight of carbon, having found it exactly twelve times that of hydrogen. M. M. Erdman and Marchand have repeated and confirmed his experiments. In waiting farther research, it would appear that M. Dumas's experiments confirmed by the German chemists have not been executed with all the care requisite, and that probably a certain agreement between the experiments and a preconceived opinion of the authors, had blinded them somewhat to certain minute precautions necessary in the process. One might almost believe that M. Dumas has tried by a single dash of the pen to cast a suspicion of inaccuracy upon all the careful and arduous researches of those who have advanced a different opinion concerning the atomic weight of

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* *Messrs. Editors,*—This is but a hasty notice of some of the important articles contained in the Report of Bezelius, and given in almost all cases in the words of the author; those of your readers who have an opportunity to refer to it I would recommend them to do so. A tolerably complete sketch is given of what has been done upon the subject of atomic weights, which goes to show that Prout's theory is as doubtful as ever. Yours, J. L. S.

simple bodies. M. Pelouze (Pogg. Ann. LVIII, 171) has also examined this question. He has followed a new method, and has arrived at a result which would seem to put an end to this discussion. To accomplish this, it is only necessary to perform a single experiment requiring two weighings. This experiment consists in decomposing the chlorate of potash, entirely free from water, in a convenient vessel. This salt is composed as we all know of one atom of potassium, two atoms of chlorine and six atoms of oxygen. The six atoms of oxygen are driven off by heat, and one atom of chloride of potassium is left. The weight of the six atoms of oxygen, according to the hypothesis, is equal to forty eight equivalents of hydrogen, consequently the weight of the chloride of potassium which remains ought to be exactly divisible by the weight of the equivalent of hydrogen, if the atomic weights of chlorine and potassium are equal to entire multiples of the equivalent of hydrogen. M. Pelouze has obtained from three experiments upon one hundred parts of the chlorate of potash.

Oxygen gas,	39·157	39·143	39·161
Chloride of potassium,	60·843	60·857	60·839

These numbers accord with those of M. DeMarignac and of Berzelius.

	M.	B.
Oxygen gas,	39·161	39·150
Chloride of potassium,	60·839	60·850

M. Pelouze has deduced from these results 932·295 as the atomic weight of the chloride of potassium, and if this divided by 12·5, which is the equivalent of hydrogen from the experiments of Dumas, we obtain 74·583 equivalents of hydrogen; a result which differs from the hypothesis by too large a number to be due to an error of analysis.

Atomic Weight of Hydrogen.—M. Berzelius reviews at some length the experiments of M. Dumas upon this subject, who thinks that he has established the equivalent of hydrogen to be 12·5, oxygen being 100. Berzelius seems to think that the method of experimenting was not free from error.

Atomic Weight of Chlorine.—The authors of the revision of the atomic weights have also made some experiments upon the equivalent of chlorine. The opinions of M. Dumas determined M. Marignac to make a series of experiments, which he communicated to the Académie des Sciences, April, 1842. The result of the calculation from these experiments, pointed out 450 as the equivalent of chlorine, or thirty-six times the equivalent of hydrogen. M. Laurent (Comptes Rendus, XIV, 570) communicated about the same time a research upon the same subject. He analyzed the substance obtained by treating the chloride of naphthaline ($C^{10}H^8+4Cl$) by an additional quantity of chlorine, from which results the new compound ($C^{20}H^{14}Cl^{10}$). Three analyses of which gave

Carbon,	39·47	39·41	39·39
Hydrogen,	2·31	2·30	2·33
Chlorine,	58·22	58·29	58·28

The following numbers represent those derived from calculation with the various numbers proposed for chlorine.

	Cl=437·5	Cl=442·65	Cl=450
Carbon,	39·73	39·468	39·088
Hydrogen,	2·32	2·302	2·280
Chlorine,	57·95	58·230	58·632

It is seen from this that only those numbers which have been obtained by means of the old atomic weight, accord with the results of the analysis.

M. Marignac (Ann. der Chem. und Pharm., XLIV, 11) recommenced his experiments upon this subject, following a different process from the one he originally adopted; his experiments would occupy too much space to detail, and we refer those curious upon this subject to the Annual Rapport, or to the original article itself. The result of these last experiments has been 442·198 as the equivalent of chlorine.

Atomic Weight of Potassium.—M. Marignac, in the experiments just alluded to, determined the atomic weight of Potassium to be 489·954.

Atomic Weight of Calcium.—M. Dumas (Jour. für Pr. Chem. XLVI, 460) has tried to prove that the atomic weight of calcium is a multiple of that of hydrogen, and the result of his experiments is that it is twenty times that of hydrogen or 250. M. M. Erdmann and Marchand have confirmed the accuracy of the result. Berzelius in his report enters into some detail as to what may be the inaccuracy attendant upon such experiments, as were made by those gentlemen, and gives as the result of some recent experiments of his 251·942 as the atomic weight of calcium.

Atomic Weight of Glucinium.—From the experiments of M. Awdejew, and those of the Count Schaffgotsh, M. Berzelius concludes that the atomic weight of glucinium is 87·124, and that its equivalent is two atoms or 174·28, glucina being represented by $\dddot{G}$=474·28.

Atomic Weight of Silver.—M. Marignac in determining the atomic weight of chlorine ascertained also that of silver, which he concludes to be 1350 and not 1351·6.

Atomic Weight of Uranium.—According to M. Peligot (Ann. de Chim. et de Phys. V, 547) it is 750. M. Ebelmen makes it 742·875. M. Wertheim 740·512.

Atomic Weight of Cerium and Lantanium.—M. Beringer (Ann. der Chem. und Pharm. XLII, 139) has determined the atomic weight of cerium and found it to be 576·97. M. Rammelsberg (Pogg. Ann. LV, 64) has found for lantanium 554·88, and for cerium 572·8.

Variation of the Boiling Point of Water in Vessels of Different Materials.—M. Marcet (Ann. de Chim. et de Phys. V, 449) has made a series of accurate experiments upon the variations of the boiling point of water, dependent upon the nature of the vessel in which it is made to boil. The following are the results of his researches:—1st. The boiling point of water in glass vessels is from 100°·3 to 102° cent., varying with certain circumstances, more especially with the different kinds of glass. In these cases the temperature of the vapor of water is constantly the same, and some hundredths of a degree lower than when water is boiled in metallic vessels. 2d. Whatever the nature of the vessel in which the ebullition takes place, the temperature of the vapor is always lower than that of the water from which it is generated. This difference in the case of glass vessels is about 1°·06 cent.; in metallic vessels 0°·15 to 0°·20 cent. There is only one exception to this rule, which is when the interior of the vessel, whether of glass or metal, is covered with a thin coat of sulphur or gum lac, or any other substance which exercises a repulsion for water; the boiling water and the vapor then have the same temperature. 3d. Contrary to the opinion generally admitted, it is not in metallic vessels that the boiling point is lowest under an increased pressure, but in glass vessels covered internally with a thin coat of the substances mentioned above. 4th. In glass vessels having the internal surface perfectly smooth and free from foreign matter, water and alcohol can be heated several degrees above their boiling point before they enter into ebullition. Water can be thus heated to 105° cent. If the experiment does not succeed, it is because there is foreign matter adhering to the glass. The experiment may be performed successfully with a new vessel, by first heating sulphuric acid in it to 150° cent., then washing it out with pure water.

Evaporation of Water under Electrical Insulation.—M. Rowles (Phil. Mag. XX, 45) suspended two similar capsules of eight and a half inches diameter by silk strings, over a stove that was heated daily. In each vessel eight and a half ounces of water was poured. One of the vessels communicated with the earth by a copper wire. At the end of twenty-four hours, 2 ounces and 279 grains had evaporated from the insulated capsule, and 3 ounces and 144 grains from the other, making a difference of 345 grains in favor of the capsule in communication with the earth. The same result was obtained with the heat of the sun.

The Action of Light upon an Iodized Silver Plate, generates a Current of Electricity.—M. Becqueral (Pogg. Ann. LV, 588) has shown that two iodized silver plates, plunged in water in the dark, afford no current of electricity when connected by a galvanic multiplier,—but that a current is immediately produced when the light strikes the iodized side of one of the plates. The plate receiving the influence of the sun becomes positive.

Nitric Acid.—M. Millon (Ann. der Chim. und Pharm. XLIV, 109) has made a great number of experiments upon the solvent power of nitric acid upon the metals; and he has discovered that when perfectly exempt from nitrous acid, it possesses only a very feeble solvent power at the ordinary temperature, even upon metals that we know it dissolves with the greatest energy. This is owing to the fact that the nitric acid ordinarily employed contains nitrous acid, although colorless. If nitric acid is deprived of nitrous acid by ebullition, and then added to a metal which it does not dissolve, it is only necessary to add a very small quantity of some nitrite to bring about the commencement of the solution. M. Millon explains this phenomenon by supposing that the veritable oxidizing agent is nitrous acid,—this acid forming a nitrite with a disengagement of nitric oxide, (part of the acid has been decomposed to oxidize the metal, and from this nitric oxide is the residue,) which becomes converted into nitrous acid at the expense of the nitric acid; the nitric acid in its turn drives the nitrous acid from the newly formed salt, and in this manner there is constantly a sufficient quantity of nitrous acid, until the whole of the nitric acid becomes saturated or the metal entirely dissolved.

Tension of Sulphuric Acid.—It has been shown by Baron Wrede and by M. Vogel, that air or gas passed over sulphuric acid to be dried always contains a small portion of this acid. M. Vogel placed under a bell glass some concentrated sulphuric acid, with a plate containing the chloride of barium; they were left together for five days, and upon dissolving away the chloride, he obtained a residue of sulphate of baryta, weighing 1·011 grains.

New Sulphur Acid.—MM. Fordos and Gelis (Ann. de Chim. et de Phys. VI, 484) has discovered a new sulphur acid, which has a singular composition. When to two equivalents of hyposulphite of soda, ($\dot{Na}\ \ddot{S}$,) one equivalent of iodine is added; the iodine dissolves and affords a limpid and neutral solution, containing 1 at. iodide sodium and 1 at. of a salt formed of 1 at. of soda, 4 ats. of sulphur, and 5 ats. of oxygen. The two atoms of hyposulphurous acid combine, in this reaction, with the oxygen liberated by the iodine, forming an acid composed of 4 ats. sulphur and 5 ats. oxygen. They have called this acid hyposulphuric bisulphuretted.

Solubility of Chlorine in Water.—M. Pelouze (L'Institut, No. 473, 11) determined the quantity of chlorine dissolved by Gay-Lussac's method with arsenious acid. One hundred volumes of water dissolved the following volumes of chlorine gas:

175 to 180	vol.	at the temp. of	0°	cent.
270 to 275	"	"	9	"
270 to 275	"	"	10	"
250 to 260	"	"	12	"
250 to 260	"	"	14	"
245 to 250	"	"	0	"
200 to 210	"	"	30	"
155 to 160	"	"	40	"
115 to 120	"	"	50	"
60 to 65	"	"	70	"

Hydrobromic and Hydriodic Acid.—M. Millon (Jour. de Pharm. et de Chim. I, 299) has proposed a modification of the ordinary method. He employs the bromide or the iodide of potassium in the place of the pulverized glass, and thus obtains a much larger quantity of the hydracids, because the salt employed is decomposed by the phosphoric acid which it converts into phosphate of potash. The proportions that react are, 2 ats. of the salt of potash, 5 ats. of the bromine or iodine, 1 at. of phosphorus, and 7 ats. water. To obtain the hydrobromic acid, M. Millon employs 15 grammes of the bromide potassium and a little water, to which he adds 25 grammes of bromine, and 2 grammes of phosphorus cut up in small pieces. The hydrobromic acid soon begins to be so rapidly disengaged, that it becomes at times necessary to plunge the vessel in cold water to check the action. When the disengagement of gas becomes slow, it is aided by a little heat. In the preparation of the hydriodic acid, it is necessary to heat from the beginning, and the escape of gas is very uniform.

Cyanogen.—M. Wöhler has shown that when nitrogen gas containing moisture is passed over a mixture of potash and charcoal cyanide of potassium is formed, but if the gas be dry no cyanogen is formed.

Combinations analogous to Cyanogen formed by the combination of Boron and Silicon with Nitrogen.—M. Balmain (Phil. Mag. XXI, 270) has described some very interesting experiments which seem to prove that boron and silicon form with nitrogen combinations endowed with the halogen properties of cyanogen. When a mixture of 7 parts of anhydrous boracic acid and 20 parts of cyanide of potassium are exposed in a well covered crucible to a white heat, there remains after cooling a porous mass. The proportions of this mixture are calculated in such manner as that the carbon of the cyanide of potassium suffices exactly to reduce the boracic acid in being converted into carbonic oxide. The substance as taken from the crucible is white and porous, easily reduced into powder. It is infusible and insoluble in cold water and in warm water, in a cold solution of an alkali, in nitric, hydrochloric and sulphuric acids, and in aqua regia. It is not altered by hydrogen at the temperature of incandescence; the vapor of water on the contrary decomposes it at a temperature below red heat—boracic acid, potash, and ammonia, being the products; all those bodies which retain water at an elevated temperature decompose it, as caustic potash, phosphoric acid, lime, &c. It is probable that its composition is KB**N**. By heating the cyanides of the different metals with boracic acid, similar compounds are formed. In heating in the same manner 6 parts of silicic acid and 13 parts of cyanide of potassium, a brittle porous mass s obtained, which after washing affords ammonia by fusion with caustic potash.

Formation of Ammonia.—M. Rusèt (Jour. de Phar. et de Chim. II, 257) has shown that a mixture of hydrogen gas and nitric oxide, passed through a tube containing heated peroxide of iron, gives rise to the formation of ammonia; in the space of half an hour, he obtained sufficient to saturate an ounce of hydrochloric acid. The oxide of iron exercises simply a catalytic action, and is not reduced so long as there is no excess of hydrogen.

Calomel.—It is known that the medicinal action of calomel is more energetic in proportion as the powder is finer. M. Soubeiran (Jour. für pr. Chem. XXVI, 414) effects this pulverization by means of a bellows, which drives a current of air through a large glass tube, containing the calomel heated so as to volatilize it. The current of air condenses the vapors, and carries them in the form of a very fine powder into an elongation of the tube connected with a recipient of water. M. Righini (same journal) has shown that when the air is replaced by the vapor of water, a small quantity of corrosive sublimate is formed.

A New Method of Precipitating the Sulphurets of Metals.—M. Himley (Ann. der Chim. und Pharm. XLII, 347) has made known a new method of precipitating the metallic sulphurets without the use of sulphuretted hydrogen. It consists in mixing the metallic solution with the hyposulphite of soda; this salt in most cases produces no precipitate, but if hydrochloric acid is added, the metallic sulphuret is immediately precipitated. All the metals precipitated by sulphuretted hydrogen, are also precipitated by this method. It is M. Himley's intention to examine into the accuracy of this method by new experiments.

Determination of the Copper in a Solution of a Binoxide Salt.—M. Levol (Ann. de Chim. et de Phys. V, 381) has made an important modification to M. Fuch's method for determining the copper contained in a solution of a binoxide salt, by the quantity of copper required to reduce it to the state of a protoxide salt. He places the solution of the salt in a vial, adds ammonia until the liquid becomes of a transparent blue, fills up the vial with boiling water, introduces a bright slip of copper, and closes it lightly,—when the liquid becomes colorless, the copper is withdrawn washed, dried and weighed. The method has the advantage of being applicable to all the salts of copper, even the nitrates.

To separate Manganese from Zinc dissolved in a Liquid containing an Excess of Hydrochlorate of Ammonia.—M. Otto (Ann. der Chem. und Pharm. XLII, 347) has proposed the following method for separating manganese from zinc, when they exist together in a solution containing much sal ammoniac. Ammonia is added, and the metals are precipitated by a current of hydrosulphuric acid; if acetic acid be added to the precipitates, the sulphuret of manganese will be dissolved alone.

To make Caoutchouc impermeable to Gas.—M. Chevreul (Journ. für Pr. Chem. XXVI, 35) has shown that linseed oil placed on the external surface of the caoutchouc renders it impermeable to gas.

The Influence of Colored Light upon Plants.—The result of these experiments (Journ. de Chim. Med. VIII, 645) has shown that plants prosper better under blue and violet colored glasses, and less under yellow and green, and do not grow at all under red glasses. It is known that when plants are exposed behind ordinary glass, they direct themselves toward the light; now under the yellow and green they do not incline at all, and under red glasses they bend in an opposite direction to that from which the light comes. If these facts are confirmed, they will be very interesting, and merit the attention of those physiologists occupied with the study of vegetation.

Experiments upon the Inorganic Elements of Plants.—MM. Wiegmann and Polstorff cultivated plants in two different soils. These plants were the Vicia sativa, Hordeum vulgare, Avena sativa, Polygonum fagopyrum, Nicotiana tabacum, and Trifolium pratense. One of the soils was a quartzose sand from Kœnigslutter near Brunswick, which was first heated to redness to destroy all organic matter that it might contain, and then boiled for six hours with aqua regia,—finally the acid was washed out, and with it such matter as had been dissolved, and the sand then dried. The other soil was a mixture of this sand with the following ingredients and in the following proportions:

Sand, - - -	861·26	Phosphate lime, -	15·60
Sulphate potash, -	0·34	Humate potash, -	3·41
Chloride sodium, -	0·13	" soda, - -	2·22
Sulphate of lime, (anhyd.)	1·25	" ammonia, -	10·29
Chalk, - - -	10·00	" lime, - -	3·07
Carbonate magnesia, -	5·00	" magnesia, -	1·97
Oxide manganese, -	2·50	" alumina, -	4·64
Oxide of iron, - -	10·00	" iron, - -	3·32
Alumina (pure,) -	15·00	Humine (insoluble in water,)	50.00

The plants were moistened with distilled water free from ammonia, and were protected from external influences by being covered. In the sand they germinated and grew, but they were stinted,—some of them flowered, others did so imperfectly, but they produced no fruit, not even those which according to M. Boussingault bear fruit when cultivated in soils free from nitrogen compounds. In the mixed earth the plants grew luxuriantly; they flowered and produced ripe fruit.

A quantity of grain equal to that planted was calcined, and the composition of the ashes determined; when the plants had finished vegetating, they were dried and burnt, and the ashes weighed and analyzed. The principal results of these analyses, are that the ashes of the plants grown in the sand weigh in general twice as much as the ashes of the grains sown, and sometimes more. The ashes of the plants cultivated in the mixture, were two and a half times as great as those cultivated in the sand from the same weight of seed, and in the case of the tobacco five times as much. To account for the origin of that portion of ashes of the plants grown in the sand which did not originate from the seed, the sand was analyzed after being well washed with boiling water; it contained—

Silicic acid, - -	97·900	Magnesia, - -	0·009
Potash, - - -	0·320	Alumina, - -	0·876
Lime, - - -	0·484	Oxide of iron, -	0·315

The same sand was exposed during a month in water, through which carbonic acid was continually passed; the solution resulting from it was evaporated to dryness, and the residue analyzed. This last operation proved that the liquid had extracted silica, potash, lime and magnesia. From this it is readily seen that the sand here used, as well as feldspathic sand of the ordinary soil, furnishes plants with alkalies and earthy oxides. M. Wiegmann has proved in a conclusive manner the inaccuracy of the opinion which admits that elements are formed in the various parts of plants, by the following experiment. He sowed some seeds of cresses, in very small fragments of fine platinum wire contained in a platinum crucible, and watered it with distilled water. The cresses grew perfectly well, but the ashes of the plants had the same weight as the ashes of the seeds from which the plants were grown. The conclusions from these results are that inorganic matter is necessary to the organization of plants,—that where it fails they perish, although it is possible that the whole or a part of the inorganic matter found in the ashes is not indispensable to the growth of the plants, and it is possible that potash, soda, lime, magnesia, alumina and oxide of iron, may substitute one another, as in minerals when the earth contains more of one than of the other. There is still room for many modifications of these experiments.

Succinic Acid.—M. Ronalds has produced succinic acid by prolonged action of nitric acid upon wax, and also by the action of the same acid upon spermaceti. In this latter case, adipinic and pimilinic acids are formed; but by a continued action of the nitric acid, both of them may be converted into succinic acid.

Angelicic Acid.—M. Buchner, Jr. (Buchner's Report, XXVI, 263) has discovered in the root of the *Angelica archangelica*, a new acid which he has called angelic acid, and which resembles valeric acid.

Opianic Acid.—MM. Wöhler and Liebig (Ann. der Chem. und Pharm. XLIV, 126) have discovered a new vegetable acid, which is the product of the action of dilute sulphuric acid and oxide of manganese upon narcotine. It is called opianic acid.

New Acid from Sugar.—M. Malaguti (L'Institute, No. 450, p. 279) has found that when starch sugar is heated in a solution of acetate of copper to a temperature between 80° and 100° cent., so that the oxide of copper is precipitated, there is a disengagement of carbonic acid and the formation of a new acid.

Preparation of Quinine.—M. Calvert (Journ. de Chim. et de Pharm. II, 388) has called attention to the solubility of quinine in lime water or the milk of lime, which is the reagent ordinarily used to precipitate the bases from their combination with the hydrochloric acid used to extract them from the bark. He proposes to avoid this inconvenience by almost saturating the hydrochloric acid with carbonate of soda, driving off the carbonic acid, and precipitating with caustic soda, which dissolves hardly a trace of quinine. In this manner a much larger quantity of quinine is obtained, and this excess more than repays the cost of the reagent. Lime water does not dissolve cinchonine, and therefore may be used to detect the presence of it in quinine, taking care to use a considerable portion of lime water.

Quinoline.—M. Gerhardt (Journ. für Pr. Chem. XXVIII, 76) has examined the reaction of potash upon several of the vegetable bases aided with a high temperature, and he has found that a volatile and oleaginous base is produced, which is called quinoline.

Cinchovatine.—M. Manzini (Ann. de Chim. et de Phys.) has discovered in the bark of the Cinchona ovata a new vegetable base, which he has called cinchovatine.

Bleaching Oils.—M. Payen (Jour. de Chim. Med. VIII, 121) had described a method in use in England for bleaching palm oil, which is applicable to all oils that do not easily become rancid. It consists in placing a thin coat of the oil upon the surface of water contained in flat vessels, and maintained at a temperature of 100° cent. by means of a tube communicating with a boiler; the oil is at the same time exposed to the action of the air and of the sun's rays. The oil is bleached in this way in ten or fifteen hours.

Adulteration of the Volatile Oils with Alcohol.—M. Lipowitz (Pharm. Cent. Blatt. 1842, 415) mixes two parts of the adulterated essence with one of a saturated solution of common salt, and agitates the two together after allowing some time for the separation to take place, the extent of the adulteration may be determined by what remains of the essence.

An Easy Method of preparing the Ethers of the Vegetable Acids.—M. Gaultin (Ann. der Chem. und Pharm. XL, iii) proposes a method for preparing directly these ethers without the intervention of any mineral acid. He heats the acid in a tubulated retort to as high a temperature as possible without decomposing or subliming it; then lets fall the alcohol drop by drop upon it; the ether is immediately formed and distils over. He has experimented with oxalic, citric, succinic and benzoic acids.

Gouty Calculus.—M. Marchand (Journ. für Prac. Chem. XXVI, 95) has analyzed a gouty calculus formed in the articulation of the knee, and found it composed as follows:

Lithenate of soda, -	34·20	Animal matter, - -	32·53
Lithenate of lime, -	2·12	Water, - - - -	6·80
Carbonate of ammonia,	7·86	Loss, - - - - - -	2·37
Common salt, - - -	14·12		

Mucilagenous Meteorite.—M. Mulder (Scheik Arderzock, 1st St. 34) has examined the mucilagenous matter that is found sometimes in the morning upon the grass, the origin of which is not known. It has been considered a *tremella* swollen by the dew,—also supposed to be a body thrown from some falling star. The experiments of M. Mulder put this question beyond a doubt, in proving that it is an animal mucilage, which has been swollen by water to the greatest degree, so that the solid mucilage constitutes but a very small portion of the mass. He found in it bone earth, a trace of proteine, a little lactate of soda, and common salt. The elementary analysis gave—

Carbon, - -	50·53	Nitrogen, - -	9·27
Hydrogen, - -	6·53	Oxygen, - -	33·67

As to the origin of the mucilage, M. Mulder supposes that it is a mucilage of frog's spawn, swollen by water.

Art. XIV.—*Bibliographical Notices.*

1. De Candolle, *Prodromus systematis naturalis Regni Vegetabilis, sive Enumeratio contracta Ordinum, Generum, specierumque Plantarum huc usque cognitarum, juxta methodi naturalis normas digesta: Editore et pro parte auctore* Alphonso De Candolle. Pars VIII, sistens Corollifl. Ord. xiii. Paris, 1844. pp. 684, 8vo.—We can at length announce the publication of this important volume; the first of the series under the editorship of the son of the great Genevan Botanist, and which he has appropriately dedicated to the memory of his illustrious father.* We are glad to state, that arrangements have been made to expedite the publication of the succeeding volumes. The printing of the ninth, it is said, has already commenced, and its appearance may be expected in the autumn of the present year. It will contain the *Loganiaceæ*, *Bignoniaceæ*, *Cyrandraceæ*, *Sesameæ*, and *Borragineæ*, from the notes prepared by the late Prof. De Candolle; the *Hydrophyllaceæ*, by Alph. De Candolle; the *Gentianaceæ*, by Grisebach; the *Polemoniaceæ*, by Bentham, and the *Convolvulaceæ*, by Choisy. The tenth volume will be occupied with the *Solanaceæ*, by Dunal, and the *Scrophularineæ*, by Bentham; in the elaboration of which orders, these two distinguished botanists are now actively engaged. Before noticing the contents of the present volume, it may be worth while to say, that, in the hands of the new publishers, (Fortin, Masson & Co. of Paris,) its typographical appearance is greatly improved; and that the price of the whole work is much reduced. Although details of this sort belong rather to an advertisement than to a critical notice, yet, as most of our botanical readers throughout the country, may not meet with the publisher's announcement, we may oblige them by stating, that the price of the whole eight volumes is ninety four francs; that of each of the seven earlier volumes, thirteen francs; that of the eighth volume separately, sixteen francs. The first order in the volume before us, the *Lentibulariæ*, is prepared by the editor. The North American species of *Utricularia*, are distributed into three sections, viz. 1. *Megacista*, where the verticallate foliage is floated by inflated petioles: 2. *Lentibularia*, where the capillary segments of the submersed foliage are utriculiferous; and 3. *Oligocista*, where the leaves are few, undivided, and disappear after flowering; the roots strike into the soil or mud, and generally bear the utriculi, when these are present. *U. resupinata*, discovered by B. D. Greene, Esq., and first mentioned in the Massachusetts Catalogue of

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* "*Memoriæ suavissimæ Parentis Optimi Alphonsius filius patria Vestigia passu licet non æquo persequutus pio animo dedicabat.*"

Plants and Animals, 1835, is wrongly placed by De Candolle among the yellow-flowered species of the second section. It has purple flowers and should stand next *U. purpurea.* The name *U. Greenei*, Oakes, in Hovey's Mag. must stand as a synonym ; as there is no good reason for changing the prior name imposed by the discoverer.

The order *Primulaceæ* is elaborated by M. Duby of Geneva ; who follows Endlicher in the general distribution of the family. From some inadvertance, *Glaux maritima* is not cited as an American plant. *Naumburgia thyrsiflora*, Mœnch, = *Lysimachia thyrsiflora*, Linn. and *L. capitata*, Pursh. The *L. revoluta*, Nutt., is referred to *L. longifolia*, Pursh. The common *Samolus* of the south western United States, which has smaller flowers than the true *S. Valerandi*, is referred to *S. floribundus*, H. B. K. *S. ebracteatus*, is not noticed as a plant of the United States, although it is common along our southern borders ; nor is it distinguished even as a subgenus, although, on account of its nearly free ovary and want of sterile filaments, a recent writer (M. Baudo, in *Ann. Sci. Nat.*, *Dec.* 1843,) has separated it, to form his genus *Samodia.* In the *Myrsinaceæ*, elaborated by the editor, we meet with two North American species, both natives of Florida, viz. *Myrsine Floridana*, Alph. DC., and *Ardisia Pickeringia*, Torr. & Gr. To the small order *Theophrastaceæ*, Alph. DC., our author has joined *Jacquinia*, a West Indian genus, one species of which extends into Florida. In the order *Sapotaceæ*, the editor has proposed one new North American species of *Bumelia.* In *Ebenaceæ*, we have only our Persimmon. From this, the order *Styraceæ* (embracing *Symplocineæ* and *Halesiaceæ* of Don,) is distinguished chiefly by the position of the cells of the ovary opposite the lobes of the calyx. *Hopea* is kept as a mere section of *Symplocos ;* including a dozen Asiatic species as well as our *S. tinctoria.*

The order *Oleaceæ* is published from the manuscripts of the late Prof. De Candolle. The American species of *Fraxinus* still require the labors of a monographer.

The order *Jasmineæ* is made to comprehend *Bolivaria*, (of which there is at least one Texan species,) and *Menodora* ; and the family *Bolivariaceæ* is shown to have been founded upon misconceived characters.

For the elaboration of the *Apocynaceæ*, we are indebted to the younger De Candolle. The only North American genera are *Amsonia* (is not *Echites Fraseri*, Roem. and Schultes the *A. ciliata*, Walt. ?) *Apocynum* and *Forsteronia*, (*F. difformis*, DC.,=*Echites difformis*, Walt.)

The order *Asclepiadeæ* has been very faithfully studied by De Caisne. All the North American representatives belong to the tribe of *True Asclepiadeæ*, with the exception of *Gonolobus*, of which we have several species, (one of them, collected by Dr. Short, forms the new *G. tiliæfo-*

lius,) and one, or possibly two species of *Chthamalia*, DeCaisne. *Metastelma Fraseri* is probably a native of the West Indies, not of Carolina. *Enslenia albida*, we notice, is about to be figured in the forthcoming volume of Delessert's Icones; as also is *Podostigma*. *Acerates* includes ten, chiefly North American species. *Asclepias* is reduced to forty four species, all of which are American, and the greater part extra-tropical. We are happy to learn that the plates of the fifth volume of the *Icones Selectæ* of the liberal Delessert—chiefly devoted to the illustration of the eighth volume of the Prodromus—are already in the hands of the engraver. A. Gr.

2. *Repertorium Botanices Systematicæ;* auctore Guil. Ger. Walpers. Leipsic, 1842—4. 2 vols. 8vo.—This work constitutes a supplement to De Candolle's Prodromus, and purports to contain the species which have been published since the appearance of the successive volumes of the latter. The first volume, (fasc. I–V,) comprising 950 pages, brings up the arrears as far as the end of the *Leguminosæ*. We understand that the second volume, which finishes the work, is also completed; but of this we have as yet received only three fasciculi, which carries the enumeration to the end of the genus Aster. When the remaining fasciculi reach us, we may, perhaps, offer some annotations respecting the synonymy of some American plants, for which we have not room at present. A. Gr.

3. *Enumeratio Plantarum omnium hucusque cognitarum, secundum Familias Naturales disposita;* auctore C. S. Kunth, Prof. Univ. Berol, &c. &c. Tom. IV. Stuttgardt and Tübingen, 1843. pp. 752, 8vo.—This work is making such progress in the ascending series, that the publication of the Endogenous plants will probably be finished about the time that the Prodromus of De Candolle reaches the lowest Exogenous orders. We shall then have a complete *Species Plantarum* for Flowering Plants; a consummation which we hope may be effected in the course of eight or ten years—possibly by the year 1850. In this volume, Professor Kunth has given the *Xyrideæ*, *Mayaceæ*, (Mayaca or Syena,) *Commelyneæ*, *Pontederiaceæ*, *Melanthaceæ*, *Uvularieæ*, *Liliaceæ*, (=Tulipaceæ, *De C.*) to which he refers Medeola; and finally the *Asphodeleæ*, which fill more than half of the volume. We shall take another opportunity for some critical observations upon the *Melanthaceæ*. We will here only repeat our former remark, that the name of *Melanthium* must be restored to the American species, upon which the genus was established. From *Uvularia puberula*, Michx., the synonym of Richardson must be excluded. The plant, which is truly a distinct species, is confined to the mountains of Virginia, Caro-

lina, &c. where it takes the place of *U. sessilifolia.* To *Prosartes Menziesii*, belongs of course *Uvularia Smithii.* The *Streptopus maculatus* of Mr. Buckley, (described in Vol. XLV, p. 170,) forms a third, and the most showy species of Prosartes; as the flowers are nearly an inch long, and the perianth beautifully spotted with purple dots. It may be thus characterized:

PROSARTES MACULATA: umbellis sessilibus bifloris, sepalis lanceolatis acuminatis patentibus (albidis maculis purpureis conspersis) basi saccatis, staminibus styloque glabro perianthium subæquantibus.—In montibus '*Cumberland*,' Tennessee, legit cl. *S. B. Buckley.*

There seems to be no good reason whatever for separating *S. roseus* from the genus *Streptopus.* A. GR.

4. *Endlicher*, *Mantissa Botanica, sistens Generum Plantarum Supplementum Tertium.* Vienna, 1843.—We noticed in Vol. XLIV, p. 198, the preceding part of this valuable supplement to the excellent Genera Plantarum: that for 1843 has just reached us. About one half of it is devoted to a thorough revision of the *Algæ*, with a complete enumeration of the species; to which is appended an account of the fossil species by Unger. The author has availed himself of the highly important contributions to our knowledge of this tribe of plants which have recently been made by J. G. Agardh, Meneghini, Montagne, Kützing, and especially by DeCaisne; but Kützing's admirable *Phycologia generalis* (Leipsic, 1843, Gr. 4to, with 80 colored plates) had not made its appearance when the sheets of this supplement were printed. A full Bibliotheca of algological writings is prefixed to this part of the work, in which three hundred and seventeen authors in that department are alphabetically enumerated. A revision of the *Cyperaceæ* is promised for the Supplement for the year 1844. We are sorry that Prof. Endlicher still continues to change generic names which have been earlier employed in zoology;—an endless task, if fully carried out, and surely needless. But certainly Acanthocephalus, *Karel*, (non Helminthol,) should not be replaced by *Harpocarpus*, (Endl. suppl. 3, p. 70, while there is an earlier genus of Compositæ, bearing the essentially identical name of *Harpæcarpus.* (Vide Suppl. 2, p. 45.) And further, as priority in publication is always to be regarded, we protest against the adoption of the *Engelmannia* of Klotzsch, to the exclusion of the prior *Engelmannia*, Torr. & Gr. published in Nuttall's paper on Compositæ in the Transactions of the American Philosophical Society, which Endlicher here changes into the more euphonious name of *Angelandra*; although he at the same time recognizes the undoubted priority of the American publication, by the suppression of Klotzsch's *Tuckermania*, in favor of the earlier one of

Nuttall published in a still later number of the Transactions of the American Philosophical Society. It is true that Endlicher, in his second Supplement, has omitted the *Engelmannia*, as well as a few other of the genera of Nuttall's paper; but that is entirely his own fault; and the *Engelmannia*, Torr. & Gr. must undoubtedly retain that name. A. Gr.

5. *The Botany of the Voyage of H. M. Ship Sulphur, under the command of Capt. Sir Edward Belcher, R. N., &c. during the years* 1836–42. By Richard Brinsley Hinds, *Surgeon R. N. attached to the Expedition; the Botanical descriptions by* George Bentham, Esq. No. 1. London, 1844. pp. 16, imp. 4to, with 10 plates.—Mr. Hinds first notices the physical and phytostatical features of the coast of north-west America, at least of the portion visited by the Sulphur in her surveying voyage, extending from lat. 60° 21′ to 46° 19′. As his remarks are brief, and just now particularly interesting on some accounts, we cite them entire.

"The whole territory"—that is, reaching south to the mouth of the Oregon River—"though extensive, is remarkably uniform in its physical character and natural productions. The climate is far more moderate than on the eastern coast, not liable to those great vicissitudes, nor ever known to display any great range of temperature. The number of rainy days in the year is very great, and at Sitka only thirty seven really fine clear days were recorded throughout this period. At this Russian settlement, some extended observations gave the mean temperature of the year as 45° 5′, and the range as from 2° 3′ to 81° 9′. The whole country is bold and mountainous, intersected by deep and moist valleys, and is every where covered by a gloomy forest of spruce. These vast forests offer a scene which powerfully arrests attention. The trees are often of enormous dimensions, stretching upwards with scarcely a branch to where the eye almost fails to follow them, with enormous trunks very deceptive till brought within the scope of our experience by the tape-line: beneath, a most luxuriant undergrowth everywhere abounds, and has an exuberance and charm about it which is rarely supposed to be possible beyond the tropics. But over these the influence of the moist climate is unceasing. It most probably hurries through a rapid existence the more lowly shrubs, and its effect on the trees is very marked. None are seen to attain any great age; that is, none have that appearance; but when the vigor of life is past, they rapidly yield to the constant influence of the moist atmosphere, soil, and investment of mosses and lichens, and soon fall to the ground, which in some places they occupy in great numbers. But, as is every where observable where the climate is uniform, the variety of species is not great, and some will occur with multitudes of individuals covering a very large space. It is curious to observe how tenaciously some genera extend throughout this territory, though continually represented by a different species. This is particularly conspicuous with *Vaccinium*, *Rubus*, *Rosa*, and *Lupinus*. The former has several deciduous species towards the northern portion, but towards the south they become neat evergreen shrubs, with a myrtle-like foliage. To a European, the general features of the country are entirely such as he is familiar with, only modified by the character of the climate and country; with the exception that there are two common plants, *Panax horridum* and *Dracontium Camtschaticum*, which differ so entirely from the surrounding vegetation as to exert a very considerable influence on the physiognomy."

This description applies to the whole region bordering the coast. Farther back an arid, desert region succeeds, doomed to perpetual sterility, of which a good account will be found in the journal of the late Mr. Douglas.

As Mr. Hinds did not obtain any entirely new species of plants along the northwest coast, no enumeration or further account of the collection is given.

Late in the autumn of 1837, the Sulphur touched at some parts of Upper California, at a season very unfavorable for herborization; and an expedition up the Rio Sacramento penetrated from San Francisco some distance into the interior. "The country exhibited a vast plain, rich in a deep soil, and subject to periodical submersion. Occasional clumps of fine oaks and planes imparted an appearance of park-land. . . . . On quitting the coast for the interior, we exchanged the evergreen oaks for deciduous species. The latter grow to fine trees, with wood of great specific gravity. But the natives have a very pernicious practice of lighting their fires at the bases; and as they naturally select the largest, it was really a sorrowful sight to behold numbers of the finest trees thus prematurely and wantonly destroyed. And it is not a country where wood is superabundant; for no sooner is the Oregon crossed than the spruce forests disappear, and the prevailing trees are oaks, which towards the south become gradually less abundant. But Upper California had already been tolerably examined, and it was our good fortune to touch rapidly at several places on the coast of the Lower, or New California, during October and November, 1839; and here we trod in no footsteps, as none had preceded us."

Mr. Hinds confirms our previous impression that the two Californias are essentially different in many respects, and especially in their vegetation; and that San Diego, their political place of separation, stands on the southern boundary of the proper extra-tropical flora. Thus, the account of the Californian collections, which, following the order of De Candolle's Prodromus, extends to Paronychiaceæ, does not embrace a single addition to the species already described in Torrey & Gray's Flora of North America, which comprises Upper California alone ; while the collections south of that limit were mostly new as to species, which almost exclusively belong to tropical or Mexican forms. There Cactaceæ and Euphorbiaceæ prevail, Malpighiaceæ and Burseraceæ make their appearance ; and the Mangrove with its companion the Laguncularia make their appearance on the shores ; their northern limit, being the Bay of Magdalena, in lat. 24° 38′. The plant doubtfully referred by Hooker and Arnott to *Vitis Caribœa*, is here characterized as a distinct species under the name of *V. Californica.*

A. Gr.

6. *Sertum Plantarum, or Drawings and Descriptions of Rare or Undescribed Plants, from the author's Herbarium;* by H. B. Fielding, F. L. S. and R. G. S., assisted by George Gardiner, F. L. S. London, Bailliere, Part I, 8vo.—This work is on the same plan as Hooker's excellent *Icones Plantarum;* the figures being executed in neat lithography, and each with a single page of descriptive letter-press. Four parts are announced, each to comprise, like the one before us, twenty five plates ; but as Mr. Fielding's truly rich herbarium, (founded on that of the late Mr. Prescott, of St. Petersburgh and enhanced by the choicest portions of the late Mr. Lambert's collections, and from many other sources,) will long furnish a series of most appropriate and important subjects for illustration, we trust his zeal will lead him to continue the work. Among the more remarkable plants here figured, we notice *Acacia smilacifolia*, with phyllodia simulating the leaves of a Smilax; and the Brazilian *Passiflora speciosa*, with bright scarlet flowers five or six inches across. The only North American species is *Silene Nuttallii*, Torr. & Gr., from Drummond's Texan collection.

A. Gr.

7. *Cours Élémentaire de Botanique*, par M. Adr. de Jussieu. Paris, Part II. pp. 227–728, 18mo.—The rapid multiplication of elementary works upon botany of an increasingly excellent character, affords a just indication not only of the actual progress of the science in the hands of a few devotees, but of the zeal and thoroughness with which it is now generally pursued, and of the rank which it is beginning to hold in a liberal education. We should like to commence the study of botany anew, with this volume of Jussieu for a text-book. In a small compass, and in a treatise of a really elementary character, he has given the most complete and lucid exposition of the science that we have

yet met with. We have already hastily noticed the first part of the work, (Vol. XLVI, p. 195.) The second is principally occupied with the Organs and Functions of Reproduction; in which all the recent discoveries are incorporated, and novel theories presented or briefly discussed. The principles of classification are then considered, and the leading natural families are neatly characterized, and many of them illustrated—like the organographical part of the work—with beautifully executed wood cuts. A chapter on Geographical Botany, with a cursory notice of fossil plants, concludes a volume upon which it would be difficult to bestow exaggerated praise. A. Gr.

8. *Jahresbericht über die Arbeiten für Physiologische Botanik im Jahre*, 1841; von Dr. H. F. Link. Berlin, 1843. pp. 79, 8vo.—The interesting yearly Reports on the progress of Vegetable Physiology, by the late Prof. Meyen, which were contributed to *Wiegmann's Archiv für Naturgeschichte*, are generally known to English readers through the translations prepared by Mr. Francis, and published either in a separate form, or in Taylor's London and Edinburgh Philosophical Magazine. Since the decease both of Prof. Meyen and of Prof. Wiegmann, these reports have been drawn up by the veteran Professor Link, and published in Erichson's continuation of the same scientific journal, and also in a substantive pamphlet form. We have just received the Report for 1841, the second of the present series, which was published only a few months since; and would only remark, that these almost indispensable summaries are not likely to decline in interest in the able and impartial hands of Professor Link. We may here briefly notice another work by the same author, viz.

9. *Anatomia Plantarum Iconibus Illustrata*, auct. H. F. Link, Fasc. 1. cum tab. lithogr. xii, (with a corresponding German title.) Berlin, 1843.—This is the first part of what appears to be in fact a continuation, in a cheaper and quarto (instead of folio) form, of those fine illustrations of vegetable anatomy which we announced in this Journal at the time of their publication. The text consists merely of the explanation of the plates, which are executed in admirable lithography. A. Gr.

10. *Crania Ægyptiaca, or Observations on Egyptian Ethnography derived from Anatomy, History and the Monuments*; by Samuel Geo. Morton, M. D., Author of Crania Americana, &c.; 4th part, pp. 67, 14 plates. (From the Transactions of the Am. Phil. Soc., Vol. IX.) Phila. 1844.—The previous labors of Dr. Morton as a craniologist, have placed his name in the first rank of ethnographical science, and given

him a reputation in this department of human knowledge which is not diminished by his eminence in others. The able pen of a distinguished foreign craniologist (then in this country) has already recorded in these pages (Vol. xxxviii, p. 341) the verdict of an acute mind accustomed to reason on these subjects, of the uncommon value of that performance.

We confine our remarks on the "Egyptian Ethnography" to a few extracts in the words of the author. Dr. Morton says in his introduction, that he has through the kindness of Mr. Gliddon, been able to examine a great number of Egyptian skulls, and to make extensive comparisons by means of nearly six hundred human crania, which form a part of his anatomical collection.

It was remarked fifty years ago by the learned Professor Blumenbach, that a principal requisite for an inquiry such as we now propose, would be "a very careful, technical examination of the skulls of mummies hitherto met with, together with an accurate comparison of these skulls with the monuments." This is precisely the design I have in view in the follewing memoir, which I therefore commence by an analysis of the characters of all the crania now in my possession. These may be referred to two of the great races of men, the Caucasian and the Negro, although there is a remarkable disparity in the number of each. The Caucasian heads also vary so much among themselves as to present several different types of this race, which may, perhaps, be appropriately grouped under the following designations:—

CAUCASIAN RACE.

1. The *Pelasgic* Type.* In this division I place those heads which present the finest conformation, as seen in the Caucasian nations of western Asia, and middle and southern Europe. The Pelasgic lineaments are familiar to us in the beautiful models of Grecian art, which are remarkable for the volume of the head in comparison with that of the face, the large facial angle, and the symmetry and delicacy of the whole osteological structure.

2. The *Semitic Type*, as seen in the Hebrew communities, is marked by a comparatively receding forehead, long, arched, and very prominent nose, a marked distance between the eyes, a low, heavy, broad and strong, and often harsh development of the whole facial structure.

3. The *Egyptian* form differs from the Pelasgic in having a narrower and more receding forehead, while the face being more prominent, the facial angle is consequently less. The nose is straight or aquiline, the face angular, the features often sharp, and the hair uniformly long, soft and curling. In this series of crania I include many of which the conformation is not appreciably different from that of the Arab and Hindoo; but I have not, as a rule, attempted to note these distinctions, although they are so marked as to have induced me, in the early stage of the investigation, and for reasons which will appear in the sequel, to group them, together with the proper Egyptian form, under the provisional name of *Austral-Egyptian* crania. I now, however, propose to restrict the latter term to those Caucasian communities which inhabited the Nilotic valley *above* Egypt. Among the Caucasian crania are some which appear to blend the Egyptian and Pelasgic

* I do not use this term with ethnographic precision; but merely to indicate the most perfect type of cranio-facial outline.

characters: these might be called *Egypto-Pelasgic* heads; but without making use of this term, except in a very few instances by way of illustration, I have thought best to transfer these examples from the Pelasgic group to the Egyptian, inasmuch as they so far conform to the latter series as to be identified without difficulty.

NEGRO RACE.

The true *Negro* conformation requires no comment; but it is necessary to observe that a practised eye readily detects a few heads with decidedly mixed characters, in which those of the Negro predominate. For these I propose the name of the *Negroid* crania; for while the osteological development is more or less that of the Negro, the hair is long but sometimes harsh, thus indicating that combination of features which is familiar in the mulatto grades of the present day. It is proper, however, to remark in relation to the whole series of crania, that while the greater part is readily referrible to some one of the above subdivisions, there remain other examples in which the Caucasian traits predominate, but are partially blended with those of the Negro, which last modify both the structure and expression of the head and face.—pp. 3, 4.

We extract the following valuable ethnographic table, p. 19.

*Ethnographic Table of one hundred ancient Egyptian Crania.**

Sepulchral localities	No.	Egyptian.	Pelasgic.	Semitic.	Mixed.	Negroid.	Negro.	Idiot.
Memphis,	26	7	16	1	1	1		
Maabdeh,	4	1	1			2		
Abydos,	4	2	1	1				
Thebes,	55	30	10	4	4	5		2
Ombos,	3	3						
Philæ,	4	2	1				1	
Debôd,	4	4						
	100	49	29	6	5	8	1	2

The preceding table speaks for itself. It shows that more than eight tenths of the crania pertain to the unmixed Caucasian race; that the Pelasgic form is as one to one and two thirds, and the Semitic form one to eight, compared with the Egyptian: that one twentieth of the whole is composed of heads in which there exists a trace of Negro and other exotic lineage:—that the Negroid conformation exists in eight instances, thus constituting about one thirteenth part of the whole; and, finally, that the series contains a single unmixed Negro.

The conclusions to which Dr. Morton comes, after his careful and laborious examination, are as follows:

CONCLUSIONS.

1. The valley of the Nile, both in Egypt and in Nubia, was originally peopled by a branch of the Caucasian race.

* It will be observed, on comparing this table with the original one published in the *Proceedings* of the Society for December, 1842, (and since republished in Mr. Gliddon's *Ancient Egypt*,) that there is a great difference in the relative number of Pelasgic and Egyptian heads; which fact has been already adverted to, and explained, (page 4.) I have been governed, in the present classification, by the manifest presence of the Egyptian physiognomy, even in those instances in which it appears to be blended with an equal and even preponderating Pelasgic character. It will be observed, however, that *the whole number of Circassian heads* is nearly the same in both tables; and that the relative proportion of Semitic, Negro and Negroid crania is unaltered.

2. These primeval people, since called Egyptians, were the Mizraimites of Scripture, the posterity of Ham, and directly affiliated with the Lybian family of nations.

3. In their physical character the Egyptians were intermediate between the Indo-European and Semitic races.

4. The Austral-Egyptian or Meröite communities were an Indo-Arabian stock grafted on the primitive Lybian inhabitants.

5. Besides these exotic sources of population, the Egyptian race was at different periods modified by the influx of the Caucasian nations of Asia and Europe,—Pelasgi, or Hellenes, Scythians and Phenicians.

6. Kings of Egypt appear to have been incidentally derived from each of the above nations.

7. The Copts, in part at least, are a mixture of the Caucasian and the Negro in extremely variable proportions.

8. Negroes were numerous in Egypt, but their social position in ancient times was the same that it now is, that of servants and slaves.

9. The national characteristics of all these families of man are distinctly figured on the monuments; and all of them, excepting the Scythians and Phenicians, have been identified in the catacombs.

10. The present Fellahs are the lineal and least mixed descendants of the ancient Egyptians; and the latter are collaterally represented by the Tuaricks, Kabyles, Siwahs, and other remains of the Lybian family of nations.

11. The modern Nubians, with a few exceptions, are not the descendants of the monumental Ethiopians, but a variously mixed race of Arabs and Negroes.

12. Whatever may have been the size of the *cartilaginous* portion of the ear, the osseous structure conforms in every instance to the usual relative position.

13. The Teeth differ in nothing from those of other Caucasian nations.

14. The Hair of the Egyptians resembled, in texture, that of the fairest Europeans of the present day.

15. The physical or organic characters which distinguish the several races of men, are as old as the oldest records of our species.

11. *Ehrenberg's Researches on the Distribution of Microscopic Life.*—The comparison of the microscopic organisms of Europe with those of other countries, which Ehrenberg commenced in his paper on the American forms, of which an account appeared in this Journal, (Vol. XVI, p. 297,) has been continued by him with true German industry, and his examinations have now been extended to Asia, Africa, and Australia. Aided by his method of examining the portions of soil clinging to the roots of plants in herbaria, and by contributions from travellers, of Algæ and earths containing infusoria, he has been enabled to give a comprehensive view of the minute living forms of an immense portion of the surface of the earth. Abstracts of several of Ehrenberg's memoirs upon this subject are given in the monthly reports of the Academy at Berlin for March and May, 1843, and from these we have selected the following as being of most interest and importance.

In his memoir on the Asiatic forms the author accompanied his remarks upon the importance of the most careful observation of the minutest organic forms, by the statement "that the granular portions of the oolitic limestone of the Jura formation, in Germany as well as in England, appears to be chiefly composed of the shells of Meloniæ."

He alluded to the different views which have been entertained with regard to the origin of granular (körnigen) oolitic limestone, and stated that the idea, that like peastone, it originated in an incrustation of various small fragments in a former sea having a high temperature, was improbable and untenable; for among the round grains with concentric coatings, like the peelings of an onion, which might be supposed to have had such an origin, there also often occurred calcareous or siliceous bodies of the same size, which showed no trace of incrustation, as for example spines of Echinæ, encrinital plates, fragments of shells, and minute Polythalamia. All these forms which had existed and remained in the same conditions, but which had received no shelly covering, prove that the shells of many oolitic grains can be no incrustations. Besides, the oolitic grains have generally a very similar limit with regard to size, while in the formation of peastone there is no limit.

The author stated that he possessed a piece of oolitic limestone from Baden, the grains of which showed in addition to the shells, longitudinal striæ, and chambers were visible in the cross section, so that the structure opposed the supposition of a mere calcareous deposition. The same structure was seen in the Meloniæ from the mountain limestone of Lake Onega in Russia, and by means of sections distinct views of the structure of the Meloniæ were also obtained, in a piece of hornstone from the mountain limestone of Tula, to which a Spirifer remained adhering, and which was crowded with various Polythalamian forms.

Ehrenberg states that in many cases "the Meloniæ of the oolitic limestone are so changed into calc spar that the shells are no longer divided. In other cases there is found in their interior a small kernel of calc spar which might easily lead to the supposition that a real incrustation of a grain of sea-weed had taken place, while it is in fact only the internal commencement of the change to crystalline calcareous spar, as may be recognized by the brilliant fracture.

The author states the microscopic organisms from Australia and New Holland present less peculiarity than was expected in consequence of the remarkable forms of the larger animals of those regions. Only one peculiar genus, Rhizonotia, was found, and all the forms belonged to well known orders, classes, and families.

The following are the general results from all these examinations.

"1. Microscopic life, particularly in the forms which constitute masses of earth and rock, appears to exist in the same manner over the surface of the whole earth.

"2. The results already obtained by direct examinations have proved, that in all zones of the surface of the earth, in all climates, in low situations, and the bottom of the ocean, as well as on high mountains at the elevation of about nine thousand feet, (Niglherri, Mexico,) and even in the smallest particles of humus, microscopic life has not merely an existence, but is in exuberant abundance.

"3. The European microscopic organisms have been shown to be so related to those of other parts of the earth, that new orders, classes, and families, are no where found, but the forms all belong to the often siliceous, never calcareous shelled Polygastric infusoria, and to the never siliceous but generally calcareous shelled Polythalamia, which are not infusoria.

"4. Besides these independent microscopic forms of life, there also are found in soil and in calcareous strata all over the earth, astonishing numbers of small undecomposed regular parts of larger organisms, which are either siliceous or calcareous, and both of vegetable and animal origin which every where present the closest resemblance in characters, however different may be the Flora or Fauna of the localities.

"5. In place of orders, classes, or families, peculiar to different parts of the world, there occur peculiar local genera, which however are no where numerous, while on the contrary there are very numerous peculiar species of widely distributed genera.

"6. Certain geographical latitudes have their characteristic forms of minute animal life. Thus the proportionally large and slender serrated species of Eunotia—*E. tetraodon*, *pentodon*, *diadema*, *serrulata*, &c.—have hitherto been found only in Sweden, Finland, and in North America, from New York to Labrador. Similar species of the broad and small, many-toothed species of *Himantidium* and *Eunotia* only occur on the south coast of Asia, at Senegal in Africa, and in Cayenne in South America. The genus Tetragramma is found only in Libya and in the Ladrone isles, and the same species occurs at both places.

"7. There is distributed over all parts of the earth a considerable number of perfectly identical forms, among which are (*Navicula*), *Pinnularia viridis*, *Himantidium arcus*, and *Eunotia amphioxys*. These most common forms appear to be the most important in their relations to the economy of nature.

"8. The so-called inorganic constituents of the body and shells of animalcules are chiefly carbon, silica, lime, and iron, with traces of alumina and manganese; magnesia and other substances are probably only present as mechanical mixtures.

"9. The quantity of iron in the minutest organisms is sometimes surprisingly great. It is never united with the lime, but only with the silica, appearing to be rather mechanically than chemically combined, and sometimes it seems to exist in a very peculiar, colorless, and chemically inexplicable state. This mechanical union of iron and silica appears to be chiefly an organic deposition of the metal in closed siliceous cells.

"10. In consequence of the uniform and extensive development of minute organic life, it must exert a great and important influence upon other conditions of the surface of the earth, and particularly upon the formation of humus in the valleys of rivers. If the larger organic bodies have direct relations to the conditions of the atmosphere, the widely extended and immensely developed minuter forms, cannot be without a great influence on those relations.

"11. But the evidence of the influence of microscopic life is not confined to the surface of the earth. The same incomprehensible formation of rocks from the siliceous or calcareous shells of animalcules which is seen in the chalk formation of Europe, occurs also on a gigantic scale both in the northeast and northwest of Africa, (Egypt, Oran.) It occurs in the northwest of Asia, (Bir Hamam, Ante Libanus, Libanus,) and according to recent observations in perhaps still greater development in North America, (Mississippi, Missouri, New Jersey.) The Jura limestones of Europe also show generally, and sometimes quite distinctly, an intimate connection with organic life, and the very ancient limestones and included chalcedony said to occur directly beneath the coal at Lakes Tula and Onega in Russia, occasionally show quite distinctly that microscopic life had as extensive a development in that ancient epoch as at any more recent period. Rocky masses of infusoria are presented by the polishing slates of Lucon and Caucasus, and extensive earthy beds of siliceous infusoria occur, not only in the edible clay of the Amazon, and the very extensive (fifteen to twenty feet thick) infusorial strata of Richmond, Virginia, mentioned by Rogers and Bailey, but also in the siliceous marl (Kieselguhr) in Siberia, and near Perth in New Holland.

"12. Finally, microscopic life is demonstrated to be a most important agent in the formation of the surface of the earth."

J. W. B.

12. *Experimental Researches, Chemical and Agricultural, showing Carbon to be a compound body, made by plants and decomposed by putrefaction;* by ROBERT RIGG, F. R. S. London, Smith, Elder & Co., 65 Cornhill, 1844. 12mo, pp. 264.—This is a startling title, and our conservatism at once takes the alarm; for it threatens to subvert the entire foundations of chemical science, and more especially those of physiological chemistry, which seemed to be just emerging out of its chaotic state into order and harmony. However, it claims to be only an experi-

mental inquiry into matters where certainly experiment and inquiry are yet much needed, and is therefore at least entitled to an unprejudiced and candid hearing. The volume is mainly occupied with a detailed account of a series of numerous experiments, made by the author, on the chemical changes that take place during the growth and decay of plants. From these the deductions are simply and directly drawn; so that whatever credit is given to them will depend mainly on the accuracy and value of the methods employed in the experiments, which are so fully detailed that they may be readily repeated and tested. A few of the author's most important deductions are subjoined.

1. Plants impart to the atmosphere more carbonic acid than they remove from it.

2. In the process of vegetation the carbon, which goes to the growth of the plant together with that given off in the form of carbonic acid, is very considerably greater than can be accounted for by its disappearance from all the sources by which the plant is supplied. Carbon is therefore a product of vegetation.

3. The quantity of carbon made by plants is affected by a variety of circumstances, such as the nature and quantity of the soil, temperature, miasm, saline and earthy bodies, carbonaceous matter in the soil, quantity of water supplied, and by the presence of carbonic acid in the atmosphere.

4. During the putrefactive fermentation a portion of carbon is decomposed; for it is not to be found in the carbonic acid disengaged, or in any other of the products, while in its place water and miasm are obtained.

The author adds at the close of his book, "My observations, during the whole course of my experiments, have led me to the conclusion that of the elements carbon, hydrogen, oxygen, nitrogen, sodium, potassium, calcium, &c., which constitute the organic and inorganic elements of plants, hydrogen is the only ultimate element, the rest being compound bodies; and to question the compound nature of hydrogen." Whatever may be thought of this, the experiments contained in this volume must be welcomed as a valuable contribution to our stock of knowledge in this obscure and intricate department of science.

G. H.

MISCELLANIES.

DOMESTIC AND FOREIGN.

1. *Notes on the Cretaceous Strata of New Jersey and parts of the United States bordering the Atlantic;* by C. Lyell.—The cretaceous formation of New Jersey resembles its European equivalent in mineral character, with the important exception that the white chalk with flints, and the chert, so common in our green sand, are wanting. The American strata, consisting of green sand and marl, red and highly ferruginous sandstones, white sand, limestone, and some beds of lignite, have been usually compared to our lower cretaceous series, with which they correspond in lithological character, but in their fossils they agree far more nearly with the European strata, ranging from the Gault to the Mæstricht beds inclusive.

Dr. Morton pointed out in 1834 the general agreement of the organic remains of these American strata with those of the chalk and green sand of Europe, while Mr. Conrad correctly pronounced almost all the species to be new and distinct.

Mr. Lyell, in an excursion in September, 1841, in company with Mr. Conrad, collected in New Jersey a large portion of the fossil shells described and figured by Dr. Morton, together with some new species. Having examined the whole, with the assistance of Mr. E. Forbes and others, he finds not more than four in sixty species of shells identical with European fossils. These are *Belemnites mucronatus*, *Pecten quinquecostatus*, *Ostrea falcata*, (*O. larva*, Goldfuss,) and *O. vesicularis*. Several others however approach very nearly, and may be the same as European species, and at least fifteen may be regarded as geographical representatives of well known chalk fossils of Europe, belonging for the most part to beds above the Gault. There are a few peculiar forms, such as *Terebratula Sayii*, a new species of Bulla, and others, unknown in Europe.

Prof. H. D. Rogers has divided the New Jersey beds into five formations, two of which are rich in organic remains. The lower of these consists chiefly of green sand or marl, the upper is a calcareous rock. The corals obtained in the latter by Mr. Lyell at Timber Creek, twelve miles southeast of Philadelphia, have been referred by Mr. Lonsdale to the following species—

Montivaltia Atlantica, (*Anthophyllum Atlanticum*, Morton,) *Idmonea contortilis*, sp. n., *Alecto fascicularis*, sp. n., *Cellepora tubulata*, *Escharina sagena*, (*Flustra sagena*, Morton,) *Eschara digitata*, Morton.

The same coralline rock contains *echinoderms* of the genera Spatangus, (Holaster, Agassiz,) Cidaris, and other forms closely allied to upper cretaceous fossils of Europe. It also abounds in Foraminifera characteristic of the chalk, comprising among others, the genera Cristellaria, Rotallina, and Nodosaria.

Mr. Owen has recognized in the fossil reptiles from New Jersey the vertebræ of Mosasaurus and Pliosaurus, and a large crocodile of the Procælian division. There are also many fish of the shark family, analogous to those of the English chalk, and the *Galeus pristodontus* of Europe is represented by a species very closely allied, if not identical.

At South Washington, in North Carolina, three hundred and fifty miles southwest of New Jersey, Mr. Lyell found cretaceous marls, containing *Belemnites mucronacus*, *Exogyra costata*, and other characteristic chalk fossils, some of them common to the lower, others to the upper fossiliferous group of New Jersey, besides several new species.

The author ascertained that the pebbly limestone of Wilmington, N. C., and the white limestone of the Santee river, S. C., and that of Shell Bluff, in Georgia, which had all been usually regarded as upper cretaceous, are in fact eocene tertiary rocks, containing no admixture of secondary fossils.

2. *On the probable age and origin of a bed of plumbago and anthracite, occurring in mica schist, near Worcester, Mass.;* by C. LYELL, F. G. S., &c.—A bed of plumbago and impure anthracite, described by Professor Hitchcock, in his Geology of Massachusetts, is found interstratified with mica schist, near Worcester, forty five miles due west of Boston. It is about two feet in thickness, and has been worked for coal and lead pencils. It is occasionally iridescent, like coal, and contains pyrites, which is also found in the associated clay slate and garnetiferous mica schist, both of which are impregnated with carbonaceous matter. These schists, including plumbago, are separated from the anthracite occurring on the borders of Rhode Island and Massachusetts, by a district of gneiss and hornblende slate about thirty miles wide. The anthracite of those states is impure and earthy, but has been worked for coal at Wrentham, Cumberlend, Mansfield, and other places, where, in the accompanying carbonaceous and pyritiferous shales, are seen numerous impressions of the most common coal plants, such as *Pecopteris plumosa*, *Neuropteris flexuosa*, *Sphenophyllum*, *Calamites*, *&c.* The shales and grits of these coal measures are very quartziferous, were formerly called greywacke, and have been shown by Prof. Hitchcock and Dr. Jackson to pass into mica schist and other metamorphic rocks, and to be invaded by syenite and trap. Mr. Lyell is of opinion that the stratified rocks containing the plumbago of Worcester, consisted originally of a similar carboniferous formation, but have since been so altered by heat and other causes, as to assume a crystalline and metamorphic texture, by which the grits and shales of the coal have been turned into quartzite, clay slate and mica schist, and the anthracite into

that state of carbon which is called plumbago or graphite. To render this hypothesis more probable, he adverts to the progressive debituminization of the coal of the United States, as we proceed from the horizontal coal measures of the Ohio and the west, to the eastern and more disturbed axes of the Apalachian mountains, where coal, occupying precisely the same geological position, and exhibiting the same species of fossil plants, assumes the form of anthracite, as has been shown by Prof. H. D. Rogers and others.

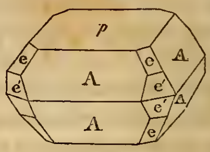
The Rhode Island anthracite may be considered as representing a further state of change, in which the volatile ingredients of the original coal have been still more completely expelled; and in the plumbago of Worcester we have the last step in the series of transmutation, where all traces of fossil plants and vegetable structure have been obliterated, and where the lithological character of the sedimentary rocks has been entirely altered. It is remarked that the Silurian formations, which are so largely developed in the United States, yield no beds of coal or anthracite which could by metamorphosis be supposed to become turned into such a carbonaceous stratum as that of Worcester.

The author concludes by observing that the difference of strike between the mica schist containing plumbago at Worcester, and the nearest carboniferous rocks of Rhode Island and Massachusetts, affords no argument against the theory of both having belonged originally to the same group of sedimentary strata. In New England and in Nova Scotia, the coal measures frequently deviate widely from the same strike in contiguous districts, and the direction of continuous anticlinal axes in the Alleghany Mountains, composed throughout of similar Silurian and carboniferous rocks, has been shown by Professors W. B. and H. D. Rogers to vary more than 40° in different sections of that chain.

3. *Hydrous Borate of Lime.*—This mineral, first made known by A. A. Hayes, has been analyzed by him with the following result: Boracic acid 46·111, lime 18·889, water 35, giving the formula $\dot{C}a\dddot{B}^2+6\dot{H}$. The crystals, figured on Mr. Hayes's authority in Dana's Mineralogy, p. 243, have been found on analysis to be glauberite. The borate of lime occurs in interlacing fibres of a snow-like whiteness. Besides glauberite, it is associated with gypsum, magnesian alum, and a native *iodate of soda*, a new species first pointed out by Mr. Hayes. It comes from near Iquique, S. A. (See this Journal, vol. XLVI, p. 377.)

4. *Anatase.*—This mineral has been lately examined by Damour. He obtained for the composition of a specimen from Brazil, Titanic acid 98·36, peroxyd of iron 1·11, oxyd of tin 0·20=99·67, according to which it is chemically identical with *rutile*. A specimen of rutile from St. Yrieux afforded him Titanic acid 97·60, peroxyd of iron 1·55=99·15. Specific gravity of the anatase 3·857. Descloiseaux has measured crys-

tals of this mineral from the same region, and infers from his investigations that the forms are distinct, although both belong to the *dimetric* system. The crystals of anatase are rarely octahedrons; the annexed figure is one of the forms. They differ from rutile in cleavage, and upwards of ·3 per cent. in specific gravity. If an instance of dimorphism, as it appears, it is remarkable that both forms belong to the same system of crystallization. (*Ann. Ch. Ph.* 3*d ser.*, *April* 1844, pp. 414 and 418.)



5. *Pennine.*—According to Marignac and Descloiseaux, the primary of this mineral is an acute rhombohedron of 63° 15′. This form is sometimes presented by minute crystals, but the larger are usually flat tables, arising from a truncation of the extremities. Specific gravity 2·653. An analysis of specimens from the valley of Zermatt afforded Silica 33·36, alumina 13·24, chromic acid 0·20, peroxyd of iron 5·93, magnesia 34·21, water 12·80, from which Marignac and Descloiseaux deduce the formula $2\dddot{\mathbf{A}}\text{l}, \dot{\text{M}}\text{g}+5\dddot{\text{S}}\text{i}, \dot{\text{M}}\text{g}^2\dot{\mathbf{H}}^2$. (Ibid. p. 427.) The above formula, omitting one atom of water, which the calculation according to the above gives in excess, may be written $2\dddot{\mathbf{A}}\text{l}\dddot{\text{S}}\text{i}+3\dot{\text{M}}\text{g}^3\dddot{\text{S}}\text{i}+3\dot{\text{M}}\text{g}\dot{\mathbf{H}}^3$, which, except in the proportions of the silicates, is similar to Varrentrapp's formula for chlorite.—D.

6. *Talc.*—This mineral from Chamouni, analyzed by Marignac and Descloiseaux, contains Silica 62·58, magnesia 35·40, protoxyd of iron 1·98, water 0·04=100, from which they obtain the formula $\dddot{\text{S}}\text{i}^3\dot{\text{M}}\text{g}^4$. (Ibid.)

7. *Dioptase.*—Damour has obtained for the formula of Dioptase, $\dot{\text{C}}\text{u}^3\dddot{\text{S}}\text{i}^2\dot{\mathbf{H}}^3$. His analysis gave Silica 36·47, oxyd of copper 50·10, water 11·40. (Ibid, p. 485.)

8. *Beaumontite.*—This mineral from the vicinity of Baltimore, Md., has been analyzed by M. A. Delesse with the following results: Silica 64·2, alumina 14·1, protoxyd of iron 1·2, lime 4·8, magnesia 1·7, soda and loss 0·6, water 13·4; from which he deduces the formula $\dot{\text{R}}\dddot{\text{S}}\text{i}^2+\dddot{\mathbf{A}}\text{l}\,\dddot{\text{S}}\text{i}^3+5\dot{\mathbf{H}}$. In a closed tube it whitens and intumesces, and a platina wire affords a white opaline pearl. With salt of phosphorus it forms easily a glass with a skeleton of silica. Specific gravity=2·25. (Ann. de Ch. et de Phys. 3e ser., ix, 385, 1843.) Notwithstanding the above results, Beaumontite is probably nothing but Heulandite. The primary is not a *square prism*, as given by Levy, which fact is evident from the dissimilar lustre on the faces assumed as the prismatic faces by Levy, and

also the difference of cleavage in the two directions. (See Alger, this Journal, vol xlvi, p. 233, and Dana's Mineralogy, p. 324.) The formula is an improbable one, as Delesse admits; the excess of silica may be mechanical: in other respects the composition is that of Heulandite.

9. *Sismondine—a new mineral.*—This species, instituted by Delesse, occurs in chlorite slate, associated with garnets, titanic iron and pyrites, and presents the following characters:

Massive with a very easy cleavage. Scratches glass, but is scratched by steel. *Specific gravity* 3·565. *Lustre* brilliant. *Color* deep green. *Streak-powder* clear grayish green. No action on the magnetic needle, either before or after calcination. *Fracture* uneven. Changes to pinchbeck brown before the blowpipe, but does not fuse. In a tube yields water. With salt of phosphorus it dissolves with difficulty, and with borax affords the reaction of iron.

Composition, according to M. Delesse,

Silica,	24·1	Oxyd of Titanium,	7·6
Alumina,	43·2	Water,	trace
Protoxyd of iron,	23·8		

from which he deduces the formula $\dddot{Si}^3\dot{Fe}^4+5\dddot{Al}\dot{H}$, the last member of which is the formula for diaspore. M. Delesse suggests that sismondine is allied to chloritoid, a mineral occurring in the Ural associated with diaspore.

The analysis above may afford nearly the formula $\dot{Fe}^3\dddot{Si}+\dddot{Al}^3\dddot{Si}+3\dot{H}$, which is perhaps more probable than that given by Delesse, and differs but little from the formula of chloritoid, as obtained from Bonsdorff's analysis. (See Dana's Min. p. 557.)

10. *Description by Captains Cook and Flinders of Birds' Nests of enormous size on the coast of New Holland;* in a letter from Prof. Edward Hitchcock to the Editors, dated Amherst, Mass., Dec. 22, 1843.

In lecturing on the huge footmarks of sandstone in the Connecticut valley, I have been in the habit for many years of reading to my classes, *as the poetry of the subject*, some statements from the twelfth volume of the Athenæum, or Spirit of the English Magazines, (p. 48,) respecting enormously large birds and birds' nests. As some of these statements are manifestly fabulous, it never occurred to me till to-day, to inquire whether any of them were true. I was led to make the inquiry probably by the astonishing discoveries of Prof. Owen respecting the *danger bird* of New Zealand; and the result is, that I have almost persuaded myself, that with the help of Captains Cook and Flinders *I have found the nest of the Dinornis on the coast of New Holland.* These navigators have given the following statements in their published voyages. I quote Cook's account from Kerr's Collection of Voyages and Travels, Vol. XIII, p. 318. It was Cook's first voyage. Lizard Island is near the northeast coast of New Holland, not far from Cape Flattery, and in about 15° S. lat.

"At two in the afternoon," says Cook, "there being no hope of clear weather, we set out from Lizard Island to return to the ship, and in our way landed upon the low sandy island with trees upon it which we had remarked in our going out. Upon this island we saw an incredible number of birds, chiefly sea-fowl; we found also the nest of an eagle with young ones, which we killed; and the nest of some other bird, we knew not what, of a most enormous size. It was built with sticks upon the ground, and was no less than six and twenty feet in circumference, and two feet eight inches high. To this spot we gave the name of Eagle Island, &c."

Capt. Flinders found two similar nests on the south coast of New Holland in King George's Bay. Not having his work at hand, I quote from the Quarterly Review for October, 1814, his description of these nests.

"They were built upon the ground, from which they rose above two feet, and were of vast circumference and great interior capacity; the branches of trees and other matter of which each nest was composed, being enough to fill a cart."

Now I suppose from the character of Captains Cook and Flinders, we may place implicit confidence in the truth of these accounts. Indeed, Cook was accompanied to Eagle Island by Sir Joseph Banks. Equally certain is it that no known bird but the Dinornis would have built so enormous a nest. I am led therefore almost irresistibly to inquire whether the Dinornis may not be an inhabitant of the coast of New Holland, and still alive! Even if extinct upon New Zealand, it may have remained longer in the warmer climate of New Holland. It may be that these nests have been accounted for in some other way; but if so, I have seen no other explanation.

P. S. *Feb.* 1844.—Having occasion to give a lecture this winter before the Young Men's Association in Troy, N. Y., I had a drawing made of the Dinornis of the natural size, on the type of the Apteryx and Cassowary, and also of one of the nests described above, and I assure you that the nest was only of a *respectable size* for a bird sixteen feet high!

11. *Festival in honor of Berzelius.*—Last Saturday (says a correspondent, writing from Stockholm, Nov. 14th, 1843) we had a festival here of no ordinary interest. A quarter of a century having just elapsed since our celebrated countryman Baron Berzelius was appointed Hon. Secretary of the Royal Academy of Science at Stockholm, which most distinguished situation he still continues to occupy, the leading

members of the Academy, being anxious to give a public acknowledgment of the great honor which the name of Berzelius has reflected upon the Academy, and also the immense services, never to be forgotten, which he during this long period has rendered to their interests as a scientific body, resolved that this jubilee should be celebrated within the Academy in an appropriate manner, due to his illustrious name in the world of science and literature, not less than to his high rank in society.

Arrangements were accordingly made for a grand dinner in the house of the Academy, and his Royal Highness the Crown Prince, being first honorary member of the Academy, accepted graciously the invitation to honor the company with his presence on this occasion.

As the name of Berzelius is known over all the world, it may be of some interest to many of his friends in foreign countries, to have a short outline of his life. He was born on the 20th August, 1779, in Ostergöthland in Sweden. His father was a clergyman. In common with Linnæus, and many other stars in the horizon of science, it fell also to the lot of Berzelius to struggle against poverty and many adversities in the earlier part of his life; but his ardent spirit and indomitable desire for knowledge overcame all hindrances. At the age of seventeen he came to the university of Upsala, where he made very rapid progress in his learning, particularly in his favorite study, chemistry. After having passed his examinations, he was promoted Doctor in Medicine, 1804. Having been appointed Medicinæ et Pharmaciæ Adjunctus at the Collegium Medicum at Stockholm, he continued for several years to give public and private instruction in chemistry to young students; and besides, he was obliged, on account of his small income, to practice occasionally as a physician. In 1807 he was appointed Medicinæ et Pharmaciæ Professor, and in the same year he instituted, in company with seven other eminent men, the Swedish Medical Society at Stockholm, which is now highly flourishing, and constitutes the very heart of the medical profession in Sweden. In 1808 he was called a member of the Royal Academy of Science, and officiated as President in 1810. In the same year he was appointed a member of the Royal Sanatory Board, of which he is now the senior member. In 1818 he was appointed secretary of the Royal Academy of Science. He has traveled through several foreign countries for scientific purposes, viz. to England, 1813; to Germany and France, 1819; to Bohemia, 1822; and to Germany, 1830 and 1835. When the Medico-Chirurgical College was established at Stockholm in 1815, Berzelius was appointed Professor of Chemistry; and having lately resigned his place, his Majesty graciously allowed him to remain as Professor Honorarius, and to retain his salary.

The merits of Baron Berzelius, as regards the science of chemistry, are so multifarious, that it is quite impossible to comprehend them within the limits of the present outline. As a proof of the magnitude of his laborious pursuits, it may be sufficient to mention, that he first developed the electro-chemical system, and that he has also examined and minutely described the atomic theory of the elementary bodies. Of these bodies he has discovered selenium, thorium and cerium, and first classified calcium, barium, strontium, columbium, silicium and zirconium among the metals. He has discovered and examined several great classes of chemical combinations, as, for instance, the different degrees in which sulphur combines with fluoric acid, with platinum, columbium, vanadium, tellurium and phosphorus, the sulphates, &c. Not less has he distinguished himself by his experiments in organic chemistry; and properly speaking, he has laid the foundation of the vegetable and animal chemistry, in particular the latter.

His works, which have been for the most part translated into the English, French, German, Italian, Spanish and Polish languages, are so numerous and voluminous, that considering the accuracy with which every thing is described, it appears to be almost a wonder how one man, whose time besides is occupied by a great deal of official duties, has been able to accomplish such a mass of scientific publications. His great work, 'Manual of Chemistry,' has been published in four different editions, of which the latest contains ten volumes, the last of which was published in 1841. The fifth edition is now publishing, and two volumes are already in the hands of the booksellers. His lectures on Animal Chemistry are published in two volumes; his works on Natural Philosophy, Chemistry and Mineralogy, make six volumes; and his Reports of the yearly progress of the natural and chemical sciences contain not less than twenty-three volumes.

Of eminent men from foreign countries who have worked in the laboratory of Berzelius, are Bonsdorff, Engelhardt, Gmelin, Hartwall, Hess, Hünefeld, Johnston, Magnus, E. Mitscherlisch, Nordenskiöld, Osann, G. Rose, H. Rose, Turner, Winckler and Wöhler.

Baron Berzelius has received from his Majesty King Charles John many marks of high distinction, viz. created a nobleman, 1818, and a Baron, 1835; Knight Commander of the royal order of Wasa, 1821, and Grand Cross of the same order, 1829. Besides, he is Knight of the royal Swedish order of the Polar Star, and of several foreign orders received from the Emperor of Russia, and the kings of Prussia, Denmark, Belgium, France and Sardinia. He is an honorary member of not less than eighty-eight literary and scientific societies, of which seventy-nine belong to foreign countries. In consideration of the great services which Baron Berzelius has done to his native country, the members of the last diet at Stockholm in 1840, voted to him the annual sum of two thousand dollars Banco, as a pension for his lifetime, independent of his former emoluments.—*Lond., Edinb. & Dubl. Phil. Mag.*

THE

AMERICAN

JOURNAL OF SCIENCE, &c.

Art. I.—*Researches in Elucidation of the Distribution of Heat over the Globe, and especially of the Climatic Features peculiar to the Region of the United States;* by Samuel Forry, M. D., Author of "The Climate of the United States and its Endemic Influences," Editor of "The New York Journal of Medicine and the Collateral Sciences," etc.

(Concluded from p. 50.)

Another important subject is the influence of temperature on the geography of plants, which has been ably treated by M. de Candolle. In considering its relation with the organic life of plants, it is necessary to keep in view three objects:—1. The mean temperature of the year; 2. The extreme of temperature both in regard to heat and cold; 3. The distribution of temperature among the different months of the year. The last is the most important; but in the investigation of vegetable geography it is requisite to estimate the simultaneous influence of all physical causes,—soil, heat, light and the state of the atmosphere as regards its humidity, serenity, and variable pressure. Each plant has generally a particular climate in which it thrives best, and beyond certain limits it ceases to exist. Hence having seen the great variations of summer and winter temperature on the same isothermal line, the absurdity of limiting a vegetable production to a certain latitude or mean annual temperature, is apparent. To say that the vine, the olive, and the coffee-tree require, in order to be productive, annual temperatures of 53°·60, 60°·80 and

64°·40, is true only of the same system of climate. As the annual quantity of heat which any point of the globe receives, varies very little during a long series of years, the variable product of our harvests depends less on changes in the mean annual temperature, than in its distribution throughout the year. Thus climates in regard to vegetable productions, are strongly characterized by the variations which the temperature of months and seasons experience. But this subject is too extensive for present investigation. It has been already observed that the parallels, which, in western Europe, yield the olive and the orange, are with us productive of ice and snow; but on the Pacific coast of our territory, the requisite temperature is found at Fort Vancouver, which is in the latitude of Montreal. Here vegetation grows luxuriantly in mid-winter. That vegetables common to the warm climates, as the orange, lemon, citron, fig, olive, and pomegranate, can be successfully cultivated here, is no longer a doubtful question; and the cotton-plant also, is said to flourish well. The British Fur Company at Fort Vancouver, besides cultivating all these plants, have likewise a fine grapery, which yields fruit equal to those in France.

The influence of the unequal distribution of heat upon vegetable geography is beautifully illustrated in the four systems of climate demonstrated on the same parallels in the Northern division of the United States; and if we extend the comparison to the Pacific coast, a fifth system may be enumerated on the same latitude. Taking the coast of New England, the region of the great lakes, and the Pacific coast, the difference between the mean temperature of winter and spring varies from 6°·67 to 18°·42; while in the excessive climate of the region west of the lakes, and that intermediate to the lakes and the Atlantic, this difference ranges from 18°·82 to 30°·83; and accordingly we find, as already explained, that spring and summer, in the latter, are confounded with each other, and that the sudden excess of heat renders the progress of vegetation almost perceptible. It is necessary, however, to add that the low ratio of 6°·67 occurs on the Pacific coast, the lowest average in the Northern division of the United States being 11°·67. In the Middle and Southern divisions, this vernal increase of temperature gradually diminishes, until finally at Key West it is only 5°·99. But there is another important feature to be observed. Not only is the vernal increase greater in excessive climes; but as it supervenes upon a lower winter temperature, the effect produced on the development of vegetation is in an inverse ratio. The vernal increase of 30°·83, for example, at Fort Snelling, comes upon a mean winter temperature of 15°·95, while at Fort Sullivan, on the same parallel, the increase of only 17°·16 follows a winter temperature as high as 22°·95. Between northern and southern latitudes, this contrast is still more marked; for, while at Fort Snelling there is a difference of 13°·46 between the months of February and March, and at Key West only 1°·56, the temperature of February at the former is 18°·66 and at the latter 72°·15.

What month expresses the nearest equivalent to the mean annual temperature? This is a question which has excited considerable controversy, and in regard to which there still exists an opposite diversity of opinion. By Kirwan, it is strenuously contended that the month of *April* expresses this equivalent; while Humboldt, on the other hand, shows by extensive tabular statements that *October* is better entitled to this characteristic: and on either side of this question are arranged many other authors of lesser note. As the laws of nature are universal, these phenomena, like all others, must be susceptible of systematic arrangement; and lest it may be thought presumptuous in the author to attempt to decide between such high authorities, he will state in advance that the diverse systems of climate presented in the northern regions of the United States, on the same parallels, afford a means of comparison doubtless heretofore unequalled.

As respects the controverted question, whether April or October expresses a nearer equivalent to the mean annual temperature, the following deduction is clearly authorized by the tabular abstracts appended to the writer's work on "The Climate of the United States, etc."—*In* EXCESSIVE *climates, the mean temperature of April is generally as high as that of the year, while that of October is considerably higher; and in* MODIFIED *climes, it will be found that the former is generally as much lower as the latter is higher.* Now this relation is precisely what might have been anticipated from a consideration of the preceding facts; for as the vernal increase of temperature is always much greater in *excessive* than in *modified* climates, it follows that, if under any circumstances, April expresses a nearer equivalent than October, it must be when its mean temperature is augmented by a sudden vernal increase.

These results, comprising some of the posts in the Northern division of the United States, are exhibited in the following table:

Excessive climates remote from large bodies of water.	No. of years of observations.	MEAN TEMPERATURE OF The year.	April.	October.
Hancock Barracks,	2	41°·21	43°·85	45°·84
Fort Snelling,	8	45 ·83	46 ·00	49 ·27
" Howard,	9	44 ·92	43 ·28	47 ·51
Council Bluffs,	5	51 ·02	51 ·82	53 ·65
Fort Armstrong,	4	51 ·64	51 ·26	54 ·58
Modified climates on the ocean and lakes.				
Salem, Massachusetts,	33	48 ·61	46 ·11	51 ·15
Fort Vancouver,	1	51 ·75	46 ·00	54 ·00
" Brady,	6	41 ·39	38 ·50	45 ·52
" Sullivan,	5	42 ·95	43 ·28	47 ·51
" Preble,	5	46 ·67	45 ·44	49 ·28
" Niagara,	2	51 ·69	47 ·52	58 ·94
" Constitution,	4	47 ·21	45 ·31	50 ·43
" Wolcott,	9	50 ·61	46 ·41	54 ·45
" Trumbull,	2	55 ·00	51 ·00	58 ·10
" Columbus,	9	53 ·00	49 ·89	55 ·82

It is thus seen that in excessive climates the law above stated holds good invariably. There is but one exception in the tables appended to the author's work before alluded to, viz. Fort Crawford, but the results of this post are based on only two years' observations. Fort Howard, it is seen, has in April a somewhat lower mean than that of the year, which, as it differs from the rest in this respect in consequence of having its temperature *partially* modified by the waters of Green Bay, is an exception which confirms the rule. At the posts in the modified climates, the mean temperature of April, with the single exception of Fort Sullivan, is generally as much lower as that of October is higher than the annual mean. This law is beautifully illustrated in the results obtained at Salem, based on thirty three years' observations; the mean of April being 2°·50 lower, and that of October 2°·54 higher, than the annual mean. Fort Vancouver, which is not situated near a large body of water, derives its uniform climate from its position near the western coast of the continent.

A decision of this long mooted question is thus presented, illustrating the ancient axiom, that truth is never found in extremes. Kirwan, however, was somewhat nearer the truth than Humboldt. As regards any credit that may pertain to this explanation—a deduction that the writer made several years ago—he considers it exclusively his own.

Reference may here be made to the "*well established fact of meridians of greatest cold*," so called by Traill in the Encyclopœdia Britannica. "The remarkable fact," he says, "of the influence of *longitude* on temperature leads to the conclusion, that on each side of the equator there are two meridians, under which the mean temperature is lowest. These have been termed by Sir David Brewster the *cold meridians*, and their extremities are the *poles of greatest cold.* The position of these in the northern hemisphere may be approximated from recent investigations; and perhaps we shall not greatly err if we assign the longitude of 95° W. for the American, and of 100° E. for the Asiatic cold meridians. The apparent coincidence of the cold meridians with the general directions of Hansteen's lines of no variation is perhaps more than accidental, when we reflect that there seem to have been, in former ages, migrations of the cold meridians eastward and westward, coincident, as far as we can judge from recorded changes of climate in northern countries, with similar migrations of the magnetic needle."

This theory of cold meridians is at best extremely problematical. If there is any truth in the laws of climate as based on physical geography, then is this theory no more than the "baseless fabric of a vision." It is true that the meridians selected are the coldest in some latitudes of the northern hemisphere, inasmuch as they traverse the interior of the continents, considerably nearer the eastern or colder side. The longitude of 95° W. for instance, passes nearly two degrees west of Fort Snelling, Iowa, which is very probably the coldest meridian in the United States; but as this same meridian passes through the Gulf of Mexico, the result there at once explodes the whole hypothesis. And as the same meridian continued into the southern hemisphere, traverses the Pacific Ocean, the theory is again at fault; for the laws of climate resulting from physical geography place it about 30° farther east on the continent of South America; and in the old world, the coldest meridian, if continued into the southern hemisphere, instead of being that of 100°, will be found either in Africa or New Holland. But the circumstance of the migration of these meridians, makes still greater demands upon our credulity, inasmuch as it runs directly counter to the well established principle that any alteration in the climate of a locality supposes a corresponding alteration in its physical features. That there are *poles of greatest cold* corresponding to the terrestrial magnetic poles, as distinguished from the poles of rotation, is, however, quite probable.

Does the climate of a locality, in a series of years, undergo any permanent changes? The question has been much debated, whether the temperature of the crust of the earth or of the incumbent atmosphere, has undergone any perceptible changes since the earliest records, either from the efforts of man in clearing away forests, draining marshes, and cultivating the ground, or from other causes. So general is the opinion that the temperature of the winter season in northern latitudes has become higher in modern than it was in ancient times, that it has been regarded as an admitted fact. Among the writers of reputation who have adopted and maintained this opinion, are the Abbé Du Bos, Buffon, Hume, Gibbon, Volney; and, in our own country, Jefferson, Williams, and Dr. Holyoke. By them it is alleged that the winters of the south of Europe, in the time of the first Roman emperors, were, according to the concurring testimony of many authors, much more severe than now. In proof of this assertion, they quote many passages from the ancient authors—Juvenal, Virgil, Ovid, etc.; and in regard to Gaul and Germany, the writings of Cæsar, Diodorus Siculus, etc.

Gibbon, in his "Decline and Fall of the Roman Empire," has contributed perhaps more than any other to perpetuate the affirmative of this opinion. The ignorance of the great laws of climate betrayed by him in the observation, that "*Canada, at this day, is an exact picture of ancient Germany*," finds an excuse in the fact that he lived before the epoch of Baron Humboldt; but it is truly surprising that Malte Brun should, many years after, make the same comparison. Gibbon's reputation is that of a *historian;* but it is easy to show, as we have done elsewhere, that he falls short even in this character, as regards the assertion, when speaking of transporting heavy waggons over the frozen rivers of ancient Germany, that "*modern ages have not presented an instance of a like phenomenon.*"

As the full discussion of this question alone would take up the space allotted to this article, the most general view of it must here suffice. For several years past, we have devoted much attention to climatology, ransacking all the libraries to which we have had access for works treating on this subject; and we early became satisfied that the universal opinion that modern winters have experienced a material increase of temperature, has no foundation in reality. As we have no exact instrumental observations of temperature that go back much farther than a century, our information in regard to more remote periods being derived from loose notices scattered through the old chronicles relative to the state of the harvest, the quantity of the vintage, or the endurance of frost and snow in the winter, great allowance must be made for the spirit of exaggeration which tinges all rude historical monuments. It must be borne in mind that the thermometer is a comparatively modern instrument, invented in 1590, but still left so imperfect, that it was not till the year 1724 that Fahrenheit succeeded in improving it sufficiently to warrant a comparison of observations. It is not surprising that one should hear continual complaints of the altered condition of the seasons, especially from elderly persons, in whom the bodily frame has become more susceptible to the impressions of cold; but similar lamentations, like the prevalent notion that men in general were taller in the earlier ages of the world, have been repeated by the poets and the vulgar from time immemorial.

The facts stated by the Roman poets, if not exaggerated, doubtless in many instances stand isolated, not unlike the circumstance recorded in relation to the Baltic, which in 1688 was so firmly frozen that Charles XI of Sweden crossed it with his army, or the similar fact that in the winter of 1779–80, horse and artillery were transported over the ice in the harbor of New York. We have elsewhere clearly established from historical evidence, but which is here precluded from want of space, that the most remarkable extremes of heat and cold have been frequently recurring ever since the time of the Romans referred to above, the opinion of Gibbon to the contrary notwithstanding.

Although in possession of the facts requisite to establish the opinion that the climate of Europe has undergone no material change since the era of Julius Cæsar, yet it was not without considerable hesitancy that we announced this conclusion in the face of all the world. What then was our surprise in finding subsequently a remarkable coincident confirmation of this deduction in the last work of that extraordinary man, Noah Webster, LL. D., entitled "A Collection of Papers on Political, Literary, and Moral Subjects," published in 1843. One of the papers is "*On the supposed Change in the Temperature of Winter*," which, containing forty four octavo pages, consists of two distinct essays, read before the Connecticut Academy of Arts and Sciences—the first in 1799, and the supplementary remarks in 1806.*

In this investigation Dr. Webster commences with the dawn of history. From several passages in the Scriptures of equivocal import, written as early as the days of Moses and David, it has been hastily concluded that the climate of Palestine or Judea has undergone a most remarkable melioration. But the fallacy of this inference he establishes most conclusively, proving that the climate of Palestine has experienced no increase of temperature for more than three thousand years. In every part of the Bible mention is made of olives, figs, and pomegranates; and even in the time of Moses, the spies sent to explore the country came back laden with figs and pomegranates. According to Pliny, "Judea is particularly renowned for palm-trees or dates;" and that these trees were not first introduced into Judea by the Israelites when they migrated from Egypt, is evident from the single fact that they found in the plains of Moab and in the vicinity of Jericho the most luxuriant palm-trees. Even the word Jericho, which is called in Deuteronomy "the city of palm-trees," signifies in the Ethiopic, according to Dr. Webster, a palm-tree. "We have, then," says Dr. W., from these and other facts adduced, "*certain proof* that Palestine, more than three thousand years ago, was a milder climate than Italy, milder than the south of France, as mild as the coast of Africa, at that time, and milder than South Carolina at this day."

It is by the same summary mode of argument that Dr. W. dismisses the seeming facts adduced by the Abbé Du Bos, Gibbon, Hume, Williams, and others, from the writings of Homer, Virgil, Pliny, Juvenal, Livy, Ælian, Horace, and Julius Cæsar. In regard to what writers have recorded of the winters of ancient Gaul and Germany, our own views, given in detail elsewhere, find a singular confirmation in those of Dr. Webster. The remark of Gibbon, already quoted, referring to the freezing of the Rhine

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* These Essays were published in New Haven, A.D. 1810, constituting the first article in the "*Memoirs of the Connecticut Academy of Arts and Sciences*," Vol. I, part 1, pp. 216, 8vo.—Eds.

and Danube, that "*modern ages have not presented an instance of a like phenomenon*," he says, "is contrary to all historical evidence, and even to facts which took place during that author's life."

Dr. W. dwells particularly upon the vegetables which flourished in the Augustan age in Gaul; and these evidences, he regards as our safest guides. He concludes with the following striking remark: "Olives grow and mature there [the south of France] precisely within the limits marked by Strabo and Pliny, and as far as we can judge, not a league further north than they did 1800 years ago." But as respects the influence of temperature on the geography of plants, Dr. W.'s remarks are defective, in consequence of his limiting his comparisons to the *mean annual temperature* of a locality, which is entirely subordinate to the distribution of the temperature *among the different months of the year*. It is true, De Candolle had not written in 1799, but in publishing in 1843, it should not have been forgotten that a new era had arisen in meteorology.

Dr. Webster's final conclusion from this extensive and most learned array of historical facts, is, for the first essay, that, as respects both the old and new world, "the hypothesis of a moderation of climate appears to be unsupported." And, for the second—"But we can hardly infer, from the facts that have been yet collected, that there is, in modern times, an actual diminution of the aggregate amount of cold in winter, on either continent."

Now, as Dr. W.'s views relative to the influence that clearing the country of its forests exercises upon the seasons of the year, also correspond remarkably with our own deductions, we cannot fail to discover a marked coincidence in the similarity of his conclusions and that previously expressed by ourselves: "All observations then, thus far, confirm the belief in the general stability of climates. As regards the seasons, it will be shown, however, that in countries covered with dense forests, the winters are longer and more uniform than in dry, cultivated regions, and that in summer, the mean temperature of the latter is higher. Hence, in regard to the opinion generally entertained, that the climate of Europe has been very much meliorated since the days of Julius Cæsar, it is clearly apparent, from the foregoing facts, that it is far from being sustained by evidence sufficient to enforce conviction. But, at the same time, while it is obvious that no material change has taken place, for the last two thousand years, in the climate of Europe, the conjecture that it has gradually acquired rather a milder character, or at least that its excessive severity seems on the whole to occur less frequently, appears to be warranted."

It is thus seen, and it will be still further shown, that whatever difference there may be between the seasons of ancient and modern times, they find an explanation without resorting to the unphilosophical hypothesis of a general augmentation of temperature. So universal has been the opinion that modern winters have experienced a material increase of temperature, that Dr. Webster remarks—"Indeed I know not whether any person, in this age, has even questioned the fact." Under these circumstances, the coincidence that both of us, independent of each other, arrived at the same conclusion—a conclusion adverse to the *vox populi*, which thus seems to be not always the *vox Dei*—may be regarded as affording a confirmation of the legitimacy of the deduction.

We have thus attempted to answer the question—*Does the climate of a locality, in a series of years, undergo any permanent changes?* And we may add, that although the mean temperatures, as has been ascertained by instrumental observations, vary from one another irregularly, either a few degrees above or below the *absolute* mean temperature of the place; yet it has not been found that the temperature of a locality undergoes changes in any ratio of progression. At the same time, this series of atmospheric changes, however complicated and perplexing, there is good reason to believe, is as determinate in its nature as the revolutions of the celestial bodies. When, however, the science of meteorology shall have become more advanced, we shall doubtless discover that these apparent perturbations of annual temperatures are real oscillations—vast cycles, which will enable us to predict, no doubt with some degree of certainty, the condition of future seasons.

It now remains to advert to two collateral questions: *Is the climate west of the Alleghanies milder by 3° of latitude than that east? Does the climate of our Northwestern frontier resemble that of the Eastern States, on their first settlement?*

The opinion was early entertained that the climate of the region west of the Alleghanies, is much milder than that of the district east. Mr. Jefferson estimated the difference equivalent to 3° of latitude, as similar vegetable productions are found so many degrees farther north.

These phenomena M. Volney ascribed to the influence of the southwest winds, which carry the warm air of the Gulf of Mexico up the valley of the Mississippi. As North America has two mountain chains, extending from northwest to southeast, nearly parallel to the coasts, and forming almost equal angles with the meridian, Humboldt endeavored to explain the migration of vegetables toward the north, by the form and direction of this great valley which opens from the north to the south; while the Atlantic coast presents valleys of a transverse direction, which opposes great obstacles to the passage of plants from one valley to another. The tropical current or trade-wind, it is said, deflected by the Mexican elevations, enters the great basin of the Mississippi and sweeps over the extensive country lying east of the Rocky Mountains; and that when this current continues for some days, such extraordinary heat prevails even through the basin of the St. Lawrence, that the thermometer at Montreal sometimes rises to 98° of Fahr. In winter, on the contrary, when the locality of this great circuit is changed to more southern latitudes, succeeded by the cold winds which sweep across the continent from the Rocky Mountains or descend from high latitudes, this region becomes subject to all the rigors of a Siberian winter.

Upon the fallacy of these views it is deemed unnecessary to dilate. It is proved by thermometrical data that the climate west of the Alleghany is more excessive than that on the Atlantic side—a condition that would seem unfriendly to the migration of plants. Thus Jefferson Barracks, on the Mississippi, exhibits a greater contrast in the seasons than Washington City; and the same is true in regard to Fort Gibson and Fort Monroe, notwithstanding the former is 1° 32′ farther south. That the climate of the peninsula of Michigan encompassed by ocean-lakes, should prove genial to plants that will not flourish in the same latitudes in the interior of New York, is, indeed, consonant with the laws of nature; and that the same plants should flourish 3° farther north in the valley of the Mississippi than on the eastern side of the mountain, finds a sufficient explanation in the following extract from Murray's Encyclopædia of Geography: "Powerful summer heats are capable of causing trees and shrubs to endure the most trying effects of cold in the ensuing winter, as we find in innumerable instances; and *vice versâ.* Hence, in Great Britain, so many vegetables, fruit-trees in particular, for want of a sufficiently powerful sun in summer, are affected by our comparatively moderate frosts in winter; while upon continents in the same degree of latitude, the same trees arrive at the highest degree of perfection."

As the writer has found the opinions of M. Volney, as well as those of Jefferson, Williams, and Rush, quoted as oracular in every work professing to treat of our climate, it may not be amiss to examine this subject a little more in detail. This French philosopher had the singularly bad fortune of adopting the errors of Dr. Rush and Mr. Jefferson. For example, according to the former, as we recede from the ocean into the interior of Pennsylvania, "the heat in summer is less intense,"—a phenomenon contrary to every law of nature, unless reference was had to the Alleghany elevations; and, in accordance with the latter, the climate becomes colder as we proceed westward on the same parallel until the summit of the Alleghany is attained, when this law is reversed until we reach the Mississippi, *where it is even warmer than the same latitude on the sea-board.* This theory, by the way, is based upon the *testimony of travellers;* and "their testimony," says Jefferson, "is strengthened by the vegetables and animals which subsist and multiply there naturally, and do not on our sea-board." "As a traveller," adds Volney, "I can confirm and enlarge upon the assertion of Mr. Jefferson;" and in regard to the temperature of the regions lying east and west of the Alleghanies, he concurs in the opinion, "that there is a general and uniform difference equivalent to 3° of latitude in favor of the basin of the Ohio and the Mississippi." This conclusion, which is not deduced from thermometrical data, rests, it will be observed, upon the phenomena of temperature and of vegetation exhibited in the region of the great lakes. "Even as high up as Niagara," he continues, "it is still so temperate that the cold does not continue with any severity more than two months, though this is the most elevated point of the great platform—a circumstance totally inconsistent with the law of elevations." He proceeds to say that this climate does not correspond with similar parallels in Vermont and New Hampshire, "but rather with the climate of Philadelphia, 3° farther south. * * * At Albany, no month of the year is exempt from frost, and neither peaches nor cherries will ripen." The influence of the great lakes in modifying temperature has been already so abundantly demonstrated, that further illustration is deemed supererogatory. The phenomena observed by Volney are truly facts; but the causes being unknown, the theory in regard to the difference of temperature east and west of the Alleghanies, was naturally suggested. Instead of deducing general laws from universal facts, this theory of Volney and Jefferson was, as will be seen, a premature deduction—the result of hasty and partial generalization.

M. Volney also presents an extended investigation of the system of winds in the United States; and the ignorance of this celebrated traveller in thus attempting to explain the meteorological phenomena peculiar to the region of the great lakes, shows how little was known forty-five years ago of the laws of meteorology. In reference to the Trans-Alleghany region, he thus remarks:—"I think I have clearly demonstrated that the southwest wind of the United States is nothing but the trade-wind of the tropics turned out of its direction and modified, and that consequently the air of the Western country is the same as that of the Gulf of Mexico, and previously of the West Indies, conveyed to Kentucky. From this datum flows a natural and simple solution of the problem, which at first must have appeared perplexing, *why the temperature of the Western country is hotter by 3° of latitude than that of the Atlantic coast*, though only separated from it by the Alleghany mountains. The reasons of this are so palpable that it would only be wearying the reader to give them. Another consequence of this datum is, that the southwest winds being the cause of a higher temperature, it will extend the sphere of this temperature so much the farther, the greater the facility with which it can prevade the country; and this affords a very favorable presage for the parts that lie in its way, and are exposed to its influence, namely those in the vicinity of Lakes Erie and Ontario, and even all the basin of the river St. Lawrence, into which the southwest wind penetrates."

Now these are the opinions still maintained at the present day, to account for the supposed fact of the higher temperature of our tramontane region. It is a good rule in philosophy to ascertain the truth of a fact before attempting its explanation,—a truism, the observance of which would have saved M. Volney the labor of constructing his complex theory of the winds. All thermometrical results confirm the law, that in proportion as we recede from the ocean or inland seas, the climate grows more excessive;

and that the meteorological phenomena of the region of the great lakes do not arise from the agency of tropical winds, is apparent from the single fact, that the winters are several degrees warmer, and the summers at least ten degrees cooler, as regards the mean temperature of these seasons, than positions one hundred miles distant, notwithstanding on the same parallel or even directly south, and consequently equally exposed to the current from the Gulf of Mexico. Volney's theory, in truth, bears a contradiction upon its own face; for, while he ascribes the modified climate of the lakes to the agency of tropical winds, he admits that the intermediate country traversed by these winds has a much more rigorous climate.

The influence of predominant winds is manifest, however, throughout the United States; for, one prevailing wind, the southwest, blows from a warm sea,—another, the northeast, from a frigid ocean,—and a third, the northwest, from frozen deserts.

The modification in the climate of the valley of the Mississippi, whatever may be its degree, arises from the combined agency of the Gulf of Mexico and the great lakes: for if land were substituted for the area of the latter, (93,000 square miles,) that region would become, so far as the social state of man is concerned, scarcely habitable.

The opinion that the climate west of the Alleghany range is milder by 3° of latitude than the east,—an opinion quoted generally by writers as an established fact,—arose from the circumstance that the United States present on the same parallel different systems of climate—causes upon which the geographical distribution of plants mainly depends.

In reference to the organic life of plants, it is well known that to some entirely different constitutions of the atmosphere are adapted. In respect to the culture of vegetables, it is necessary to keep in view three objects,—the mean temperature of the summer, that of the warmest month, and that of the coldest month; for some plants indifferent to high summer temperature, cannot endure the rigors of winter; others, slightly sensible to low temperature, require very warm but not long summers; while to others, a continuous rather than a very warm summer seems best adapted. The development of vegetation in the same mean temperature, is also retarded or accelerated, according as it is struck by the direct rays of the sun, or receives the diffuse light of a foggy atmosphere. On these causes depend in a great degree, those contrasts of vegetable life observed in islands, in the interior of continents, in plains, and on the summits of mountains. As the region of the great lakes does not exhibit a greater contrast in the opposite seasons than that of Philadelphia, it follows that plants which, from not being adapted to extremes of temperature, cannot endure the severe winter of Albany, will flourish in the more equalized climate of the same latitude on the ocean or the great lakes.

Thus, as Volney and Jefferson saw that the vegetation of Philadelphia is found in the modified climate of our northern lakes, while similar plants will not flourish on the same parallels in the the interior of New York, Vermont, and New Hampshire, the theory in regard to the difference of temperature, east and west of the Alleghanies, was naturally suggested. If, however, these philosophers had chanced to observe the vegetation, by way of comparison, along the coast of Rhode Island or Connecticut, and on the same parallel in Illinois or farther westward, instead of comparing the region of the lakes and Albany, the world would, *of course*, have been edified with the opposite theory, viz. that the climate east of the Alleghanies is milder by 3° of latitude than that west. While at Fort Trumbull, Connecticut, the mean winter temperature is 39°·33, at Council Bluffs it is as low as 24°·47. Hence plants sensible to a low temperature, which flourish in the climate of the former, will perish in the latter; for while the mean temperature of the coldest month at Fort Trumbull is only 34°·50, at Council Bluffs it is 22°·61. This is also demonstrated by the average annual minimum temperature, that of the former being 9°, and that of the latter −16°; and equally so by the minimum temperature of the winter months, that of December, January, and February being at Fort Trumbull respectively 20°, 10°, and 16°, and at Council Bluffs −4°, −13°, and −11°. On the other hand, it will be found that the vegetables which can endure the rigorous climate of Council Bluffs, will flourish more vigorously than in the region of Connecticut; for at the former, the vernal increase is 27°·47, and at the latter only 11°·67. Moreover, the latter increase is added to a winter temperature of 39°·33; while the former, added to 24°·47, more than doubles itself, the influence of which upon the sudden development of vegetation has been already pointed out. These relations, as developed in the tabular abstracts appended to the author's work on "The Climate of the United States, and its Endemic Influences," might be traced out much farther. At Council Bluffs, the extreme of temperature in summer is also much greater than at Fort Trumbull, the mean maximum of the former being 104°, and of the latter 87°, and consequently the average annual range stands respectively as 120° to 78°. In addition to these facts, it may be observed that so far as elevation is concerned, that of the lakes being 600 feet and that of Albany only 130 feet above the sea, the advantage of the comparison is, at first view, on the side of the latter; but this gradual elevation, it has been shown, exerts no perceptible influence.

Does the climate of our northwestern frontier resemble that of the Eastern States on their first settlement?

This question, it is true, has already been decided in the negative; but as changes of climate in the New World also are alleged by Jefferson, Volney, Rush, and Williams, to have supervened, it may be well to make some inquiry into its truth. It is deemed unnecessary to quote here the loose, vague, and general statements of these writers, who thus assert that, on comparing the results of recent observations on our frontier with the best authenticated accounts we have of the climate of the Eastern states in their early settlement, a close similitude is found. The winters, it is said, have grown less cold and the summers less warm—consequences which are ascribed to the clearing of the forest and the cultivation of the soil. That the climate of the great lakes resembles that of the sea-coast is very apparent; but that the region intermediate or the one beyond, ever maintained such a relation, is an assumption contrary to the laws of nature.

Dense forests and all growing vegetables, doubtless tend considerably to diminish the temperature of summer, by affording evaporation from the surface of their leaves, and preventing the calorific rays from reaching the ground. It is a fact equally well known, that snow lies longer in forests than on plains, because, in the former locality, it is less exposed to the action of the sun; and hence, the winters, in former years, may have been longer and more uniform. As the clearing away of the forest causes the waters to evaporate and the soil to become dry, some increase in the mean summer temperature, diametrically contrary to the opinion of Jefferson, Lyell, and others, necessarily follows. It is remarked by Umfreville that, at Hudson's Bay, the ground in open places thaws to the depth of four feet, and in the woods to the depth only of two. Moreover, it has been determined by thermometrical experiments, that the temperature of the forest, at the distance of twelve inches below the surface of the earth, is, compared with the adjacent open field, at least 10° lower, during the summer months; while no difference is observable during the season of winter.

"The mere effect of cultivation," as Dr. Traill very correctly observes, "can never be very considerable in changing a climate;" but, although cultivation of the soil may not be productive of a sensible change in the mean annual temperature, yet such a modification in the distribution of heat among the seasons may be produced, as will greatly influence vegetation.

Although upon all subjects connected with natural phenomena, there is no higher authority than Charles Lyell, Esq., yet his unqualified decision of this question, as exhibited in the following quotation from his "Principles of Geology," is unsustained by any well observed facts:—"In the United States of North America, it is *unquestionable* that the rapid clearing of the country has rendered the winters less severe, and the summers *less* hot; in other words, the extreme temperatures of January and July have been observed from year to year, to approach nearer to each other. Whether in this case, or in France, the mean temperature has been raised, seems by no means as yet decided: but there is no doubt that the climate has become, as Buffon would have said, 'less excessive.'"

Contrary to Mr. Lyell's opinion, one effect of clearing the country is doubtless to distribute the temperature of the year *more unequally*, thus rendering the seasons more variable; and hence causing, by the exposure of fruits to spring frosts, a serious inconvenience, which has been experienced both in this country and in Europe. The reason of late and variable springs, under these circumstances, may be explained by reference to the fact that, while the earth, clothed with forest and covered with snow, is never frozen, and hence sprouts forth its vegetation as soon as the snow is dissolved in spring; the earth in an open country, on the other hand, requires, after the snow is melted, to be thawed, thus rendering latent for several weeks a great quantity of caloric. This affords an explanation of the changes of climate referred to by Jefferson, Rush, and Williams. If, indeed, the mean annual temperature of Vermont has risen, according to Dr. Williams, 10° or 12° within a century and a half, it must surely have had an intolerable climate, when our ancestors landed on the rock of Plymouth; and upon the same principle of a general increase of heat in our climate, the cultivation of the olive and the fig, since the first settlement of our country, ought to have advanced as far northward as Pennsylvania, if not to Vermont itself!

Dr. Webster devotes some ten pages, in his first essay, to the question of the cold of American winters; and he arrives, from a most extensive investigation of historical facts, at the conclusion, "that the winters have been from the first settlement of America variable, now mild, now severe, just as they are in the present age." A leading object with him is to show the errors of Dr. Williams, who having maintained that the mean temperature of Italy has increased 17°, wished to establish some analogous change in our own climate since its occupation by Europeans; and Dr. Webster proves most conclusively that "if Dr. Williams is unfortunate in his facts, he is still more so in his reasonings and deductions."

As respects a supposed change of climate in the United States, the quotation of a single passage from Dr. W.'s essay, must here suffice. According to John Megapolensis, a Dutch clergyman, who resided at Albany just two centuries ago, "the summers are pretty hot, and the winters very cold. The summer continues till All Saints' Day, (Nov. 1,) but then the winter sets in, in the same manner as it commonly does in December, and freezes so much in one night that the ice will bear a man. The freezing commonly continues *three months*—sometimes there comes a warm and pleasant day, yet the thaw does not continue; but it freezes again till March, and then commonly the river begins to open, seldom in February." Modern winters, according to this account, have not moderated. A common winter is still of *three months'* duration, the Hudson, at Albany, usually freezing early in December, and continuing closed till March.

From this extensive and learned array of historical facts, Dr. Webster's final conclusion, in his second essay, as regards the influence of clearing the country of its forests upon the seasons of the year, has a striking correspondence with our own deductions.

"From a careful comparison of these facts," he says, "it appears that the weather, in modern winters, is more inconstant, than when the earth was covered with wood, at the first settlement of

Europeans in the country; that the warm weather of autumn extends further into the winter months, and the cold weather of winter and spring encroaches upon the summer; that the wind being more variable, snow is less permanent, and perhaps the same remark may be applicable to the ice of the rivers. These effects seem to result necessarily from the greater quantity of heat accumulated in the earth in summer, since the ground has been cleared of wood, and exposed to the rays of the sun; and to the greater depth of frost in the earth in winter, by the exposure of its uncovered surface to the cold atmosphere."

It is thus apparent that the opinion that the climate of the States bordering the Atlantic on their first settlement, resembled that now exhibited by Fort Snelling and Council Bluffs, is wholly gratuitous and unsustained by facts. No accurate thermometrical observations yet made in any part of the world, as already remarked, warrant the conclusion that the temperature of a locality undergoes changes in any ratio of progression; but conversely, as all facts tend to establish the position that climates are stable, we are led to believe that the changes or perturbations of temperature to which a locality is subject, are produced by some regular oscillations, the periods of which are to us unknown. That climates are susceptible of melioration by the extensive changes produced on the surface of the earth by the labors of man, has been pointed out already; but these effects are extremely subordinate, compared with the modification induced by the striking features of physical geography—the ocean, lakes, mountains, the opposite coasts of continents, and their prolongation and enlargement toward the poles.

But even Malte-Brun has ventured the assertion, that "France, Germany, and England, not more than twenty centuries ago, resembled Canada and Chinese Tartary—countries situated, as well as our Europe, at a mean distance between the equator and the pole." This illustration is certainly very unhappy; for, rejecting the pretended antiquity of the Chinese—the fables in relation to Fohi and Hoang-Ti, the former of whom, we are told, founded the empire of China about five thousand years ago, we must, with Malte-Brun, date its origin at least eight or nine centuries before Christ. China should, therefore, possess a milder climate than Europe, inasmuch as agriculture is represented to have been always in the most flourishing condition. As the practice of fallowing is unknown, almost the whole arable land is constantly tilled, and even the steepest mountains, cut into terraces, are brought under cultivation. Now, as this country still presents a climate as austere as that of Canada in the same latitudes, the conclusion is irresistible, that in proportion as the leading physical characters of a region are immutable in their nature, does error pervade the remark of Malte-Brun—"That vanquished nature yields its empire to man, who thus creates a country for himself."

A partial view of this question, indeed, not unfrequently leads to the most unwarranted conclusions. Any changes in the climate of the United States as yet perceived, are very far from justifying the sanguine calculations indulged in, a few years ago, by a writer whose observations upon many other points are very valuable.

"But there will doubtless be," he says, "an amelioration in this particular, when Canada and the United States shall become thickly peopled and generally cultivated. In this latitude, then, like the same parallels in Europe at present, *snow and ice will become rare phenomena, and the orange, the olive, and other vegetables of the same class, now strangers to the soil, will become objects of the labor and solicitude of the agriculturist.*"

The fallacy of the opinion which ascribes the mild climate of Europe to the influence of agricultural improvement, becomes at once apparent, when it is considered that the region of Oregon, lying west of the Rocky Mountains, which continues in a state of primitive nature, has a climate even milder than that of highly cultivated Europe in similar latitudes; and again, China, situated like the United States on the eastern coast of a continent, though subjected to cultivation for several thousand years, possesses a climate as rigorous, and some assert even more so, than that of the United States proper on similar parallels.

It is thus sufficiently obvious that the most diverse climatic phenomena on the same parallels find an explanation in the local influences of physical geography; and that, contrary to the opinion of Lyell, even the apparent anomaly presented by the mild climate of Europe, and by the climatic rigor of eastern North America, but confirms the harmony of these laws throughout the globe. But to explain this supposed exception to the general law, it has even been found necessary, as appears by a recent treatise on comets by M. Arago, to have recourse to the action of one of these bodies.

"As soon as the northern regions of America," he says, "were discovered, it was remarked by the navigators that, at the same latitude, they were much colder than those of Europe. This fact, which could not be satisfactorily explained by the *astronomic theory* of climates, engaged the attention of many naturalists, and among others, of Halley. According to that celebrated philosopher, a comet had formerly struck the earth obliquely, and changed the position of its axis of rotation. In consequence of that event, the north pole, which had originally been very near to Hudson's Bay, was changed to a more easterly position; but the countries which it abandoned had been so long a time, and so deeply frozen, that evident vestiges still remain of its ancient polar rigor. A long series of years would be required for the solar action to impart to the northern parts of the new continent, the climate of their present geographical position."

Fortunately our knowledge of meteorology is now sufficiently advanced to enable us to laugh at this crude explanation of a change in the position of the terrestrial axis, resulting from the concussion of a comet.

Art. II.—*On the Condition of Equilibrium between Living and Dead Forces;* by Robert Henry Fauntleroy, Civil Engineer, New Harmony, Ind.

It is of essential importance to practical mechanics to establish mathematically the relations between a *moving* or *living force* and a pressure or weight at rest, sometimes called a *dead force.* As for example, it is frequently important to know the greatest weight which may be placed on the head of a pile without causing it to sink deeper, the circumstances attending the driving being known. It is chiefly to facilitate the solution of problems of this nature that the following remarks are submitted.

Among the mathematical works consulted on the subject, there are several that allude to it under the title of *vis viva,* stating its force to be proportional to matter multiplied by the square of its velocity; but there was found no expression for the exact measure of effect.

A few quotations showing how the subject is sometimes treated, may not be out of place here.

"Mathematically speaking, there is no comparison between an active force and a dead one; however, repeated experience in driving piles shows that a certain pressure may put a certain percussion in equilibrio."*

"One of the circumstances which creates the greatest difficulty in the theory of the wedge, is the very heterogeneous nature of the resistance, and the force or power by which it is overcome. The resistance is generally that modification of force called pressure. The power which is opposed to the resistance is commonly that species of action called *percussion.* These are modifications of force so totally different as not even to admit of comparison. It has been generally thought that there is no *blow* or *impact*, however slight, which will not overcome a pressure or resistance however great. From which it would seem to follow, that an infinitely small impact is equivalent to an infinitely great pressure. Be this as it may, however, the great difference between these modifications of force is sufficiently evident to demonstrate the total impossibility of establishing the condition of equilibrium of a machine in which the weight or resistance is the force of the one, and the power is a force of the other species."†

With the view of presenting this subject in as familiar a manner as possible, let us consider the case of the pile, regarding it for the present without weight or vis inertiæ, and as resisting percussion by friction alone, considered as a constant retarding force. We may, without error, suppose this friction to exert a retarding force in all respects similar to an imaginary force equal to the friction, and acting as gravity, only in an opposite direction and with greater energy. Let this imaginary force be called W, and the distance through which it acts upon the ram be called x. Now it is evident that if the force W be permitted to react upon the ram through the same distance x, through which it was acted upon by the ram in its descent, it will, like gravity, generate the same velocity in the ram which it had at the instant of percussion. It is also evident, that if this force be opposed by a dead weight equal to its effort to move the ram, it would be held in equilibrio by such dead weight, and no motion could ensue after such a state of rest had been established.

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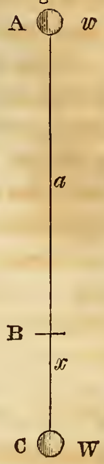
* Sganzin's Civil Engineering, p. 123,—a work of high reputation, and used in the Royal Polytechnic School in Paris, and the United States Military Academy at West Point.

† Natural Philosophy, No. 1, Library of Useful Knowledge, p. 43, Baldwin & Cradock, 1829.

Fig. 1.



The question now is narrowed down to the consideration of two forces, producing the same velocity in the same amount of matter, w, the one force gravity $= w$,* acting through the height $AB = a$, the other force, W, acting through the distance $BC = x$.

Let us assume $W = aw$. Taking seconds and feet as units of time and space, and putting t for time, s for space, and g for the velocity in feet produced in one second by the force of gravity, the relations are—

For the force of gravity $w \propto g = 32$,

t.	s.	v.
1″	$\frac{1}{2}g$	g
2″	$2g$	$2g$
&c.	&c.	&c.

For the force $W = aw \propto ag = 32a$,

t.	s.	v.
1″	$\frac{1}{2}ag$	ag
2″	$2ag$	$2ag$
&c.	&c.	&c.

Now the velocity generated by gravity on the body w, falling the height a, will be $(2ga)^{\frac{1}{2}}$; and the velocity produced on the same body w, by the force $W = aw$, (equal the friction,) acting through the space x, will be $(2agx)^{\frac{1}{2}}$. These velocities being equal, we have—

$$(2agx)^{\frac{1}{2}} = (2ga)^{\frac{1}{2}}$$

Substituting for ag and g their proportionals W and w, we have—

$$(2Wx)^{\frac{1}{2}} = (2wa)^{\frac{1}{2}}$$

Hence, $$W = \frac{aw}{x} \quad . \quad . \quad . \quad . \quad . \quad (A)$$

That is, W is the pressure or dead weight which will hold in equilibrio the percussion of the ram w, falling the height a, and driving a pile of unappreciable weight the depth x. The height a must in this case be taken after the blow is made; because, during the percussion the ram is not only acting by virtue of its

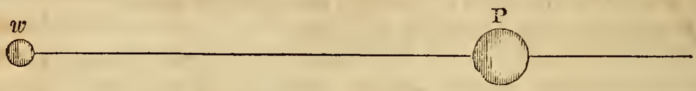
* It will be here observed that the gravitating force acting upon the weight w, is equal to the weight of the body itself, taken if we please in pounds. Hence the propriety of representing that force by w, equal to the weight of the body.

moving force, but is also exerting a force equal to its own weight through the distance x.

Since in practice there are many cases when the pile or other matter driven is heavier than the ram or driving weight, it becomes necessary to introduce other relations.

By the laws of momentum it is known that when an elastic body w, moving with an uniform velocity v, strikes another heavier body P at rest, the body w will return with a diminished

Fig. 2.



velocity $=\frac{(P-wv)}{P+w}$; and that a velocity will be imparted to P equal to $\frac{2vw}{P+w}$. Now, taking w and P as the weight of the ram and pile respectively, the problem resolves itself into the question, What dead weight will hold in equilibrio a pile without vis inertiæ, driven to the depth x by a weight P (the pile itself) moving with the velocity $\frac{2vw}{P+w}$? Because, whatever velocity v the ram may have at the moment of impingement upon the pile, it will impart to the pile a velocity $=\frac{2vw}{P+w}$, at which instant it will cease to act upon the pile, and the pile itself by its own moving force will go down opposed only by friction, the pile being thus, as it were, converted into the driving weight.

The height H, then, through which the pile should fall to acquire the velocity $\frac{2vw}{P+w}$, when v is considered in feet per second, (using 16 feet fall for the first second of time,) will be $H=\frac{1v^2w^2}{16(P+w)^2}$, and the dead weight which it will bear, by equation (A), will be

$$W=\frac{HP}{x}=\frac{Pv^2w^2}{16x(P+w)^2}$$

Now if a be the height through which the ram w descends, we have $v^2=a64$, and by substitution, we have

$$W=\frac{64Paw^2}{16(P+w)^2x}=\frac{4Paw^2}{(P+w)^2x} \quad . \quad . \quad . \quad . \quad (B)$$

Fig. 3.

By similar process we prove that when a pile heavier than the ram is driven to the depth x by the ram w, falling through the height $AC=a$, if we subtract from AC the height $DC=h$, to which the ram would rebound by perfect elasticity, we shall have for the dead weight which will hold the pile in equilibrio,

$$W=\frac{(a-h)w}{x} \quad . \; . \; . \; . \; . \; . \; . \quad (C)$$

When the ram and pile are of the same weight, the expression B becomes

$$W=\frac{aw}{x} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (D)$$

In this and the two preceding cases, the height a is to be measured before the stroke is made, because the draw does not act upon the pile by its own weight through the distance x. Therefore, all the difference which W undergoes between the results of equations (A) and (D) can never exceed the weight of the ram w, regarding the ram and pile as elastic bodies.

From the result at (C) we infer the following approximate method of deriving by experiment the force lost in consequence of such disturbing elements as the momentary yielding of the substance penetrated by its elasticity, the alteration in form, whether permanent or not, of the body driving and body driven, produced during their impingement, &c.; viz.

Let the ram, for instance, be let fall from several small heights, until that height of fall is found at which the pile will just begin to penetrate farther. Call this small height k, the whole working height being a, we shall then obtain a useful effect which will be to that obtained by the appropriate formula as $(a-k)$ is to a nearly.

If a bar of unappreciable weight, perfectly elastic till broken, is subjected to pressure or is broken by a dead weight W, producing a deflection x, and the same effect is produced by a weight w, falling the height a, measured before the stroke, we have

$$W=\frac{2aw}{x}+w \quad . \; . \; . \; . \; . \; . \; . \; . \quad (E)$$

For the wedge of unappreciable weight, considered without friction on its sides, putting l for its length, and b for its thickness at the heel, and considering the resistance as constant and acting at right angles to its direction, measuring a before the stroke, we have

$$W=\frac{(a+x)wl}{bx}.$$

If the friction on the sides of the wedge be just sufficient to prevent its recoil, measuring a after the stroke, we have

$$W=\frac{awl}{2bx} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (F)$$

The results of equations A, B, C and D, the writer has roughly tested by experiments made with small battens of yellow poplar weighing one pound and under, clamped between boards and driven by small weights of the same material varying from a quarter to two pounds, falling heights from one to four feet, and driving the battens from $\frac{1}{6}$ to $1\frac{1}{4}$ inches at a blow, the motions being in the direction of the fibres. All these experiments gave results less than the equations, but some of the most successful came within six per cent. of the calculated results. Equation (E) also was roughly tested by experiment, and came still nearer the calculated result. Equation F has not been tested.

The reader will observe that several elements connected with these propositions have not been considered in detail, the present article being intended merely as an introduction to the subject. Among the most important of these elements are those arising from the elastic properties of all the materials concerned. For instance, it is quite evident that a metallic wedge driven by a metallic hammer will penetrate when a wooden wedge, driven by the same blow, would not, although the actual resistance to penetration remains the same, and neither wedge undergoes permanent alteration in form, and each is considered as elastic. This will result from the difference in intensity of the impingements, which intensity will be inversely proportioned to the amount of compression which takes place in the wedge and hammer during the impingement. Equation (E) will assist in this part of the investigation.

If the foregoing remarks should stimulate inquiry on the part of others better qualified than the writer, to a degree at all proportioned to the importance of the subject, his highest aim will be gratified.

Art. III.—*Address delivered at the Meeting of the Association of American Geologists and Naturalists, held in Washington, May,* 1844; by Henry D. Rogers, Professor of Geology in the University of Pennsylvania, F. G. S., &c.

(Concluded from p. 160.)

MESOZOIC PERIOD.

Let us now take a brief survey of the state of our knowledge of the mesozoic formations of this country, or those produced during what may be termed the middle ages of geological history. The vast interval between the remote epoch of our coal and the dawn of the existing marine species in the eocene tertiary, is much more imperfectly represented in North America than either the earlier or later periods. With the exception of its last or cretaceous age, this great interval, which, in the eastern continent, is so rich in beautiful monuments of extinguished life, so abundant in striking records of the physical revolutions of our globe, seems to have left on this side of the Atlantic only a few fragmentary memorials of its races or its events. Like some of the obscurer periods in human mediœval history, these, the dark ages of American geological time, have been explored of late with a zeal and skill awakened by the very difficulties of the research, and which have already produced some very instructive results.

Referring the isolated formations of this country, whose dates are intermediate between the coal and the tertiary, to the more full and continuous scale of the European strata, as a present standard of comparison, we are now acquainted with deposits belonging to three distinct mesozoic epochs, the equivalents severally of the upper new red sandstone or Triassic rocks, of the lower oolitic deposits, and of the cretaceous strata. Thus we are destitute, so far as this continent has yet been explored, in products of the newer palæozoic period, or, in other words, in any representatives of the Zechstein or magnesian limestone group of Europe, and likewise in equivalents of several of the middle and upper oolitic formations of the old world.

A concise exhibition of some of the facts bearing upon the determination of the age and origin of the three known mesozoic formations of the United States may not be unsuitable on this occasion.

The older of these groups of strata, which I shall call the *Mesozoic red sandstone*, occupies two long and narrow and probably shallow troughs, extending along the eastern side of the great Blue Ridge chain and its northeastern prolongation in New Jersey, New York and New

England. The larger of the two troughs ranges continuously from beneath the trap of the Palisadoes of the Hudson southwestward in a diminishing belt to the interior of Virginia, and beyond this point the deposit occurs in narrow detached tracts as far as the southern side of North Carolina. Throughout all this course, the strata dip towards the north and northwest at angles from fifteen to twenty five degrees. The smaller trough fills the valley of the Connecticut river through the whole breadth of Connecticut and Massachusetts, and in this belt the beds dip at an angle of ten or fifteen degrees to the east and southeast and northeast. The materials of both basins are red shale and argillaceous sandstone, with some detached beds of conglomerate.

Of the organic remains, through an investigation of which alone we can hope to establish the position of these strata in the scale of time, or reach definite conclusions respecting the physical conditions under which they were produced, the most instructive are the remarkable bird-tracks brought to light by Professor Hitchcock in Connecticut and Massachusetts, the fishes of the genera Palæoniscus and Catopterus, discovered by the Messrs. Redfield in New Jersey and elsewhere, and one or two small but most expressive testacea detected by my brother in the formation in Virginia. Through the discovery by Mr. Redfield of some of the same fishes and even bird-tracks in the two distinct belts described, these deposits are identified in age and origin; but what exact epoch should be assigned to the formation, was not until lately susceptible of that rigorous demonstration which the present advanced state of our science calls for.

Guided by mere lithological resemblance, Maclure imagined this stratum to be the equivalent of the old red sandstone of England; but Prof. Hitchcock and Mr. Redfield, chiefly by their investigations into the character of the fishes, have shown conclusively that the date of the deposit is somewhere in the great period of the European new red sandstone, an opinion several years ago expressed by Prof. Hitchcock. But while the low degree of heterocercal structure in the tails of the fishes indicated that these strata could not be newer than the new red sandstone, and the same feature and the bird-tracks both proved them newer than the coal, it was still not settled which of the epochs between the oolitic and the carboniferous ones should claim them. The more favorite supposition was, if I mistake not, that the deposit belonged to the age of the older new red sandstone, and highly instructive as both the fishes and the bird-tracks are, as respects the general date, and above all as regards the nature of the waters, the climate and the surface, still there being among them no one species identifiable with any European relic, some further evidence was required in order to establish the exact epoch. In this stage of research, my brother was fortunate enough, about two years ago, to discover in the formation in Virginia, two or three small shells, and to recognize, in the most abundant of these species, the *Posidonia Keuperi*, a well known and characteristic species of the Trias, or upper new red sandstone of Europe. With this positive link, supported by the strong analogies already mentioned, the most skeptical can no longer hesitate to refer our red sandstone formation to some part of the Triassic or earliest mesozoic period. The conjecture recently expressed by Mr. Murchison, therefore, that the Connecticut deposit is of the age of the magnesian limestone group, or the Permian system of this able geologist, resting as it does chiefly on the occurrence of the genus Palæoniscus, but not on an identity of species, cannot be considered as consistent with the other more conclusive evidence which we now possess of a difference of date; and should it hereafter appear, as seems to be suspected by Mr. Redfield, that the American fishes referred to Palæoniscus, constitute truly a new though allied genus, even the present supposed generic relationships of the formations will vanish. The fact mentioned by Mr. Redfield, that "the scales and apparently the vertebræ in the American species are prolonged to a more limited extent into the upper lobe of the tail than in the European species," while it supports his surmise that they may not be true Palæonisci, must be regarded as in itself an indication of a somewhat more modern period. It will be seen, therefore, that the affinities of this formation forbid our yet assuming that any birds coexisted with the last races of primeval or palæozoic life. But the existence of creatures thus high in the scale of animal organization, in times so remote as the earliest mesozoic epoch, is a fact full of interest, and it is gratifying to the American geologist to perceive that the views so early and candidly submitted in the face of skepticism by Prof. Hitchcock, of the true origin of the bird-tracks, are now universally admitted. That to him alone is due the merit of being the original scientific discoverer of the nature of these impressions, all who are familiar with the history of his labors must acknowledge. Others before him had found specimens of the foot-marks, and had shrewdly suggested that they may have been produced by birds, that opinion having been entertained by a few persons even early in this century, and Dr. James Deane, in drawing the attention of Prof. Hitchcock to some specimens, distinctly intimated the same belief as to their origin. But conjecture in science is not discovery; and if the principle be a sound one, which awards the high title of "minister and interpreter" of nature to him only who, by extended, laborious and systematic observation and inductive reasoning, turns conjecture into demonstration, and from a few isolated phenomena casually presented to him, developes to the view of all after generations an eternal truth, then assuredly must Prof. Hitchcock be regarded as the only scientific discoverer of the origin of these curious and instructive foot-prints. The conscientious wish to acknowledge the claim of Dr. Deane as a suggester of the ornithichnite character of these impressions, has led some of those who have lately reviewed this subject, to do, I think, an unintentional injustice to the well-earned and far more ample claims of Prof. Hitchcock.

In speculating upon the circumstances connected with the deposition of the red sandstone, Prof. Hitchcock supposes the Connecticut formation to be the sediment of an extensive tidal estuary, upon the low and muddy beach of which the various birds whose footsteps he has described, were in the habit of walking and wading at low tide in pursuit of their prey—their foot-prints being covered with a thin layer of silt, probably at each reflux of the waters. Some of these birds, rivaling in size the recently extinct gigantic Dinornis of New Zealand, whose height is computed by Prof. Owen to have been more than ten feet, and frequenting in numerous flocks those lonely shores of the great Connecticut bay, must have imparted a strange aspect to the landscape.

Prof. Mather conceives that in this early mesozoic age, the Blue Ridge and Green Mountain chains having been previously elevated, and much of the country to the eastward being still submerged, "the current which we call the Gulf Stream must have flowed along the eastern coast of this part of the continent where the red sandstone formation now extends;" and to this, and to a supposed polar current from the northeast, he ascribes its deposition, attributing the present dip of the beds to the overlapping of the one current upon the other. To sustain this hypothesis, it would be necessary to show that it is not at variance with the inferences to be drawn from the bird-tracks and other data, which I cannot but consider to indicate, not an open sea, but the existence of two confined tidal estuaries.

The strata which constitute the next link in our broken succession of mesozoic formations, are the coal rocks of Eastern Virginia. The portion of these productive in coal, occupies a basin in the vicinity of Richmond, about thirty miles long, and eight miles wide in its centre, extending from the Appomattox across the James river to near Chickahominy. Another rather higher and unproductive mass is spread irregularly in a narrow belt east of the former, from the Potomac to the James river.

Both of these deposits repose immediately on gneissoid and granitic rocks, and consist for the most part of coarse grits composed of the materials of those underlying crystalline aggregates. North of the James river, the coal lies in two, and in some places in three distinct seams, and is confined to the lowest hundred and fifty feet of the series; but south of that river, it forms one huge stratum, which though very variable, is frequently from twenty to forty feet thick,* and what is remarkable, the principal bed is separated by only a few feet, and sometimes by not more than a few inches, of carbonaceous shale from the granitic floor. By Maclure this formation was referred to the period of the old red sandstone, and more recently Mr. Richard C. Taylor assigned it to the so called "transition carboniferous deposits;" but my brother having during the last three years investigated its fossil vegetation, finds not only a general agreement, but a specific identity between some of its forms and those of the oolite coal of Europe; and he has therefore in a paper in our Transactions, stated that he feels "no hesitation in referring the formation to a place in the oolite system on the same general parallel with the carbonaceous beds of Whitby and Brora, that is, in the lower part of the great oolite group." The most abundant of the coal plants are the *Equisetum columnare*, a large species of *Zamites*, a beautiful fern, *Tæniopteris magnifolia*, and *Tæniopteris scitaminea;* and the remains of these four species, occurring interlaminated with the coal and immediately upon it in great profusion, appear to have furnished the materials of the stratum. No traces of the *Stigmaria ficoides*, so universally associated with the older or palæozoic coal, can be discovered in the soft slates in contact with the coal of this formation. Of *animal remains*, the most interesting is a small fish four or five inches long, referred by Mr. W. C. Redfield to the genus *Catopterus.*

It is perhaps worthy of remark that this genus *Catopterus* of Mr. Redfield, Jr. contains three other species which characterize the mesozoic red sandstone deposits of both Connecticut and New Jersey, and that therefore while there remains some doubt as to the existence of true *Palæonisci* in our American red sandstone, and consequently as to any generic link between it and the Permian strata of Europe, of which the genus Palæoniscus is so definitive, this red sandstone *is* related by such a link in the genus Catopterus to a formation of the oolitic epoch. Thus even as respects its ichthyolite remains, the affinities of the red sandstone are obviously rather to the mesozoic than the palæozoic rocks.

Cretaceous Period.—But if the memorials are scanty of the early and middle mesozoic periods, those of the last or cretaceous age are

* See Reports Geol. Survey of Virginia; also, Memoir in Trans. of Geol. Society of Pennsylvania, by Mr. R. C. Taylor.

full and satisfactory. Perhaps no one group of rocks in North America covers so many square miles, or more abounds in various, expressive and beautifully preserved remains of former life, than our greensands and cretaceous limestones. Commencing on the northeast at the Raritan Bay, these deposits range in a contracting belt along the Atlantic plain through New Jersey into Delaware. There they disappear beneath the tertiary, and though not visible in Maryland, Virginia or North Carolina north of Cape Hatteras, they probably form the floor of the tertiary in all this space; for in North Carolina they emerge again, and hold the same relations to the older and newer systems of strata which they observe in New Jersey. From the Cape Fear river these cretaceous deposits spread southwestward, occupying the seaboard of South Carolina, much of the southern half of Georgia, a large part of Florida, the southern half of Alabama, and the chief part of Mississippi; and west of the great river of that name, they expand so as to underlie the surface of a great portion of the enormous interior basin of the continent from Louisiana and Texas northward to the upper Missouri, and westward probably to the Rocky Mountains. Throughout all the southern tracts, they support detached local patches of interesting tertiary formations, principally eocene.

This whole cretaceous group of the United States is viewed by Dr. Morton and Mr. Conrad, after a careful investigation of the fossils, to consist of three great divisions, called the upper, middle and lower.* The *upper division* includes the nummulite limestone, familiarly called the "rotten limestone" of southern Alabama and the contiguous southern states. The *middle division* consists of a thin but very fossiliferous straw-colored limestone, capping in small patches the greensand beds of New Jersey, and this is supposed by Dr. Morton to be contemporaneous with the European white chalk. The *lower division* embraces the expanded greensand deposits of the Atlantic states and the Missouri basin, and this is considered by Dr. Morton and others to have been formed contemporaneously with the strata which in Europe lie between the chalk and the oolites.

Above the *middle* division or thin limestone of New Jersey, there occur in that state two strata not mentioned in Dr. Morton's classification, the lowest a yellow ferruginous sand with fossils, and the uppermost a coarse brown ferruginous sandstone. As many of the fossils of

* See a tabular view of organic remains of the cretaceous strata, by Dr. Morton, in the Journal of the Acad. Nat. Scien. of Phila., vol. viii, part 2.

the yellow sand are species common in the lower or greensand division, *Ostrea falcata* being one, may not some doubt exist, I would suggest, as to the expediency of placing the intermediate straw-colored limestone so definitely on the parallel of the chalk of Europe? Influenced by the fact just mentioned, and the still more weighty consideration that the American strata in a list of about one hundred and sixty organic forms contain probably not more than six or eight species in common with the cretaceous rocks of Europe, I may be allowed to repeat a suggestion made in my Report on the geology of New Jersey, that a further comparison of the organic remains is required, before we can determine more than approximately the degree of affinity between the several divisions of the cretaceous series of the two continents. Mr. Lyell, who when in this country collected, with Mr. Conrad's assistance, a somewhat extensive group of fossils from the straw-colored limestone of New Jersey, will probably soon give us a more ample insight into the exact degree of affinity subsisting between this stratum and the chalk.

According to Dr. Morton, the shells hitherto ascertained to be common to the cretaceous deposits of Europe and America, are four: *Trigonia alæformis*, *Pecten quinquicostatus*, *Ostrea falcata* and *Gryphea vomer;* and to these links he adds about four species of fishes, and that strange gigantic oceanic lizard, the *Mososaurus.* To these points of agreement we must add those of mere analogy in the remarkable *generic* affinity of the fossils of the two distinct cretaceous basins. But even in some of the more positive links above named, we recognize in the discrepancies of their position in the two series of deposits, the difficulty of establishing an exact equivalency between the strata of basins originally unconnected. Thus, while the *Pecten quinquicostatus* of our greensand or lower cretaceous group is absent from the middle and upper divisions, which have been placed on the same horizon of time with the chalk of Europe, it occurs on that side of the Atlantic in *all* the strata of the series; and again, the *Ostrea falcata*, restricted I believe in Europe to the limits of the chalk or upper cretaceous group, abounds in this country chiefly in the lowest and disappears in the middle. In these instances we see exemplified a general and important law concerning the distribution of fossils, which is, that those species whose geographical distribution is the widest, possess likewise the greatest *vertical* range, or, adapted to a greater variety of localities and physical conditions, they have been suited to withstand a greater series of vicissitude, and to endure therefore a longer time. Such, though usually styled the *characteristic* fossils, are in reality the least characteristic of all; for, while they are invaluable in establishing approximate identity of age between large groups of strata, separated in all other respects from each other, they fail entirely to fix the equivalency of particular members or formations, and from their obtuseness to ordinary changes they are the least instructive of all the species in respect to the special events and conditions of their epoch.

CAINOZOIC OR TERTIARY PERIOD.

Our knowledge of the tertiary strata of the Atlantic seaboard has been considerably advanced during the last three years, through the researches of Mr. Conrad, Mr. Lyell, Prof. W. B. Rogers, Prof. Bailey, Prof. Booth, and Mr. Henry Lea, while Prof. Emmons has made us acquainted with the contents of the modern tertiary of Lake Champlain. We may confidently anticipate further contributions to this very interesting portion of American geology from Mr. Conrad and Mr. Tuomey, who are engaged I believe at present in exploring some portions of the southern strata. The plan which I have assigned to myself in this address will require me to confine myself to the principal results arrived at.

Mr. Conrad, to whom this branch of our geology is so largely indebted, has, in a neat and instructive synopsis of his own labors, printed in the second bulletin of the National Institute, renewed a statement formerly made by him, that our older tertiary is linked to the newest secondary or cretaceous strata by the possession in common of three species of organic remains. Every fact which would tend to restore any part of that lost leaf in the earth's chronology, the absence of which marks the abrupt transition from the secondary to the tertiary periods, would be hailed with general interest; for, although the interruption in the succession of species at the close of the cretaceous epoch is hardly greater than prevails in other portions of geological time; and though the discoveries of Ehrenberg have partially bridged the chasm, yet the very *wide extent* of this horizon of discontinuity throughout Europe and America, lends it much importance. It was therefore a principal object with Mr. Lyell, in his visit to the tertiary strata of the Carolinas and Georgia in 1842, to investigate the evidence for the alleged passage of certain cretaceous species into the lower eocene strata. Having done so, he mentions that he was "unable to find any beds containing an intermixture of cretaceous and tertiary fossils," and he affirms that "the facts at present ascertained will not bear out the conclusion that any beds of passage exist in the southern states."* It is to be observed, how-

* See Proceed. Geol. Soc. London, No. 89.

ever, that the districts which have led Mr. Conrad to an opposite conclusion, viz. the vicinity of Claiborne in Alabama, and of Upper Marlborough in Maryland, were not visited by Mr. Lyell, and it is possible that, had he inspected the whole ground, he might have somewhat modified his opinion. Nevertheless, the evidence presented by Mr. Conrad does not seem entirely convincing, since his account of the conditions under which the *Gryphea vomer* occurs in Maryland and the *Plagiostoma dumosum* was found at Claiborne, do permit the inference that they were swept by currents into the eocene waters from an adjacent upraised cretaceous deposit. Even if it should be proved that these fossils lived associated with the earlier tertiary races, so preponderating are the true eocene forms, both as respects variety and abundance, that it would still seem inexpedient to class the stratum containing the mixture as a transition bed between the secondary and tertiary. Would it not be more philosophical indeed to suppose that the two or three intruding races had escaped the general catastrophe which cut off all the rest of the larger cretaceous species?

Mr. Lyell, I am gratified to perceive, fully sanctions the application of the terms eocene, miocene and pleiocene, to the respective divisions of our Atlantic tertiary, as made by me in 1834, from data derived chiefly from the palæontological determinations of Mr. Conrad, and this latter gentleman now lends the weight of his valuable authority to the correctness of the generalization.

The proportion of living to extinct species in our *eocene* strata appears to be as minute as in the corresponding beds of Europe, amounting probably to not more than one or two per cent., there being about two hundred and fifty species definitely ascertained. The average ratio in our *miocene* is about that in the Faluns of Touraine, Mr. Conrad having identified as living species thirty eight in a list of two hundred and thirty eight at present known to him.

The interesting exhibition of tertiary strata at Gay Head in Martha's Vineyard, referred by Prof. Hitchcock to the eocene period, and by some conjectured to contain fossils washed from a cretaceous formation, has been examined by Mr. Lyell, and is pronounced by him to appertain to the miocene age. It has yielded him some interesting organic remains, none of which are of eocene genera, while several of them are identical with species characteristic of the miocene beds of Maryland and Virginia.

In alluding to the more interesting general determinations connected with the tertiary strata of the United States, I ought not to pass over the discovery made in the wonder field of microscopic life. The remarkable infusorial stratum originally detected by my brother on the Rappahannock, and at Richmond, Virginia, he has since traced from the Meherrin river near the southern boundary of that state, to the vicinity of Piscataway, a few miles south of Washington. The great thickness of the stratum, amounting at Petersburg to thirty feet, and consisting almost exclusively of the siliceous cases of minute infusoriæ, but especially the variety and beauty of the many new species brought to light through the skill of Prof. Bailey and Prof. Ehrenberg, invest this deposit with a high interest. Respecting its geological relations, I would here observe that it is not, as intimated by Mr. Lyell, of eocene epoch, but lies, according to the investigations of my brother and Mr. Tuomey, within and near the bottom of the miocene strata, being underlaid by unequivocal miocene, both at the Stratford cliffs on the Potomac and at Petersburg. The former suspects, indeed, that the infusorial deposit occupies more than one horizon in the miocene. The following short extract from a recent memoir by Ehrenberg, an interesting notice of which has just appeared in Silliman's Journal, exhibits the palæontological affinities of this stratum to the infusorial deposits of other regions and other geological times.

After ascribing the discovery of eleven of the species to Prof. Bailey, Ehrenberg proceeds to say, that up to this time he has observed fifty two forms, among which are about forty six infusoria belonging to twenty genera, which genera are all European with the exception of two, *Goniothecium* and *Rhizosolenia*, which have not been observed at any other locality. Of the species, ten, or almost *one fifth*, are new and peculiar. "Many of the forms occurring in the deposit are, as Prof. Bailey quite correctly concluded from his smaller number of observations, similar to those of Oran, but many of these forms also do *not* occur at Oran." Thus "of the eleven species of the genus *Coscinodiscus*, five occur at Oran which are also found at Richmond, five are found at Richmond alone, and one at Oran alone."

"As a considerable number of the species of animals belonging to the chalk formation of Sicily still exist and consequently cannot be wanting in the tertiary formations, it is evident that no conclusion as to the geological age of these formations, can be drawn from the similarity or dissimilarity of these forms." He goes on to say, that "This group of American forms is of peculiar interest and scientific importance, because the strata at Richmond are decidedly of marine origin and consequently give at once a general view of the marine microscopic animals of the North American ocean; for probably the greater number of species are still living there, as they have already been found abundantly on the German coast of the North Sea. The geological position of the strata must be determined by the order of superposition, the larger included organic remains, &c., as it cannot be decided by means of the infusoria."

In the same memoir, Ehrenberg acknowledges in terms of just praise, the value of the careful researches of Prof. Bailey, gives lists of the fossil infusoria from two deposits discovered by Dr. Charles T. Jackson in Maine, and states that the knowledge of the microscopic organisms of Massachusetts has been much extended by Prof. Hitchcock through the discovery of several deposits there, during his geological survey. All of these deposits are referred, I believe, by their discoverers, to the most modern epoch.

POST PLEIOCENE PERIOD.

The later tertiary strata of this country, though existing in but circumscribed patches, possess much interest on account of the questions suggested by their organic remains, concerning the changes which this portion of the globe has undergone in the level of its surface, and in its temperature during the epochs next antecedent to the introduction of the human race. Of these post pleiocene, or pleistocene deposits as they have been called, several small areas have been described by Mr. Conrad. The principal ones are in St. Mary's County, Maryland, and on the Neuse River below Newbern in North Carolina. To the same period he also refers the numerous small beds of *Ostrea Virginiana*, which skirt the low margins of the islets and rivers in Delaware, Maryland, and Virginia, and by many people attributed to the agency of the aborigines. The deposit on the Potomac in St. Mary's County, is especially interesting for containing several southern species, one in particular, an estuary shell, the *Gnathodon cuneatus*, now restricted to the warmer waters of the Gulf of Mexico. Mr. Conrad infers that the association of the *Gnathodon*, *Mytilus hamatus*, and *Arca ponderosa*, with species now inhabiting our coast as far north as Massachusetts, indicates a climate at the period of the formation, equivalent to that of Florida; perhaps we should say, an aquatic climate. The cause of the change of temperature which banished these shells from the waters of our middle and southern Atlantic bays, connected as it is with some of the widest questions in our science, will, I doubt not, receive hereafter from American geologists and naturalists, the attention which it deserves. Was it the recession of the tepid waters of the Gulf Stream driven eastward by a partial elevation, possibly of the Florida peninsula, or was it connected with the incursion southward of a vast body of icy waters from the north, the same which in the opinion of some was concerned in the dispersion of the drift, or was it the result of some more inscrutable agency?

The post pleiocene deposit on the Neuse River, has been described by Mr. Conrad* as consisting of a shallow stratum, in which the shells with two exceptions, are such as now exist on our southern Atlantic coast and in the Gulf of Mexico, the *Gnathodon cuneatus* being one of the species. Mixed with these shells are the bones and teeth of extinct land animals, such as those of an *elephant*, a species of *horse*, and the *Mastodon giganteum.*

Another small tract of this modern tertiary has recently been described by Mr. J. Hamilton Couper,† as occupying a part of the seacoast of Georgia between the Altamaha and Turtle Rivers. Associated with shells belonging to species now inhabiting the neighboring coast, are the remains of the *Megatherium*, *Mastodon giganteum*, *elephant*, *hippopotamus*, *horse*, and *bison*, all of the four latter belonging it is believed to extinct species.

Mr. Lyell in an instructive paper on the *Mastodon giganteum* and other mammalian fossil remains found at several localities in the United States,‡ mentions the occurrence in Georgia of that curious Proboscidean described by Owen, the *Mylodon*. He also informs us that Mr. Darwin found the mastodon, horse, megatherium, megalonyx, and mylodon, in Patagonia and contiguous districts of South America, and occupying a more recent horizon than certain post pleiocene strata; and some of these extinct animals Mr. Darwin ascertained to be more modern than the drift of Patagonia. It may be remembered that several years ago, in a report to the British Association, he announced the fact, that the mastodon remains in this country lie invariably above the diluvian, a position equivalent, it would seem, to that which they occupy in South America, and I draw from this the inference, that they were not overwhelmed by any sudden catastrophe, but disappeared gradually, being probably overtaken by a progressive chilling of the climate. It is correctly urged by Mr. Lyell, that having lived after the deposition of the northern drift, their extinction cannot have proceeded from any coldness of temperature such as he conceives coin-

* Bulletin of National Institute.

† Proceedings Geol. Soc. London, No. 92. ‡ Ibid.

cided in date with that formation, and this confirms, I think, my view of their gradual disappearance. But it has been clearly shown that contemporaneously with these very fossils, existed the *Gnathodon* and other shells of the Mexican Gulf, indicative of a warm climate in the so called post pleiocene period. Does this not suggest the conclusion, that the expulsion southward of the *Gnathodon* and its tropical associates, was unconnected with the northern drift and its assumed cold, and occurred in all probability contemporaneously with the extinction of the mastodon, at an era subsequent to that which witnessed the strewing of the northern erratics. Whether the other cause I have proposed, viz. the withdrawal from our immediate coast of the Gulf Stream, that great tepid river in the ocean, could produce a sufficient cooling of the climate of the adjacent continent and its coast, to effect at length the extinction of the higher animals and the disappearance of the more susceptible testacea, is a suggestion which those will best be able to weigh, who have studied the influences which that warm current, remote as it is, even now exerts in controlling the climate of the United States.*

POST PLEIOCENE OF THE NORTH.

Turning to the northern districts of the United States, we meet with another formation referable to the post pleiocene period, which is much more widely dispersed than that containing the Gnathodon, and which also sheds additional light on the epoch of the drift. This is the great blue clay deposit which fringes so many of the rivers and lakes from the coast of Maine to Michigan, and from the parallel of the mouth of the

* Since this address was read, some very instructive facts connected with the subject of the climate of the post pleiocene period, have been presented to the Association by Dr. Amos Binney, as part of a valuable report on the land shells of North America. From this report, it appears that in Indiana and some of the adjacent western states, there exists a shallow deposit of clay, first noticed by Dr. Owen, which abounds in fossil land shells. Among them a southern species of Helecina, now rare in the middle latitudes, occurs in the greatest profusion, being as far as the evidence of a single species can reach, an indication of a somewhat warmer temperature. But the value of this discovery is much enhanced by the fact, that the same land mollusks underlie the remains of the mastodon and other large mammalia at Big Bone Lick in Kentucky. Later than the deposition of the drift which they overlie, and earlier than the epoch of the extinction of the mastodon, these fossilized land shells promise when more fully investigated, to furnish a record of an intermediate period, when according to the view of Dr. Binney, a series of shallow lakes existed in the West, and when, as we may conjecture, the temperature of the region was at least as high as during the immediately succeeding era of the mastodon.

Hudson to the lower St. Lawrence. It has been examined by Prof. Emmons and Dr. Jackson in Maine, by Capt. Bayfield, Mr. Lyell and Mr. Logan in the valley of the St. Lawrence, by Profs. Hitchcock and Mather and Emmons in the valley of the Hudson and Lake Champlain, by Mr. Vanuxem on the Mohawk, by Mr. Hall in western New York, and by Prof. Mather on the upper lakes; and the inferences arrived at by some of these gentlemen respecting the physical conditions under which the formation was produced, are not a little remarkable.

Among the most instructive exhibitions of the deposit are those of Lake Champlain and the St. Lawrence, where in certain places some very interesting fossils are met with. According to Prof. Emmons the mass on the borders of Lake Champlain, consists of a stiff blue clay overlaid by yellowish brown clay and this in turn by a yellowish brown sand, and Prof. Mather mentions that in many parts of the Hudson valley these are surmounted by gravel. More than twenty species of marine shells have been procured by Prof. Emmons from the blue clay on the St. Lawrence and Lake Champlain, the two most generally diffused being the *Saxicava rugosa* and *Tellina Grœnlandica.* The greater part if not all of these species, it is stated by our conchologists, now live either on the coast of Massachusetts, or in the Gulf of St. Lawrence, and they therefore imply a climate at the period of the clay deposit, as cold at least, as that which the same region now possesses. Although in the Hudson valley and throughout still wider limits, the blue clay is destitute of fossils, its identity with the stratum of Lake Champlain is not doubted by Profs. Mather and Emmons. The formation though generally thin, attains at certain points on Lake Champlain a thickness of one hundred feet, and Prof. Mather states the whole depth at the town of Hudson, to be one hundred and eighty four feet. It is worthy of remark that the upper surface of the deposit rises in some places in the Champlain valley, to an elevation of between two hundred and three hundred feet above the lake, or more than four hundred feet above the tide, and this agrees with its highest level in the valley of the Hudson.* At Montreal the same stratum has an elevation above the tide of between six hundred and seven hundred feet. These facts have been considered as indicating that a wide tract of the continent, as far south at least as latitude 42°, stood depressed during this recent tertiary epoch below its present level, to the extent of four hundred or five hundred feet, and

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* For the above and other facts, consult Reports on New York survey by Emmons and Mather.

Profs. Emmons, Mather and Hall, and Mr. Vanuxem, rest some interesting speculative views concerning the physical geography of the region in the period of the drift, upon this conclusion.

Here I may be permitted to suggest a caution. We are not I believe yet assured that the clay deposit in all districts belongs to one formation, or is the product of a single epoch, and especially we are destitute of proofs that the stratum which occurs on the great lakes, excepting that on lake Ontario, is of oceanic origin. The clay on the Detroit river is fossiliferous, but whether the shells are lacustrine or marine has not, I believe, been ascertained. Should they prove to be identical or nearly so with those of the St. Lawrence, then the whole amount of depression of the land required to let in the ocean to the present basins of the Upper Lakes, as supposed by Prof. Mather, must be conceded; but for the determination of this identity further observation is required.*

As it has been shown by Mr. Lyell and others, that several of the fossils found in this deposit at Port Kent on Lake Champlain and at Beauport on the St. Lawrence, are identical with species found by him at Uddevalla, and elsewhere in Sweden, and known to frequent the colder latitudes at the present day, he and other geologists conceive that they behold in these facts, proofs of an arctic climate, and this inference has been made to bear on the hypothesis of the origin of the drift. But since nearly all of these shells are stated to exist at present in the Gulf of St. Lawrence and on the coast of New England, they do not necessarily imply a decidedly colder temperature in the waters than may now prevail in certain parts of the great Labrador current.

In addition to the proofs afforded by this post pleiocene formation, of the former lower level of the land and of the nature of the climate, it leads to some important inferences connected with the epoch of the still more extensive drift formation of the same region. It has, I think, been satisfactorily established by Profs. Hitchcock, Mather and Emmons, that the blue fossiliferous clay is newer than that period of erosive action which witnessed the scratching and polishing of the rocky floor throughout the northern part of the continent, for not only does the deposit rest on that striated surface, but there often intervenes, according to Prof. Mather, a bed of gravel and bowlders, which he views as a part of the drift itself, though from this conclusion Prof. Emmons and Mr. Hall seem, if I

* Since this address was written I have been informed by Prof. Mather that he has examined the fossils of the clay of the Detroit river and found them to appertain exclusively to *fresh-water species*.

understand them correctly, to dissent. At the same time it would appear that in the Hudson and Champlain valleys and elsewhere, the tertiary clay is covered with another stratum of drift "composed of coarse gravel pebbles and bowlders," lying on its trenched and denuded surface, and this latest drift, from the magnitude of its erratics, seems not less indicative than the first, of the extent and energy of the transporting agency, whatever that may have been.

Produced in the interval of comparative tranquillity between the two epochs of more vehement disturbance, what let us inquire, is the relative antiquity of this northern tertiary clay compared with the post pleiocene beds of the south, containing the *Gnathodon cuneatus.* These, as we have seen, were contemporaneous with the *Mastodon giganteum* and other large mammalia, and there can be little doubt that the mastodon was posterior to the latest drift of the country, since no erratic deposit covers its remains any where in the region of the drift. The northern post pleiocene, is therefore older in all probability than the southern, by at least the intervening period which produced the later drift.

Reviewing now all the facts respecting the newer tertiary ages, we are led to the following conclusions. That the whole period of the drift was a prolonged one; that the active dispersion of the far transported matter was interrupted by an interval of comparative repose, when a part of the northern country was lower than it now is by at least five hundred feet, and low enough to admit the sea into its valleys; that in this interval the northern waters of this region were quite as cold as they are at present in the same latitude; that after the close of the drift period there was a condition of temperature compatible with the general distribution of the mastodon on the land and with the existence in the waters, as far northward as Maryland at least, of certain shells of the Gulf of Mexico, and that subsequently to this there was an expulsion southward of these southern shells, a slight uplift of the Atlantic coast and an extinction of the gigantic mammalia. Whether these deductions afford any countenance to the hypothesis of some eminent geologists, that the age of the drift was a period of great cold throughout the northern temperate zone, or whether we may not account for all the vicissitudes here recorded, by the simple theory of local modifications of climate by changes in the oceanic currents, I will not here further discuss. Prof. Mather in his report on the geology of New York, has with much ingenuity treated of the possible conditions of the great oceanic currents at the formation of some of our earlier strata and during the epoch of the drift, and although I cannot assent to certain portions of his reasoning respecting the origin of the supposed ancient currents, nor acquiesce in his views of an almost general submergence of the land at the deposition of the drift, yet there is a value in his speculations respecting especially the Gulf Stream, and a general consistency in the whole hypothesis, which claim for his treatise a careful consideration.

GEOLOGICAL DYNAMICS.

Drift—Earthquake Theory—Elevation of Mountain Chains.—The attention of this society has been zealously directed during the last three years, to questions of *geological dynamics.* The phenomena and origin of our anticlinal axis, the nature of the forces concerned in the elevation of mountain chains, and the cause, character and consequences of earthquake motion, have been investigated by my brother, Prof. W. B. Rogers and myself, but the subject which has enlisted the greatest number of pens and called forth the most general discussion, has been among the American geologists as among the European, the interesting and complicated problem of the origin of the superficial bowlder stratum, the great sheet of diluvium or drift. With the phenomena as they exist in New England, a region where they are particularly striking, we have been made familiar through the extended researches of Prof. Hitchcock, Dr. C. T. Jackson and other geologists. At an early day, 1826, a clear account was given through the pages of the American Journal of Science, by Mr. Peter Dobson of Connecticut, of the worn and striated aspect of bowlders in that state, and very soon after this Mr. Nathan Appleton of Boston, to whom this society is under large obligations, first called attention to the universality of the smooothed and grooved surfaces of the rocks wherever they had been protected from atmospheric action. It is due to Prof. Hitchcock to state that early in the history of the geological survey of Massachusetts, being guided by his own observations, he investigated with much minuteness and care the phenomena of smoothed and striated rocks and transported bowlders. Profs. Mather, Emmons, Hall and Dewey, and Mr. Vanuxem, have described the features connected with the drift of New York, and the former geologist embracing a wide survey has examined the phenomena from New England to the Upper Lakes and to the sources of the Mississippi. In the western states the drift deposit has been described also by Drake, Hildreth, Houghton, Lapham, Locke, Owen, Tappan and others.

Bearing upon the same general subject some interesting communications have been submitted to this Association, descriptive of icebergs and their probable influence in dispersing bowlder matter, by Mr. Couthouy and Mr. John L. Hayes. In addition to the valuable facts and views thus imparted, the animated discussions at some of our meetings have elicited from many of the gentlemen mentioned, and from others, much important information.

To embark upon a full view of all that has been done of late in field investigation and discussion connected with this multifarious subject, would lead me quite beyond my limits, and I shall therefore content myself with as brief a statement as possible of the chief generalizations and theoretical conclusions to which the geologists of this country have arrived. And here let me advert to the truly favorable field which North America, with its wide expanse and its peculiar surface, affords us, for testing some of the leading theories of drift now advocated in Europe and this country.

The most important facts connected with the great detrital stratum, are of four classes; those which relate to the grooves or scratches on the rocky surface beneath the drift; those which refer to the distribution of the bowlder material; those which indicate the condition of level of the land at the period of the formation; and those which imply the epoch and duration of the action. The principal phenomena in relation to the surface on which the drift rests are these.

1st, The smoothed and furrowed surface is coextensive or nearly so with the drift stratum, and it occurs at all altitudes, from the summits of the loftiest mountains of New England and New York, to the beds of the valleys, and over the whole broad plain of the lakes and the western states. In the mountainous and hilly tracts, the northern and northwestern brow and flank of each eminence, are much more smoothed and striated than the opposite. The scratches do not radiate from the high mountain summits, but in the vast plains and prairies of the west, among the confused hills of New England, or on the transverse mountain crests of northern Pennsylvania, and western Vermont and Massachusetts, they maintain invariably in all the higher levels a general southeasterly direction. In lower situations, however, on the slopes of the great drainage valleys, their course is diverted to conform more nearly and sometimes with exact parallelism to the direction of the natural barriers and channels. They exhibit a remarkable *general* parallelism among themselves, yet do we seldom meet with a striated surface of any extent which does not disclose more than one set of furrows; the more recent and distinct crossing the fainter one at various small angles. Nor are the scratches truly *straight* over any considerable length, except where the surface containing them is remarkably homogeneous, even and horizontal. On the other hand wherever it consists of harder and softer parts, and where the rock contains imbedded pebbles, or where it has an irregular outline, we may invariably detect a bending, and as it were a free conforming to every inequality on the part of the largest furrows and the minutest striæ. Lastly, where there is much disparity in the hardness of the different parts of the eroded surface, there will be observed a little ridge of the softer portion lying to the southeast or on the lee side of each harder knot or pebble, round which the striæ sweep and meet upon the ridge or tail, precisely as water parts at the prow of a ship and coalesces beyond the stern.

2d. Respecting the drift itself, the following appear to be the principal phenomena:

Throughout all the northern tracts of the United States and the adjoining districts of the British provinces, the surface is covered with a loose stratum composed of sand, clay, gravel and bowlders of all sizes, variously mingled and *locally stratified.*

The stratification is characterized by plains of inclined and confused deposition, denoting turbulent currents.

The materials invariably belong to formations lying north or northwest of their present positions, and great spaces occupied by broad plains, wide belts of hills and even mountains, deep valleys, and vast sheets of water, intervene. The bowlders have evidently not radiated from any local centres of dispersion.

The southern margin of the continuous drift stratum reaches in the east to Long Island and northern Pennsylvania, and in the west to the Ohio river; but its gravel extends along the immediate valleys of the Delaware, Susquehanna and Mississippi, to points much further south.

The direction of the transport of the drift is in each district coincident with that of the scratches.

The size of the rolled fragments progressively declines as we recede from the parent rock southward or southeastward, and though solitary blocks of large dimensions lie in and upon the general stratum, where the imbedding matter has only the coarseness of gravel or sand, yet even there these bowlders obey the general law of a rapid diminution of size towards the south.

In wide and level districts, the bowlders are often strewed uniformly over great spaces of country. In other places, more especially at the base of plateaus or terraces, and opposite to gorges and prominent crests in the hills and mountains, they frequently form long narrow belts, the margins of which sometimes maintain a remarkable parallelism.

The course of the drift and bowlders, like that of the scratches, is obliquely across the crests of most of our mountain ridges; but lower on their slopes, and in the beds of the deeper longitudinal valleys, it conforms partially or entirely to the directions of what would be the great natural channels of drainage, if the whole surface were temporarily or permanently under water.

It should be observed that the bowlders of all sizes are themselves smoothed and striated, and in many cases in such manner as to indicate that this effect has been produced by the fragments rushing past each other.

Lastly, blocks of considerable size have been transported from lower to higher elevations, being seen in New England, New York and northern Pennsylvania, on mountain ridges a thousand or fifteen hundred feet above the level of their parent rocks; and this fact, as Prof. Hitchcock has justly remarked, is one of great importance in the history of our drift.

3d. The third class of facts connected with the drift, relate to the proofs of a lower level in the land at the epoch of its production. In describing the post pleiocene blue clay of Lake Champlain and other northern valleys, I have already cited the proofs that at one period at least in the general era of the drift, the surface of the country in the region of New York and the St. Lawrence was lower in level than it now is by as much as perhaps five hundred feet. It is obvious too that the whole of New England was at the same time somewhat depressed, though there is no satisfactory indication that it was throughout as much submerged as the region of Lake Champlain. It is moreover highly probable that the country of the upper lakes was lower and more overflowed than at present, though it has not yet been established that the depression was sufficient to let in the sea. That the waters of the ocean flowed freely at this particular middle period of the drift through the long and narrow valleys of the St. Lawrence, Lake Champlain, the Hudson and the Mohawk, and ascended those of the principal rivers of New England, and even gained admission to the basin of Lake Ontario, there cannot be a doubt; but that our whole northern region was, as Prof. Mather and Mr. Hall suppose, lower than it now is by fifteen hundred or two thousand feet, and the greater part of New England and New York and the vast area of the western states all at that era beneath the sea, appears to me I confess entirely unsustained by that kind of demonstration which so important an inference demands. Indeed the absence of any marine deposit identifiable with the Lake Champlain clays, from levels higher than the beds of the valleys enumerated, seems a conclusive proof that the only part of the land submersed was in those narrow channels. Thus New England and the mountain region of the Adirondack, were islands separated from the main continent by merely shallow and confined straits.

This conclusion has reference, however, only to the condition of the land during the tranquil interval between the epochs of vehement action denoted by the earlier and later drift deposits; and the main question still remains unanswered, as to what was the degree of submergence of the continent at those periods of commotion. Was "the whole surface" at present overspread with the detrital matter then "permanently covered by water," as some of our geologists suppose, and the depression therefore greater even than in the quiet epoch which intervened; or do we in reality possess one satisfactory monument to record that any part of the surface, at either bowlder period, was below the general level of the ocean. If the presence of marine fossils in the post pleiocene clays is accepted as a convincing argument that the tracts to which they belong were at the time of their deposition beneath the waters of the sea, certainly the universal *absence* in all other districts of any analogous remains, is no less conclusive that the submerged condition which would have infallibly produced them did not exist. To this reasoning it cannot be objected that the sediments of the supposed waters may have been removed by the same currents which brought in the drift; for the waters of the later and perhaps most disturbed of the two drift periods, were manifestly unable to obliterate the limited post pleiocene clays which they overspread and could only partially denude.

4th. Respecting the *age* of the drift deposits of this continent, I have already presented the chief facts hitherto discovered which seem connected with the inquiry. That the whole belongs to a later age than that of the miocene tertiary, is evident from the superposition to beds of that date in Martha's Vineyard; and there can be little hazard in assigning it to an epoch in that relatively recent though vaguely defined period of the tertiary, denominated by Lyell and other geologists the post pleiocene. Commencing before the era of the Champlain fossiliferous clays, the same energetic and wide dispersion of detrital matter was repeated after its close, and yet the whole was apparently terminated before the epoch of the mastodon and megatherium. Of the *time* occupied in the formation of the drift, we have not data even for conjecture. It must have been immense indeed, if icebergs were the principal agents of dispersion; nor could it have been brief, even if produced by a succession of paroxysmal disturbances. Yet the whole period constitutes, as it were, but a *single beat* of that slow-swinging pendulum which has counted the innumerable successive stages in the geological history of our globe.

The principal hypotheses proposed for explaining the detrital phenomena, are—

First, the theory which attributes the scratches on the rocky floor of the drift, and the dispersion of the far-carried fragmentary materials, to the agency of ice, creeping forward with a slow velocity but an enormous momentum, like the glaciers of the Alps, grinding down and finely grooving the jagged asperities of the surface, and bearing on its back the collected rubbish in the mountain slopes, and strewing this still further by a rapid thaw:

Secondly, the theory which imputes the whole to icebergs, loaded with detrital matter, and floating southward until stranded on the surface of the submerged land, which the ice-fields are conceived to have smoothed and scored through the agency of innumerable fragments frozen into their lower surfaces:

Thirdly, the theory which supposes no general permanent submersion of the land, but imagines one or more paroxysmal movements of the earth's crust in the higher northern latitudes to have sent a portion of the contents of the Arctic seas—water, ice, and fragmentary rock—in a succession of tremendous deluges southward across the continent.

Other explanations, consisting in the main either of an union or of modifications of the chief features of these hypotheses, have also been suggested and find advocates. Which of these doctrines is to be deemed most in accordance with the phenomena of the drift on this continent, is a point which still causes considerable diversity of opinion, and discussion is still busy in relation to each branch of the problem, that is to say, the origin of the smoothed surfaces and striæ, the cause of the wide dispersion of the erratics, the source of the currents, and the condition of level of the land. Upon the question of the origin of the polished and grooved surface of the whole rocky base on which the drift reposes, many of our geologists conceive that ice was essential to the production of this phenomenon; but some, sharing the caution of Prof. Hitchcock, refrain from "attempting to decide whether it has been ice in one vast sheet acting by mere expansion," or the same in the form of stranded icebergs. Others, among whom is Prof. Mather, think that "there can be no doubt that the scratches on the rocks are due to the movements of floating ice, containing masses of rock frozen in, and grinding upon the bottom." Prof. Emmons, on the contrary, conceives that "the phenomena in the main are independent of the action of icebergs," which he believes "to be very poorly adapted to polish, groove and score rocks," and he urges that their motion when they are grounded is rotatory, and therefore not such as to produce striæ deviating so little from a prevailing direction as those which we behold. He thinks the grooved surfaces have been overflowed by wide shallow rivers, which have smoothed and scored the rocks by pushing along gravel, sand and ice; and confining his view to New York, he supposes that these rivers communicated with the Atlantic on the south through the Champlain, Hudson and Mohawk valleys, and that they bore along ice loaded with sand and pebbles, which scratched and grooved the surfaces of the rocks. He thinks that the erosion occurred before the true bowlder epoch.

Mr. Hall suggests several objections to its production by angular fragments set in the bottom of icebergs or icefloes. He mentions the divergence of many furrows from their regular course as indicative of a freedom of motion in the grooving body, and he calls attention to the minuteness of the striæ as implying that they were probably caused by sand and gravel moved by some superincumbent even surface, "not unlike the polishing of marble when the motion is all in one direction." Both Mr. Hall and Prof. Emmons suggest moreover, that the bottom of the ocean would be necessarily covered with detrital matter, which would protect the rocky floor from the direct graving action of icebergs.

My brother and myself entertaining very similar objections to the explanation of the phenomena by icebergs, have ventured farther, and perceiving no necessity for supposing that the cutting fragments and particles were ever pressed upon by ice, have appealed to the enormous erosive power which a thick and ponderous sheet of angular fragmentary rock would possess, if driven forward at a high velocity under the waters of a deep and general inundation, excited and kept in motion by an energetic upheaval and undulation of the earth's crust during an era of earthquake commotion.

Respecting the agencies concerned in the strewing of the detrital matter, a considerable diversity of theoretical opinion prevails among those geologists who have recently written upon our drift. Nevertheless, the doctrine which ascribes the transportation of the bowlder matter to icebergs driven by currents over a permanently submerged surface, finds evidently the greatest number of advocates. Some geologists however, believing this hypothesis insufficient to explain the phenomena, appeal to the theory which supposes a series of inundations of the land engendered by a violent paroxysmal movement of all the northern latitudes. Permit me to examine briefly some of the principal features in these doctrines, and the leading arguments connected with them.

An extensive submergence of all the northern tracts of the continent is of course implied in the supposition, that the detrital matter has been floated to where it now rests by icebergs. Thus Prof. Hitchcock, who thinks that the greater part of the phenomena must be explained by icebergs, conceives that nearly the entire surface of the land must have been beneath the level of the sea, and against the opinion that diluvial currents could have rushed across the continent while it stood at nearly its present elevation, he objects that the assigned causes for such, are insufficient to send an ocean to the summit of our mountains, five or six thousand feet above its proper horizon.

Mr. Hall in like manner, urges that no explanation of the mode of transport of the bowlders reconcilable with their present situation, can be offered, which does not assume that the whole surface was permanently covered with water,* and he thinks that a depression below the present level of as much as two thousand feet, is required for the transport and deposition of the bowlders forming the later drift of southern New York. He is led to the conclusion, that they were "not moved by any powerful flood." He supposes that the "mountain chains of New England and New York formed long ranges of islands rising from the ocean to two or three thousand feet above its level, their sides covered with perpetual snow and glaciers, and their bays terminated by cliffs of ice from which detached masses floated off, bearing with them bowlders and fragments of rocks," and he thinks, "that it can be demonstrated that this dispersion of the bowlders and fragments continued for a long period, while the land was rising from the ocean." "After the land had risen to within eight hundred or one thousand feet of its present elevation, the great valley of Lake Ontario would form a broad bay communicating with the ocean through the valleys of the Mohawk

* See page 336 of Mr. Hall's Rep. Geol. New York.

and Susquehanna, the communication by the valley of the Mississippi becoming closed." At this stage of partial conversion to dry land, both Prof. Mather and Mr. Hall conceive, if I do not mistake their views, that the tertiary clays of Lake Champlain and the adjoining valleys were deposited. Mr. Hall, adverting to the position of the post pleiocene clays of Lake Champlain in relation to the drift, believes that "the facts clearly establish distinct and widely distant periods between the formation of the great body of the drift in western New York and the erratic blocks or bowlders," and he conceives "that the scouring and polishing of the rocks has taken place at a period long anterior to the transportation of these northern bowlders, and that their passage over the surface has had little or no connection with this phenomenon."

Very similar views as to the physical condition of the region now covered with drift, appear to be entertained by Prof. Mather. Describing the phenomena throughout an extensive area, and assuming about the same amount of submergence, this geologist has entered into an elaborate and ingenious enquiry respecting the character and direction of the great systematic currents which should prevail under the supposed distribution of land and ocean. Conceiving that the configuration of the continent at the drift period was in the main the same as at present, he shows that the great polar or Labrador current and the Gulf Stream being the results of this configuration and the laws of aqueous motion, connected with the rotation of the earth, and its belts of different temperatures, these currents must have existed then equally as at the present day. The Labrador or polar current possessing necessarily a *westward* travel, he supposes to have flowed over the northern parts of the United States, bringing ice loaded with detritus. The Gulf Stream, deriving from the rotation of the earth an *eastward* tendency, he supposes to have been parted by the mountain chain then having the form of a great peninsula or island, and one portion to have flowed up the wide plain or valley of the Mississippi, melting by its warmth the ice of the Labrador stream, and causing its freights of rocky matter to be deposited over the country north of the Ohio and Missouri rivers. The tendency of the southern current to set eastward would, Prof. Mather thinks, convert the westward direction of the ice-bearing current into a southeasterly one, and he thus explains the southeasterly course which the detritus has evidently taken.

Prof. Emmons, in his view of the condition which attended the dispersion of the drift, conceives that the first detrital stratum and the erosion of the rocky floor, were produced by currents of the nature of broad shallow rivers, and that subsequently the land subsided and continued beneath the ocean long enough for the deposition of the Champlain tertiary, upon which the icebergs at the same time brought bowlders from the north to form the upper or later drift.*

The hypothesis of a general depression of the surface at the detrital epoch is objected to by Mr. Vanuxem, who has himself been a careful explorer of the phenomenon of the drift in New York, on the ground that " the absence of all marine productions whatever, excepting those which form a part of the materials of the alluvium, (meaning the post pleiocene clays,) is in opposition to any but a very transient submersion." This argument, as I have already intimated, appears to me conclusive. The whole method of geological reasoning requires, that we should find a marine deposit before we can assume the presence of the ocean, while analogies derived from every other geological period show that in the supposed condition of general submergence, the great, steady currents which floated those fleets of icebergs must also have wafted in *some* sedimentary matter and left *continuous strata*, however thin, of clay, fine sand, or marl, if not every where, at least in the more tranquil tracts of that extensive sea. Yet not even outside of the drift, along its southern border, do we find a trace of any such deposit. This total deficiency of all proofs of a permanent overflowing of the land, must, I think, be viewed as fatal to the iceberg theory. During the progress of the limited post pleiocene marine deposit, it is quite conceivable that ice from the neighboring lands did play some part, dropping bowlders from time to time on the bed of those inlets of the sea which occupied the present valleys of the St. Lawrence and Lake Champlain; but these very bowlders I would trace to the adjacent earlier drift. Such icefloes belong in reality less to the epoch of either drift, than to an intermediate one in which the physical circumtances were more nearly those of the present time.

But if we admit, for the sake of concession, that the wide-furrowed floor of the drift was permanently beneath the water, the explanations given will be found to be still at variance with several incontrovertible considerations. The idea that the icebergs may have come from the

* Since this address was read, Prof. Emmons has sent to the Association a brief paper, containing, I believe, some essential modifications in his views respecting the origin of the earlier drift. He now attributes it, if I understand him correctly, to a general and rapid movement of waters from the north, the explanation advocated for the last three years by my brother and myself.

coasts of mountain islands, such as, on one hypothesis, the Adirondack region and the White mountains were, is contradicted by the important fact, made known by Prof. Hitchcock and Prof. Emmons, that those high tracts have not been centres of dispersion, but like every other spot, have been invaded by the drift from the north. But there is also another opposing consideration. The extent of the drift throughout the northern regions of this continent is immense ; yet by this theory nearly the whole of it and of northern Europe was depressed below the sea at that period, and I confess I look over that vast imagined ocean of the north in vain for the conditions of physical geography, compatible with the arctic winter supposed. The conceived state of things, an enormous expanse of waters, with here and there an island, in place of a broad continent in the higher latitudes, is the very converse of that distribution which is compatible with great cold and with islands and fields of ice.

In reference to the generalizations of Prof. Mather regarding the direction and agency of oceanic currents at the period of this assumed depression, I cannot refrain from expressing my belief that the hypothesis of an ancient Gulf Stream, not flowing however up the valley of the Mississippi, but, during certain periods at least, along the line of our great Atlantic tertiary belt, is sufficiently in accordance with the probable ancient configuration of the land and with geological memorials, to promise important aid in certain speculative questions in our geology. I encounter, however, some difficulty in understanding how the two great currents, the polar and the southern one, would produce a resultant movement which would strew the drift in the direction which it obviously took. The general course of the furrows in the rocky surface, and of the trains of detrital matter, is from about north northwest to south southeast, and I cannot conceive how a current setting to the southwest could be turned by one setting in the opposite direction into a southeasterly course, or how a single direction could, by such a conflict, be imparted to such a vast area of waters, as wide in latitude and longitude as the region now occupied by the drift; or how, if it could be thus deflected, it should be able to retain the high velocity which it must have held in order to distribute the bowlders in their long narrow trains, to round the angles of the hills, and score and gutter the very hardest rocks.

Let us now give our attention for a moment to the paroxysmal theory, which I cannot but think will be found, on careful examination, to be more in agreement with the admitted laws of physical dynamics than either of the more popular hypotheses of the day. This doctrine, appealing to the proofs which our science furnishes of the sudden disturbances of the level of the different tracts of the earth's surface, at all periods of geological time, merely supposes that at the epoch of the drift, the polar half of the northern hemisphere was the theatre of violent and perhaps frequently repeated movements of the earth's crust, each particular disturbance emanating probably from a different local region. These disturbances, which are conceived by Van Buch, De Beaumont, Hopkins, De la Beche, Sedgwick, Phillips, and other distinguished geologists, to have been of the nature of simple *paroxysmal elevations*, and by my brother and myself to have consisted in an energetic and extensive *undulation* of the crust of the earth accompanying each sudden rise, are deemed sufficient to have caused a rush of the northern waters over all the higher latitudes of Europe and North America, covering the surface with an almost continuous sheet of gravel and bowlders, and polishing and scoring the whole rocky floor.

The chief cause of hesitation with many minds in embracing a theory so much in harmony with the general physical history of our globe, has arisen from their not recognizing a force sufficient to dislodge and sweep onward blocks of the huge size which we sometimes encounter, or to drive the detrital matter up and over the high mountain barriers, across which, by some process, it had travelled. So long as no definite estimate has been made of the velocity of the current which would result from a given amount of paroxysmal elevation, such a distrust of the energy of diluvial waters was natural and prudent; but we are in possession of facts and generalizations calculated greatly to exalt our conceptions of this power.

It has been shown by Mr. Hopkins, of Cambridge, reasoning from the experimental deductions of Mr. Scott Russell upon the properties of waves, that "there is no difficulty in accounting for a current twenty five or thirty miles an hour, if we allow of *paroxysmal elevation* of from one hundred to two hundred feet;" and he further proves that a current of twenty miles an hour ought to move a block of three hundred and twenty tons, and since the force of the current increases in the ratio of the square of the velocity, a very moderate addition to this speed is compatible with the transportation of the very largest erratics any where to be met with, either in America or Europe.

Holding in view these demonstrable conclusions, let us consider the far *more* enormous velocity which a broad general current would derive from that mode of paroxysmal action, *earthquake undulation*, which constitutes, as my brother and myself have endeavored to show, an essential feature in all movements of elevation. Regarding such disturbances as a true billowy pulsation of the flexible crust of the globe, we have deduced from data connected with some of the best authenticated earthquakes, the extraordinary progressive velocity of the undulations of the ground, and have shown that when the pulsation has been imparted to the sea, the vast waves engendered have moved at the amazing speed of one mile or more per minute. Making every abatement for resistance from the comparative shallowness of a continental inundation, the phenomena of earthquakes fully justify us in the belief that the broad and rapid onward undulations of the ground would be propagated to an uplifted sea above, and the gigantic billows be propelled across the surface of the heaving land, with a velocity and a propulsive energy approached by no other possible terrestrial current.

If we will conceive, then, a wide expanse of waters, less perhaps than one thousand feet in depth, dislodged from some high northern or circumpolar basin, by a general lifting of that region of perhaps a few hundred feet, and an equal subsidence of the country south, and imagine this whole mass converted by earthquake pulsations of the breadth which such undulations have, into a series of stupendous and rapid-moving waves of translation, helped on by the still more rapid flexures of the floor over which they move, and then advert to the shattering and loosening power of the tremendous jar of the earthquake, we shall have an agent adequate in every way to produce the results we see, to float the northern ice from its moorings, to rip off, assisted with its aid, the outcrops of the hardest strata, to grind up and strew wide their fragments, to scour down the whole rocky floor, and, gathering energy with resistance, to sweep up the slopes and over the highest mountains.

Perhaps I may be permitted, before quitting the class of topics connected with geological dynamics, to allude to the researches upon which this theory of earthquake action has been founded, and to refer to the phenomenon which it is conceived to explain.

At the last meeting of the Association, we deduced, from an analysis of a large mass of data connected with the recent earthquakes of the Mississippi valley and of the West Indies, and from the history of other earthquakes, a striking confirmation of some of the laws of earthquake motion long ago suggested by Mitchell. From the facts set forth, I think it can no longer be doubted that a characteristic feature of earthquake motion is a rapid progressive undulation of the ground, of the nature of a series of actual billows or waves, which are sometimes of the vast amplitude of several miles, and chase each other with an enormous velocity, equal in many instances to twenty or thirty miles per minute. This movement we are disposed to impute to an actual pulsation in the fluid lava mass, upon which the thin crust of the earth is supposed to rest, excited by a sudden rupturing and instantaneous collapsing of the crust, rent by the tension of highly elastic steam and other vapors.

In a previous year we drew the attention of the Association to the remarkable structural features of the Appalachian chain, and showed that these laws of earthquake action furnish perhaps the only solution of the origin of those grand flexures and folds into which the Appalachian strata have been bent, so extraordinary for their regularity, great length, parallelism, wave-like form, and progressive subsidence westward; and in illustration of the power of earthquakes thus to produce permanent anticlinals, we instanced among other cases the elevation in 1819 of Ullah Bund, a low, broad mound, fifty miles in length, lifted from the flat plain of the delta of the Indus by the great earthquake of Cutch.

Having now passed in review some of the more important general conclusions in relation to our geology to which recent researches have led, we may turn for a moment before closing to contemplate the magnitude and enticing interest of the field for future discovery, which these explorations have made accessible.

When we reflect on the enormous expansion of some of our great systems of strata, nothing short of the entire ancient seas in which they were deposited, and regarding their excessive thickness, permit our thoughts to dilate until they can take in the true areas which they occupy in space, and the ages of time which they reveal, and then consider to how great an extent each layer collected in those ancient seas is now exposed to view, on the flanks of our huge mountain chains and in the banks and cliffs of our mighty rivers and their unnumbered tributaries, and above all when we advert to the true *nature* of each stratum, the treasures which it contains, the secrets which lie locked within it,—we become aware of the inexhaustible variety and the grandeur of the problems connected with the geology of this continent.

Let us not think that with the completion of the explorations now so actively in progress, with the mapping in of the outcrops of the strata, and the description of their organic remains, the field for investigation will have become exhausted. It will in fact only have become opened up for more minute and critical research. The utmost perhaps that we of this generation can hope to do, is by uncovering the main parts of the buried temple to disclose its vast dimensions and some portion of its elaborate external beauty; but to penetrate its shrines and read upon its inner walls the whole narrative of its origin and construction, is the glorious privilege which awaits a future age.

Geology grows more interesting as we penetrate into the deeper secrets of the past, as we leave the obvious and commonplace phenomena and reach the recondite and remote, in the midst of which, as in the moral and intellectual recesses of the human constitution, would seem to lie the only true and actual indexes of the great forces which sway events. It is this very power to reconstruct the past which confers on geology its most distinctive feature, and has placed it in so eminent a rank among the sciences.

I question if many minds, even among those devoted to geological research, are impressed with the wonderful extent to which this science is likely in future ages to carry the restoration of antiquity, reproducing in vivid distinctness the ancient geography, climate, and inhabitants of the globe, tracing the many successive oceans, bays, and shores, and repeopling for each epoch all the waters and the land; I question if we are at all aware how *completely* the whole history of all departed time lies indelibly recorded with the amplest minuteness of detail in the successive sediments of the globe, how effectually in other words every period of time *has written its own history*, carefully preserving every created form and every trace of action.

Each broad stratum, to the very *thinnest*, be it remembered, was once the sustaining surface of its region, supported therefore all that the earth then possessed of its teeming generations, and received in some form, either perfect or mutilated, every living or organized thing. While the waters above it were the cradle and the theatre of a multitude of races, *it* was the universal tomb, receiving them all into its soft bosom in *every stage* of their life, and thus recording the minutest particulars of their individual biography. Let us reflect too that in these successively superimposed surfaces we have sequences of continuous time of all amounts, from intervals the minutest to ages the most protracted, so that we behold the birth, the spread and the extinction of long enduring races, no less than of individual beings, and illustrated by the movements of every contemporaneous physical condition and event. The *life of races* is thus disclosed, and how magnificent and vast are its higher and profounder laws compared with those which mark the development of single and fast fleeting individuals.

The creative spirit that broods over nature and has clothed matter in the garb of time, not only confers on each special being its special features and functions, but links it in long and mysterious relationship to others, past and to come, thus elaborating in the longest periods the highest generalizations. To discover and read the laws which have controlled the successive aspects of life upon this planet, is to recognize perhaps the very loftiest class of physical truths which human research can ever hope to unfold.

But, gentlemen, I must desist, having already exceeded my just limits. I pause because my pages are full, and not because my topics are exhausted. So extensive is the harvest which the geologists of the United States have of late been reaping, that I have not found it practicable to count over all the gathered store, or mention more than a portion of the products of some of the richest fields. This creditable accession to the scientific wealth of the country, the fruit in part of liberal legislation, owes much of its value to the intrepid zeal and the excellent spirit of mutual fellowship and coöperation enlisted in producing it. Whatever may be the scientific worth of the discoveries made in this and other paths of knowledge, or however cheering the prospect of the yet ampler developments that will surely attend future enquiry, let us not forget that to win for our labors the approval of the wise and good, they must bear the seal not only of physical but of moral truth; must show, as I trust they do, that in studying Nature's great laws, while our perceptions of the beautiful have been quickened, and our reason disciplined, the yet diviner faculties of our being have been exercised in the cultivation of a generous charity and a mutual kindness.

Art. IV.—*Comparison of Gauss's Theory of Terrestrial Magnetism with observation;* by Elias Loomis, Professor of Mathematics and Natural Philosophy in the University of the city of New York.

[Communicated to the Conn. Acad. of Arts and Sciences, July 23, 1844.]

The appearance of a general theory of terrestrial magnetism from a name so celebrated as that of Gauss, furnishing the three elements of Variation, Dip, and Intensity, if not with an accuracy equal to that of observation, at least with an approximation affording a very good general representation of the phenomena, and by a method independent of any particular hypothesis as to the distribution of the magnetic fluids in the body of the earth, should stimulate every friend of science to contribute his mite towards perfecting the theory. With this view, I have computed from Gauss's theory (see Taylor's Scientific Memoirs, Vol. II, Art. 5) the three elements for each station of the four tables given in this Journal, Vol. xliii, pp. 99–102, and for comparison have added the data from observation as far as they could be obtained. The observed variations are taken from the article above quoted, pp. 109–114. Those marked with an (*) were strictly observed, the others were derived from the observations by a method explained in the same article, which also furnishes the reduction to 1837. The observations of dip are to be found in the same article. For the eastern states the observations are reduced to 1837 by assuming the annual motion $-0'{\cdot}5$; for the western states no correction is applied. The observed intensities for the eastern states are from the Transactions of the American Philosophical Society; those for the western states are from Executive Documents, 1839–40, Vol. VI, No. 239.

TABLE I.

PLACES.	VARIATION.			DIP.			INTENSITY.		
	Gauss.	Loomis.	Diff.	Gauss.	Loomis.	Diff.	Gauss.	Obs.	Diff.
	° ′	° ′	° ′	° ′	° ′	′			
Montreal,	+5 23	+10 1*	−4 38	77 24·3	76 55·5	+28·8	1·7131	1·805	−0·0919
Oswego,	1 50	4 30	2 40	75 53·5	75 6·3	47·2	1·7123		
Utica,	2 30	5 31	3 1	75 44·2	74 53·2	51·0	1·7076		
Syracuse,	1 42	4 36	2 54	75 36·0	74 45·3	50·7	1·7095		
Schenectada,	3 16	6 34	3 18	75 34·5	74 40·0	54·5	1·7028		
Albany,	3 19	6 44*	3 25	75 28·0	74 32·9	55·1	1·7015	1·8097	0·1082
Cambridge,	5 2	8 57*	3 55	75 21·4	74 24·0	57·4	1·6925		
Dorchester,	5 3	8 55	3 52	75 19·4	74 21·7	57·7	1·6921	1·7858	0·0937
Worcester,	4 29	8 8	3 39	75 15·9	74 17·7	58·2	1·6939		
Springfield,	3 48	7 13	3 25	75 7·1	74 7·7	59·4	1·6953	1·8254	0·1301
Longmeadow,	3 46	7 11	3 25	75 4·2	74 4·4	59·8	1·6950		
Providence,	4 24	8 13	3 49	74 58·0	73 56·0	62·0	1·6905	1·8116	0·1211
Hartford,	3 33	6 43*	3 10	74 52·5	73 50·9	61·6	1·6936		
West Point,	2 24	6 40*	4 16	74 31·8	73 29·0	62·8	1·6947	1·8334	0·1387
New Haven,	3 5	6 0*	2 55	74 31·0	73 27·0	64 0	1·6916	1.7800	0·0884
New York,	2 3	5 23*	3 20	74 1·6	72 55·7	65·9	1·6904	1·803	0·1126
Princeton,	1 26	4 22	2 56	73 43·4	72 36·8	66·6	1·6897	1·8075	0·1178
Philadelphia,	+0 55	3 52*	2 57	73 22·1	72 14·6	67·5	1·6881	1·8021	−0·1140
Baltimore,	−0 15	2 14	2 29	72 44·1	71 38·4	65·7	1·6860		
Washington,	−0 39	+1 41	−2 20	72 22·8	71 17·5	+65·3	1·6837		

Table II.

PLACES.	VARIATION.			DIP.			INTENSITY.		
	Gauss.	Loomis.	Diff.	Gauss.	Loomis.	Diff.	Gauss.	Obs.	Diff.
Toronto,	−0° 17′	+1° 55′	−2° 12′	75° 48′·5	75° 18′·9	+29′·6	1·7185		
Buffalo,	0 17	+1 25*	1 42	75 21·0	74 47·2	33·8	1·7143		
Detroit,	3 11	−1 44*	1 27	74 34·8	73 35·7	59·1	1·7173		
Cleveland,	2 38	0 32*	2 6	74 4·0	73 4·0	60·0	1·7103		
Sandusky,	3 14	2 10	1 4	73 58·1	72 53·1	65·0	1·7120		
Kinsman,	1 55	0 21	1 34	74 8 4	73 14·5	53·9	1·7084		
Bedford,	2 31	1 14	1 17	74 0·2	73 0·3	59·9	1·7094		
Hartford,	1 58	0 23	1 35	74 2 2	73 6·7	55·5	1·7076		
Basetta,	2 5	0 31	1 34	74 1·2	73 4·8	56·4	1·7078		
Aurora,	2 27	1 8	1 19	73 58·1	72 59·0	59·1	1·7087		
Twinsburgh,	2 31	1 11	1 20	73 57·6	72 57·7	59 9	1·7088		
Warren,	2 9	0 43	1 26	73 57·8	73 0·5	57·3	1·7075		
Windham,	2 18	0 53	1 25	73 55·8	72 57·2	58 6	1·7077		
Shalersville,	2 24	1 1	1 23	73 54·9	72 55·5	59·4	1·7080		
Streetsboro,	2 28	1 6	1 22	73 54·3	72 54·2	60·1	1·7081		
Hudson,	2 32	1 5*	1 27	73 53·7	72 53·2	60·5	1·7083	1·8070	−0·0987
Tallmadge,	2 35	1 13	1 22	73 46 6	72 45·1	61·5	1·7073		
Clinton,	2 46	1 27	1 19	73 39·0	72 35·4	63·6	1·7067		
Fulton,	2 45	1 26	1 19	73 36·8	72 33·0	63·8	1·7063		
Beaver,	2 2	0 22	1 40	73 34·8	72 36·1	58·7	1·7032		
Dover,	2 46	1 54*	0 52	73 19·8	72 14·6	65·2	1·7034		
Pittsburgh,	1 52	0 8	1 44	73 27·2	72 29·1	58·1	1·7012		
Frasersburgh,	3 15	2 6	1 9	72 56·8	71 46·2	70·6	1·7013		
Hebron,	−3 30	−2 34	−0 56	72 46·5	71 33·4	+73·1	1 7005		

Table III.

PLACES.	VARIATION.			DIP.			INTENSITY.		
	Gauss.	Loomis.	Diff.	Gauss.	Loomis.	Diff.	Gauss.	Obs.	Diff.
Gros Cap,	−3° 13′	−2° 52′	−21′	77° 32′·7	77° 16′·0	+16′·7	1·7404		
Fort Brady,	2 59	2 42	17	77 32·7	77 13·6	19·1	1·7397		
Mackinac,	3 25	2 20*	65	77 4·1	76 39·7	24·4	1·7376		
South Manitou	4 18	3 54	24	76 25·5	76 0·9	24·6	1·7355		
Ann Arbor,	3 37	2 41	56	74 30·7	73 29·4	61·3	1·7179		
Ypsilanti,	3 35	2 38	57	74 28·0	73 25·8	62·2	1·7174		
Monroe,	3 32	2 33	59	74 14·4	73 8·6	65·8	1·7151		
Toledo,	3 39	2 42	57	74 2·9	72 56·3	66·6	1·7137		
Maumee,	3 45	2 50	55	73 56·6	72 50·4	66·2	1·7130		
Piqua,	4 25	3 58	27	72 41·5	71 33·9	67·6	1·7030		
Urbanna,	4 9	3 32	37	72 42·4	71 30·2	72·2	1·7021		
Columbus,	3 39	2 59	−40	72 41·5	71 23·6	77·9	1·7007		
Springfield,	4 13	4 23*	+10	72 33·8	71 21·4	72·4	1·7009		
Dayton,	4 27	3 58	−29	72 24 6	71 15·1	69·5	1·7001		
Lebanon,	4 31	4 9	22	72 8 6	70 58·4	70·2	1·6975		
Hamilton,	4 36	4 24	12	72 3 1	70 56·6	66·5	1·6974		
Mason,	4 35	4 10	−25	72 4·5	70 55·1	69·4	1·6970		
Cincinnati,	4 46	4 55*	+ 9	71 49·3	70 41·4	67·9	1·6950	1·7407	−0·0457
Will'mstown,	4 52	4 48	− 4	71 23·4	70 15·0	68·4	1·6906		
Louisville,	5 29	5 29	0	70 59·8	70 1·4	58·4	1·6885	1·7518	−0·0633
Frankfort,	5 1	4 52	9	71 3·3	69 55·9	67·4	1·6874		
Lexington,	4 51	4 38	13	70 59·0	69 48·1	70·9	1·6860		
Clay's Ferry,	−4 53	−4 40	−13	70 47·7	69 36·6	+71·1	1·6840		

TABLE IV.

PLACES.	VARIATION. Gauss.	Loomis.	Diff.	DIP. Gauss.	Loomis.	Diff.	INTENSITY. Gauss.	Obs.	Diff.
Prairie du Chien,	−7° 53′	−9° 13′*	+1° 20′	74° 18′·0	73° 30′·7	+47′·3	1·7283	1·7806	−0·0523
Madison,	6 54	7 38*	0 44	74 30·5	73 41·1	49·4	1·7279	1·8117	0·0838
Campbell's,	7 3	8 56*	1 53	74 26·8	73 37·7	49·1	1·7272		
Blue Mounds,	7 9	8 46*	1 37	74 24·6	73 35·6	49·0	1·7268	1·7841	0·0573
Hickok's,	7 15	8 31	1 16	74 21·7	73 32·7	49·0	1·7266		
Mineral Point,	7 22	8 48*	1 26	74 14·8	73 24·8	50·0	1·7258	1·7656	0·0398
Platteville,	7 41	8 52	1 11	74 6·0	73 15·5	50·5	1·7250		
Turkey river,	7 50	9 8*	1 18	74 1·3	73 11·1	50·2	1·7249	1·7617	0·0368
Little Mahoqueta,	7 41	8 38*	0 57	73 54·6	73 2·3	52·3	1·7236	1·7644	0·0408
Dubuque,	7 36	8 30*	0 54	73 54·0	73 1·2	52·8	1·7233	1·7672	0·0439
Galena,	7 31	9 33*	2 2	73 54·5	73 1·2	53·3	1·7232		
Sherwood's,	7 44	9 8*	1 24	73 50·7	72 57·8	52·9	1·7231		
N. branch Mahoq.	7 52	9 43*	1 51	73 45·6	72 52·4	53·2	1·7226	1·7631	0·0405
Whitewater,	7 45	9 18*	1 33	73 43·3	72 49·1	54·2	1·7221	1·7791	0·0570
Mahoqueta,	7 55	8 53*	0 58	73 37·6	72 43·3	54·3	1·7216	1·7659	0·0443
Farmer's Creek,	7 37	9 16*	1 39	73 41·2	72 45·8	55·4	1·7215	1·7880	0·0665
Small mill,	7 58	9 15*	1 17	73 28·9	72 33·2	55·7	1·7205	1·7642	0·0437
Iron Ore,	7 46	8 28*	0 42	73 24·4	72 26·8	57·6	1·7194	1·7976	0 0782
Chicago,	6 3	6 35	0 32	73 46·0	72 43·1	62·9	1·7183		
Wapsipinnicon,	7 39	8 30*	0 51	73 18·4	72 18·9	59·5	1·7182	1·7772	0·0590
Lost Grove,	7 31	8 18*	0 47	73 16·2	72 14·7	61·5	1·7172	1·7767	0·0595
Davenport,	7 36	8 21*	0 45	73 5·1	72 3·0	62·1	1·7160	1·7695	0·0535
Peru,	6 57	7 32	0 35	73 10·3	72 6·0	64 8	1·7153		
Pekin,	7 15	7 43	0 28	72 26·5	71 16·8	69·7	1·7092		
Copperas Creek,	7 21	7 49	0 28	72 20·7	71 10·7	70 0	1·7085		
Montrose,	7 35	8 16	0 41	72 8·1	70 57·4	70·7	1·7070		
Marion,	7 36	8 9	0 33	71 38·2	70 23·8	74·4	1·7020		
Louisiana,	7 54	8 24	0 30	71 15·0	69 59·8	75·2	1·6988		
Bunker Hill,	7 27	7 43	0 16	71 6·5	69 47·7	78·8	1·6962		
Monticello,	7 33	7 49	0 16	70 58·7	69 39·7	79·0	1·6951		
Upper Alton,	7 32	7 48	0 16	70 57·2	69 38·1	79·1	1·6948		
Alton,	7 33	7 54*	0 21	70 56·2	69 37·0	79·2	1·6947		
Edwardsville,	7 28	7 45	0 17	70 54·8	69 34·3	80·5	1·6950		
Vincennes,	6 21	6 29	0 8	71 7·2	69 43·3	83·9	1·6929		
St. Louis,	7 33	8 44*	1 11	70 41·3	69 20·7	80·6	1·6919	1·7482	−0·0563
Paoli,	5 50	5 51	0 1	71 8 5	69 42·6	85·9	1·6914		
Princeton,	6 23	6 29	0 6	70 49·7	69 24 1	85 6	1·6898		
New Harmony,	6 32	6 39	0 7	70 36 8	69 10·6	86·2	1·6881		
Mount Vernon,	−6 33	−6 40	+0 7	70 26·4	68 59·3	+87·1	1·6862		

From the zeal which is now manifested in respect to magnetic observations, we may expect that in a few years the errors of Gauss's theory for every part of the globe will be pretty well known; when it is to be hoped the illustrious author will resume the discussion, and give to his theory the greatest perfection of which it is capable.

Art. V.—*Selections from an Ancient Catalogue of objects of Natural History, formed in New England more than one hundred years ago;* by John Winthrop, F. R. S.

Remarks.—This catalogue is a curious original document, presenting a picture of the times, in regard to the objects to which it relates. In this view, it may be worth preserving, and although it contains some errors, (owing to the imperfect science of the day,) it does great credit to the industry and spirit of observation of the actual collector, whose remarks are frequently sagacious and just. Mr. Winthrop was grandson of the first governor of Connecticut, great grandson of the first governor of Massachusetts, and grandfather of the late Hon. Th. L. Winthrop, Lt. Governor of Massachusetts. He was graduated at Harvard College in 1700, and became a magistrate in the colony of Connecticut, but left it on account of a question relating to inheritance, which he wished to refer to the king in council; he never returned to his country, but died in England in 1747.

He was a Fellow and a most conspicuous member of the Royal Society, as his grandfather had been one of its founders. Being like him an industrious collector of natural objects, he presented more than 600 specimens, chiefly minerals, to the museum of the society, and was, after Mr. Colwell, the greatest contributor up to that time.

The 40th volume of the transactions of the society, was dedicated to him by their secretary, Mortimer Cromwell. An original copy of the catalogue in Mr. Winthrop's handwriting was placed in our hands by the consent of the late Lt. Gov. Winthrop of Boston, through the kind offices of the Hon. John Davis of the same city. It is a fair and beautiful MS. and perfectly legible after the lapse of 109 years from the time it was written. To remove all doubt as to its author, the present American minister in London, the Hon. Edward Everett, at the instance of Robert Winthrop, Esq., also of Boston, the respected son of Lt. Gov. Winthrop, made application to the secretary of the Royal Society for the examination of their records, and the assistant secretary, Mr. Robertson, very kindly copied out the whole gratuitously, and we now present the most interesting portions of it as the first rudiments of American natural history, and especially of mineralogy.

It is scarcely necessary to remark that the frequent citations of gold, silver, mercury and tin, are generally erroneous.

We have preserved the signs of the metals and have not altered the orthography or phraseology, wishing them to tell faithfully of the times from which they have come.*

Explanation of characters in the MS.

☉ Gold.	☽ Silver.	☿ Mercury.	♀ Copper.
♂ Iron.	♃ Tin.	♄ Lead.	c. containing.

Extracted from the Journal Book of the Royal Society, Vol. XV, p. 451 *to* 487. *June* 27, 1734.

Mr. Winthrop presented several curiosities from New England, as contained in the following list, which being read he had the thanks of the Society. These curiosities are a part of a large collection shewn at several meetings during the subsequent winter, and the whole catalogue to which these numbers refer, is entered after the minutes of the day.

Quadrupeds.

1. Omitted.

Serpents.

2. Fel Serpentio caudisoni. Four grains for a dose, cure all sorts of fevers and agues, taken in a spoonful of spring water. The gall liquid is preserved for use, by dropping it on the fine powder of chalk.

Fish.

3. The fins of the dog-fish of the size of a dog, with four short legs, and the tail like a fish. 'Tis a sort of seal.

4. Stones out of the head of a codfish; which powdered are given for the strangury and gravel.

Shells.

7. A sort of Nerites, which never grow larger. The Indians boil them and make strengthening broth of them.

9. A larger sort of Nerites; one with very small Balani growing upon it.

14. Buccinum nostro productione, with a chain of their ovaries, which are sometimes twenty or thirty yards long.

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* The Nos. which are omitted are all given in the manuscript, but the entries are so entirely unimportant that we have stricken them out, as we found that by retaining them we should swell the article to an inconvenient length; they are of this description, e. g.—" No. 12. Small Buccina," and the like.—Eds.

15. A piece of the shell of the Poquahauges, a rare shell-fish, and a dainty food with the Indians. The flesh eats like veal; the English make pyes thereof; and of the shell the Indians make money. This piece is worth two pence.

16, 17, 18, 19, 20. The same, of different values. They are called Wampampeege.

21. Young Poquahauges, Pectunculus fasciatus.

22. The wreaths of the Buccinum, of which the Indians make their money or white Wampampeege.

28. Clams, white. Their broth is most excellent in all intermitting fevers, consumptions, &c. These clams feed only on sand.

29. A very curious sort of gold-coloured pearl shells on the sea-coast near the shore. Those with marks are such as have born pearls; which powdered make the best testaceous powder in the world.

30. Unripe pearls, which in time would have become (31).

31. Bright pearls, which are produced in the same shells (30).

32. Some of the larger sea pearl shells, which are often found in deeper waters three times as large, and bear larger pearls.

N. B. Almost all the lakes, ponds, and brooks, contain a large fresh-water clam, which also bears pearls. The Indians say they have no pearls in them at certain seasons: but at the season when they grow milky, the pearls are digested in them, which causes their milkyness.

33. Shells of the razor-fish, (Solares,) which calcined the Indians mix with bear's grease, and therewith cure the piles. They drink the water, in which they are boiled together with the powder of the shells.

Insects.

34. Moths. A fine large butterfly with velvet wings, furbelow'd, and eyes on them like the rounds on peacock's feathers.

Vegetables.

36. Some red cedar wood rotten, from the middle of a post, which was sound on the outside; which shows that the common opinion that cedar never rots is false.

38. Touch-wood; being the bark of the red oak. The Indians kindle fire with it, by striking two flints together.

40. A sort of Sena from Elizabetha Island, New England. It dies an excellent black, and grows in great plenty. Prinos glaber.

41. Leaves of a plumb, which grows in swampy ground. It is an evergreen, that dyes an exceeding fine shining black: and it surpasses Sena.

42. An evergreen, with which the Indians cure the dropsy and strangury, boiling the leaves and small branches in spring water, when they are sick, and drink it in fevers. It grows plentifully in the country, and bears a spicy red berry, which the turtle-doves and partridges eat.

43. Roots of the sassafras tree, which the Indians boil and drink in fevers.

44. A root called by the Indians dram-root; because it warms their stomach like a dram.

45. Bloody root, (Sanguinaria.) It grows on the banks of Quinebaug River. The juice is like blood. The Indians use it in consumptions and fevers, to cure the bite of the rattlesnake, the bloody flux, &c.

46. Sunkucesowange, a root, with which the Indians cure cancers in the breast.

47. Squianange, a root, with which the Indians cure consumptions.

49. Mountain roots from Connecticut. The Indians chew them to expell wind.

50. Myrtle berries, of which are made candles and soap. (Myrica.)

51. One of the candles and pieces of the soap (of 49).

53. Indian beans bearing very long pods.

54. Pods, seeds, and silk of the silk-grass. It grows every where in North America, and in New England. The poorer sort of people make beds of it. Fine hatts, &c. may be made thereof. (Asclepias.)

55. The wool and seed of one sort of snake-weed, which grows almost every where in New England. It bears a purple red flower like the columbine. After the leaves of the flower fall off, it shoots out into long buttons at the top, which in autumn open, and contain this wool. The Indians cure the bite of the rattlesnake with the root, and stop bleeding with the wool.

56. Nutts from their resemblance called negro-heads, which grow on trees in Bermudas and Barbadoes.

58. Beach plum-stones, which never grow higher than the knee on the barren sand-beach. It is a very pleasant fruit.

59. A sort of Agaric, which the Indians use as touch-wood, and burn a small place with it behind their ear upon the vein, and say they never have the toothache afterwards on that side.

62. A sort of indigo made out of the wild indigo wood, which grows all over New England. The juice of this plant rubbed on horses, &c. keeps the flies from stinging them.

Fossils.

64. Fragments of shells dug up thirty feet deep in making a well three miles from the sea: great quantities of other shells were found in the same place. No water was found.

65. A piece of red cedar petrified in a short time.

Earths, Clays, &c.

70. A grey whitish earth with red streaks, containing cinnabar or ☿?

71. A reddish grey earth, a leader to cinnabar?

72. A flesh-coloured earth from the Gay-head, where are divers coloured ochres.

73. A light red earth, wherewith the Indians paint their faces when they go to war: from the Indians from the inland parts.

74. A reddish earth from Quinipiack, used internally for bruises.

75. A red earth (containing iron?) with which the Indians paint themselves. They bring it a month's travel up the country.

80. Earth that will swim, from Connecticut River, near Thirty Mile Island.

[Nos. 76 to 79 and 81 to 91 are only different colored clays, mostly from "the *inland parts*."]

Soft Stones.

92. A grey sandstone, like grindstone, not far from where the natural whetstone are found.

97. Dark reddish stones with black talc, from Newayunck near the sea.

Slates.

100. A slate which the Indians scrape into water and drink, when they have received any bruise.

101. A silver-coloured slate, which calcined is of a fine gold colour.

104. A sort of blew slate, containing alum, from the inland parts.

107. A fissile stone with mica, which burnt looks like ☾. Another sort of it resembles ☉.

109. A bright shining flakey mineral like burnished steel, from the woods at Tantinsquese.

115. A soft flakey greasy stone from Point Juda.

Marble and harder Stones.

116. Two sorts of whitish grey marble, from the uplands.

117. White stones of the marble kind, near the Massachusetts.

118. A stone used in building, containing granates from Connecticut Island in Naraganset Bay.

128. A blewish stone coated with a greyish green, the sides parallel, from the uplands.

140. Heavy brown glittering stone between Wachuset Hill and Connecticut.

143. A black stone with specks of Marcasite, from Tantinsquese.

144. Fragments of black, greenish and white stones, brought by the Indians from the uplands.

Pebbles.

145. Round pebbles, like No. 149, from near the same place, but where they are all of this form. When polished they are transparent as crystal.

149. Oblong white pebbles, with an amethyst line, from the beach on Fisher's Island: where they are all of this form.

150. Blew and white flat pebbles, from a spring, where there is a quantity of them.

151. Reddish irregular pebbles, from an iron-spring, where all the stones are of the same sort for a good way where the water flows.

153. Reddish bowlder stones, of which consists a great hill in the upland parts.

155. Small irregular stones, which compose a small beach at the southwest corner of Long Island.

Alumen plumosum, (Asbestos.)

158. The stone, between which the Alumen plumosum is found. It makes the best furnaces that can be, bearing the fire beyond any thing known. It is found near Plainfield, on Quinebauge River, and also in several other parts of the country. (See No. 159.)

159. Alumen plumosum. The stone (No. 158) where this is found, makes the best furnaces in the world. It will endure the strongest fires.

Talc.

165. A gold talc, taken up in a swamp, where it is in great plenty.

[Nos. 166 to 181 all the same—"gold, silver, and blewish talc."]

Spars.

182. White spar from the top of a very high hill in the uplands.

184. A white spar, as it is found upon a small beach in a fresh-water pond up the country.

185. White spar, with black gritt, containing steel; near Colchester.

191. A flesh-coloured spar, a leader to richer oars.

196. Spar with a blewish stone adhering, from the high cliffs of Sandwich beach.

207. Fragments of dark reddish and black spars, from Clam-Pudding Pond, in Plymouth colony.

210. Flakes of an odd sort of spar.

211. Shining spar, found in great plenty in the place it comes from in the uplands.

212. Spar from near the Spar-hill.

216. Stone composed of different coloured small grains of spar, with mica intermixt, leaders to ores.

218. Spar, a leader to ♃.

219. A white spar with flakes of pyrites, a leader to ♃ and ♄, from Poquanock.

220. White spar, a leader to ♄.

221. Crystal spars from the bottom of a well of fine water.

Ludus.

225. A Ludus like that of Paracolono, and doubtless equal to it, and as good.

Regular Stones.

226. Mineral Bozoars, from the uplands.

227. Clay generated in the form of horse shoes, from the bottom of Connecticut River. (Doubtless clay-stones.—Eds.)

228. Otites from Martha's Vineyard.

229. A sort of Otites. (Probably fossil shells.)

230. Several pieces of eagle stones.

Precious Stones.

231. Large granates, as big as nutmegs, c. ⊙ and ♂.

232. Several sorts of granates, and a piece of rock with some granates in it, c. ⊙.

Crystals.

234. Pieces of crystal, from an entire hill of it in the inland parts.

236. Yellow crystals in pointed squares, from the high white rock called Lanthorn Hill.

Sand.

237. Amethyst sand flung up by the waters of a spring near Naumeaug, three miles from the beach, where a large quantity of the same sort is found.

238. Amethyst sand, from the beach near Pequott, below the harbour's mouth, containing gold.

246. A white gritty sand from the side of a large fresh-water pond, used by the English to whet their sythes with it.

[Nos. 239 to 245 and 247 to 257, white, gray, brown and black sands.]

Salts.

258. A sort of nitric earth of a darkish colour, with mineral sparkles in it, brought by young Hyams, the sachem's son, from Shawshawnittewange.

259. Alum stone, up in the country.

260. A vitriolic earth.

Sulphurs, &c.

262. Sulphur from the inland parts near the great high mountain, Monadnuck. It is apprehended that hereafter, by some accident or other a volcano will break out thereabouts. There is ♀ earth in many other places of the country, the effects of which may have been the cause of several earthquakes which have happened there.

263. Coal from a swamp's side.

264. A sort of jet or coal from the side of a swamp.

Ores of Metals.

♀.

267. Copper ore, from Nyamesis, near Merimancke River, thirty miles from Boston.

268. A copper ore, green and shining.

♂.

274. A rich iron ore from Pettiquamscutt.

275. Iron ore from near Providence, called bogg ore.

276. A sort of hæmatites from the upper lands above New Haven.

277. Loadstone from near Acqunck.

278. A bright natural steel ore, very magnetic.

280. Steel ore from near Tantinsquese.

286. A brownish, flakey iron ore, from the banks of Hartford rivulet.

289. A dark iron stone resembling Ætites.

293. Small smooth stones like vetches, from the bottom of Merimanke River, c. ♂.

295. A gold talc, c. ♂, from Connecticut.

303. A greenish and black stone, c. ♂ and ♃.

304. A mineral sand from Concord, in New England, c. ♂ and ♃.

♃.

306. Tin ore near Lyme.

[Nos. 307 to 318 are only sand and spar c. ♃.]

♄, &c.

319. Fine lead from the upland parts.

320. Fine black lead c. $\frac{1}{5}$ of ☾, from Tantinsquese; which makes fine furnaces and crucibles.

321. Spar, in which the black lead grows.

323. A sort of bismuth, from Hudson's River above New York.

Marcasites, (Pyrites.)

327. Fragments of greenish sulphureous marcasite, from Mount Tom and Holyoke, each side Connecticut River.

329. A rich marcasite of ☾.

330. A marcasite of ♀, near Mendum.

331. Cubic marcasites c. ☾.

333. Marcasites from among the black lead, from Tantinsquese.

N. B. One sort of pyrites always relents in moist air.

340. Pyrites c. ♀, from Acqunck-hill.

345. Black and white speckled metallic stones, from a pond's side in Fisher's Island.

348. A black mineral, very heavy, from the inland parts of the country. (Is this the Columbite ?*)

351. A black mineral resembling burnt wood.

Artificial things.

352. A bundle of Indian candles, or splinters of pitch-tree.

353. Alba mater.

Additions to the preceding Catalogue.

354. Shawshaws, a shell. Pectunculus fasciatus.

364. A piece of pewter half melted by lightning, and a piece of the shelf it stood on, half shattered but not burnt, with a Belemnites found two feet deep in the ground underneath. The earth was black round the hole, and had a strong sulphureous smell. And the smoke continued half an hour after in the room, though nothing was set on fire.

Art. VI.—*Abstract of a Meteorological Register for* 1832–43, *kept at Rio de Janeiro;* by John Gardner, Esq.—(Communicated by I. W. Andrews, Professor of Mathematics and Natural Philosophy in Marietta College, Ohio.)

During a recent short sojourn at Rio de Janeiro, I received from Mr. Gardner, an intelligent merchant of that city, an abstract of a register which he has kept for the last thirteen years. Although limited to a single daily observation of the thermometer, and the general state of the weather, yet I have thought it worthy of preservation in a permanent form, and with that view forward it for insertion in the American Journal. The thermometer was observed each day at 12 o'clock. Its location was in the second story, within the room, but close by an open window. The temperature I should judge to be but little different from that of the external air, in a place protected from direct and reflected heat.

* It has been supposed that the original specimen on which Mr. Hatchett made the discovery of *columbic acid* was sent in this invoice, and that some hint as to the locality from whence it came might be had; but we find no other entry than this which corresponds at all with what Mr. Hatchett says, which is—"Upon referring to Sir Hans Sloan's catalogue, I found that this specimen was only described as 'a very heavy black stone with golden streaks,' which proved to be yellow mica; and it appeared that it had been sent with various specimens of iron ores to Sir Hans Sloane, by Mr. Winthrop of Massachusetts. The name of the mine or place where it was found is also noted in the catalogue; the writing however is scarcely legible—it appears to be an Indian name, (Nautneauge.)" We must therefore rest content probably in ignorance of the exact locality of that interesting specimen; although mineralogists have, on what evidence does not appear, considered New London as the locality.—Eds.

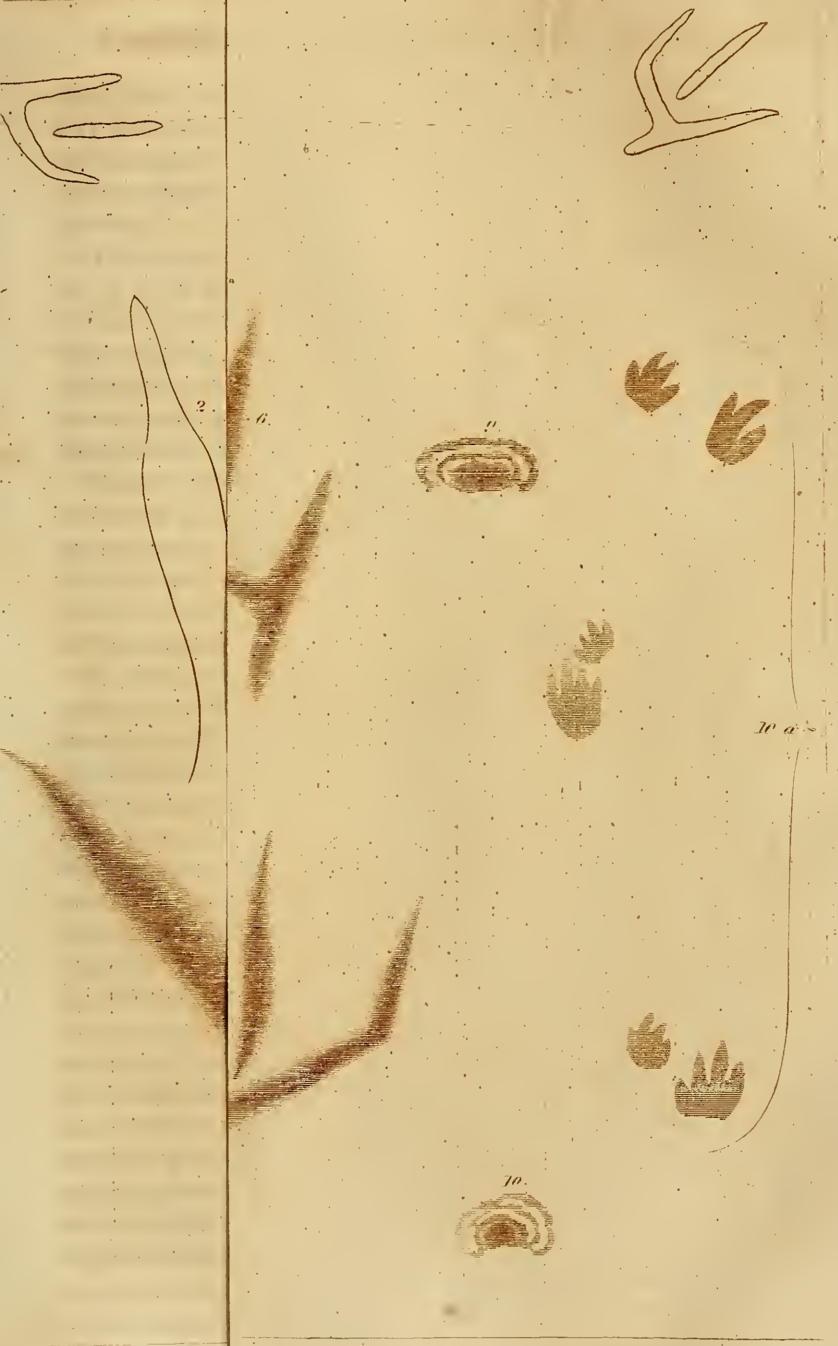
Months.	Mean temperature at noon.													Mean of highest range.	Mean of lowest range.	Mean number of fair days.	Mean number of cloudy days.	Mean number of rainy days.
	1832.	1833.	1834.	1835.	1836.	1837.	1838.	1839.	1840.	1841.	1842.	1843.	Aver-age.					
Jan'y,	83·1	82·5	83·5	82·0	83·5	82·0	82·1	81·7	82·6	83·3	83·3	84·4	82·8	88·6	76·0	19.5	4·58	6·92
Feb'y,	85·2	87·0	84·0	84·7	85·2	79·8	83·1	82·1	84·7	81·3	83·6	86·8	84·0	88·3	78·2	20·5	3·16	4·50
March,	81·4	83·3	81·3	78·5	82·7	78·6	78·9	80·2	78·8	84·3	84·0	82·2	79·5	86·9	76·1	20·9	4·58	5·50
April,	76·3	80·7	78·3	76·3	75·6	75·5	77·1	77·4	77·9	80·5	80·0	77·6	77·8	82·7	72·9	19·3	5·50	5·25
May,	73·5	74·3	77·5	73·6	71·1	73·5	71·7	74·5	76·2	74·5	75·1	78·3	74·5	78·9	70·2	20·1	5·83	5·75
June,	73·3	73·3	70·0	71·3	70·8	68·0	72·4	71·5	72·4	71·3	75·3	71·1	71·7	76·1	68·1	22·1	3·83	4·17
July,	72·0	72·2	73·5	74·0	74·3	70·0	72·3	69·6	71·5	71·3	74·3	68·9	72·8	75·7	67·6	22·6	4·58	3·83
August,	73·5	76·0	72·7	75·0	75·8	72·5	70·9	72·1	71·4	72·9	73·3	74·5	73·0	78·8	66·8	22·2	4·66	4·17
Sept'r,	75·0	78·3	76·7	72·4	72·1	74·1	75·4	72·8	76·2	74·5	71·4	76·6	74·9	80·3	70·0	19·1	5·91	5·00
Octob'r,	75·6	79·0	75·3	76·0	75·0	77·0	77·5	74·0	79·8	75·7	76·0	77·0	76·5	83·6	70·8	19·3	5·75	5·92
Nov'r,	78·0	83·0	76·0	77·7	76·3	77·0	77·4	75·9	78·8	77·3	77·5	78·1	77·8	85·1	72·9	17·3	6·16	6·58
Dec'r,	80·0	84·0	80·7	78·9	79·9	78·0	82·3	80·6	82·8	77·2	80·3	82·0	80·6	86·9	75·3	18·8	4·41	7·83
Av'age,	77·2	79·5	77·3	76·7	76·8	75·5	76·7	76·0	77·7	77·0	78·3	78·1	77·2	82·6	72·1	20·1	4·91	5·45

Years.	Fair days.	Cloudy days.	Rainy days.
1832,	227	83	56
1833,	276	40	49
1834,	235	59	71
1835,	238	52	75
1836,	210	79	76
1837,	245	60	60
1838,	228	70	67
1839,	219	67	79
1840,	231	67	68
1841,	256	50	60
1842,	258	48	59
1843,	273	35	57
Av'age,	241·3	59.2	64·8

ART. VII.—*Report on Ichnolithology, or Fossil Footmarks, with a Description of several New Species, and the Coprolites of Birds, from the valley of Connecticut River, and of a supposed Footmark from the valley of Hudson River;* by Prof. EDWARD HITCHCOCK, LL. D. of Amherst College.

(Read before the Association of American Geologists and Naturalists, at Washington, May 11, 1844.)

ICHNOLITHOLOGY, or as it is denominated by Dr. Buckland, Ichnology, has only recently been admitted as a branch of paleontology. It was a great advance upon our previous knowledge, when Cuvier demonstrated experimentally, "that when we find merely the extremity of a well preserved bone, we are able, by a careful examination, assisted by analogy and exact comparison, to determine the species to which it once belonged, as certainly as if we had the entire animal before us." But if this principle was, and still is, doubted by some able men, still more sceptical should we expect them to be, and still more sceptical they have actually been, as to the position that we are able to determine the character of an animal from its footmark. Yet this is the fundamental principle of ichnolithology. Even here however, we find that so far as one tribe of animals are concerned, the sagacious mind of Cuvier has anticipated this principle. "Any one," says he, "who observes merely the print of a cloven hoof, may conclude that it has been left by a ruminant animal, and regard this conclusion as equally certain with any other in physics or morals. Consequently this single footmark clearly indicates to the observer the form of the teeth, of the jaws, of the vertebræ, of all the leg bones, thighs, shoulders, and of the trunk of the body of the animal which left the mark. It is much surer than all the marks of Zadig." It required only to extend this principle to other tribes of animals, to constitute ichnolithology in its present state. Whether it can be confided in as implicitly in regard to other animals as in regard to the ruminants, is questionable. Nor is it probable that Cuvier, when he wrote the above, had any idea that tracks would ever be found in solid rock sufficiently perfect to indicate the animal that made them; much less without any other evidence of their existence. The difficulty of conceiving how tracks could be petrified, has indeed been with most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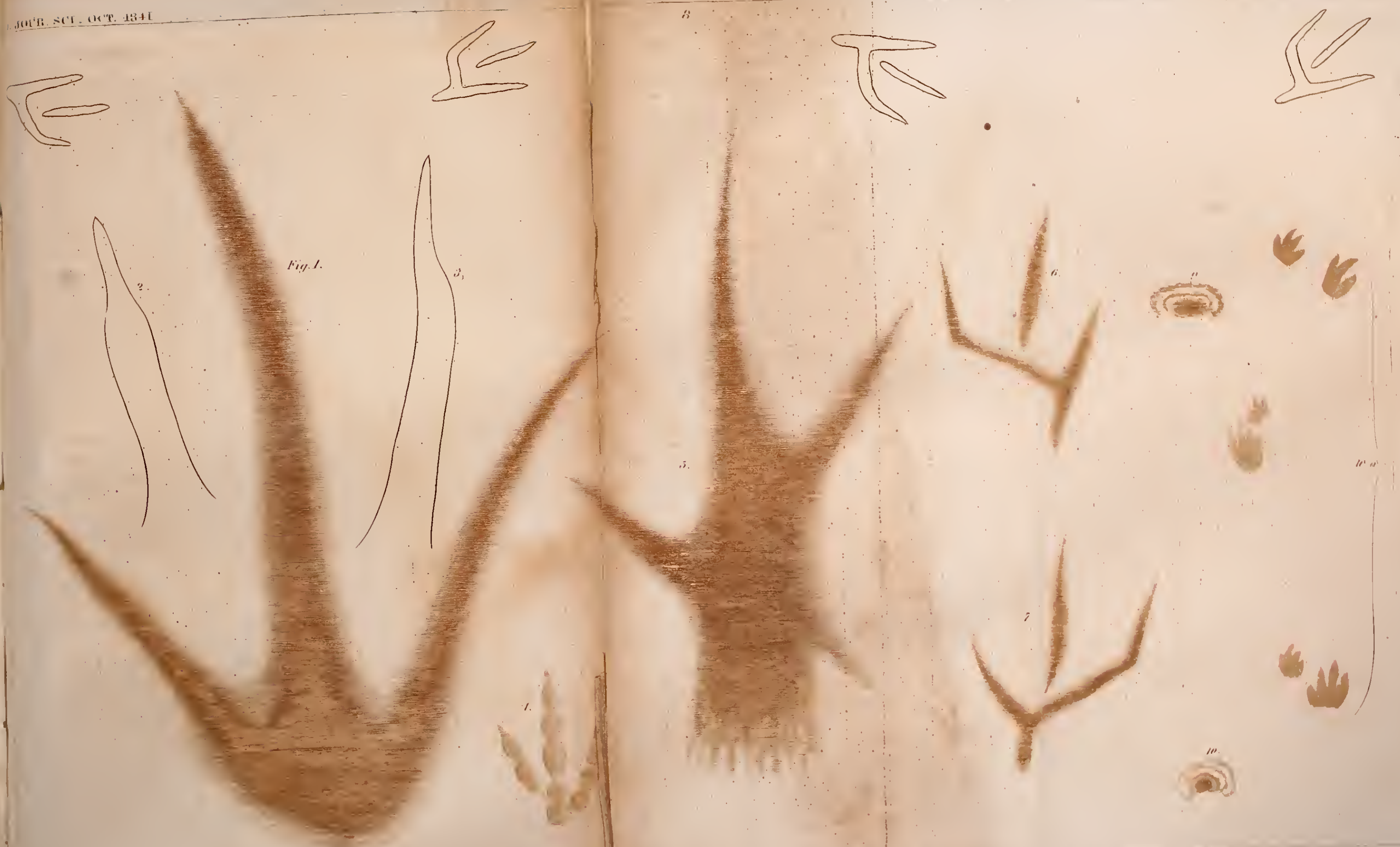

ICHNOLITES.

minds the grand objection to their existence. And yet, almost any brick kiln will furnish examples of the perfect preservation of footmarks, rain drops and other impressions made upon the clay in a plastic state, and which has been subsequently indurated by heat.

The earliest trustworthy description with which I am acquainted of fossil footmarks, was given by Rev. Dr. Duncan in the eleventh volume of the Transactions of the Royal Society of Edinburgh for 1828. They occur on the new red sandstone of Dumfriesshire, in Scotland, and were made by tortoises. They are figured by Dr. Duncan, and also by Dr. Buckland in his Bridgewater Treatise.

In 1831 Mr. G. P. Scrope found numerous small footmarks on the layers of the forest marble north of Bath in England. They are figured in the Journal of the Royal Institution of London for 1831, as well as in Dr. Buckland's Bridgewater Treatise. They exhibit traces not merely of the claws of the animal, but of his tail or stomach.

In 1834 an account was published of tracks upon the new red sandstone of Saxony, near Hildberghausen. The largest tracks were those of an unknown animal, to which the provisional name of Chirotherium was given, from the resemblance of its track to the human hand. The fore foot in some specimens, was eight inches long and five wide, and the hind foot four inches long and three inches wide. They were supposed by Dr. Hohnbaum and Prof. Kaup, who first described them, to have been made by a marsupial animal. But the investigations of Mr. Owen on the gigantic Batrachians of the new red sandstone, render it extremely probable, although not certain, that the Chirotherian tracks were produced by animals of this description. Of one genus of this tribe, the bones of the head, pelvis and scapula have been discovered, and the animal called Labyrinthodon by Mr. Owen, must have been at least as large as an ox. He must have been larger to have made some of the tracks on the Saxon rock, and especially those nine inches long and six inches wide, upon the rocks of England, more recently discovered.* M. Link had early suggested that some of the German tracks were made by gigantic

* Specimens of these impressions in the rock are now in the cabinet of Yale College, and also in that of Amherst College.

Batrachians. He regards them as having been produced by four species.

The first account of the fossil footmarks on the new red sandstone of Connecticut river, was published by me in 1836, in the January number of the American Journal of Science. Seven species only were described, of which five were three-toed, and two species four-toed. Two species were *pachydactylous*, or thick-toed, and five *leptodactylous*, or narrow-toed, the difference between the two classes being very striking. These tracks were boldly, perhaps rashly denominated *Ornithichnites*, or *stony bird tracks*. But when I came to give an account of a much larger number of species, in my Final Report on the Geology of Massachusetts, I changed this name to *Ornithoidichnites*, or *tracks resembling those of birds;* and *Saurioidichnites*, or *tracks resembling those of Saurians*, as more in conformity with the cautious spirit of true science than the former name.

My Final Report was published in 1841; and having in the four preceding years devoted much time and attention to the subject of footmarks, I was enabled to describe and figure of the natural size in that work, no less than twenty seven species. At the meeting of the Association of American Geologists at Boston in 1842, I gave an account of five species more, and figures of these, also of the natural size, were given in the first volume of their Transactions. In the present communication I propose to describe four species more, although I shall be obliged to strike two from the previous list, so that the whole number of species which I consider established to the present time, is thirty three.

It was to be expected that when it was announced that bird tracks,—some of them four times larger than those made by the ostrich,—existed as low down as the new red sandstone, all geologists would receive the statement with great scepticism. They would not have been true to their principles if they had suffered any thing but the most overwhelming proofs to satisfy them. For no trace of birds had hitherto been found deeper than the Wealden formation. I was well aware, when I first published on the subject, with what incredulity my conclusions would be viewed. To a person not familiar with the details of paleontology, there is not any thing very remarkable in the statement, that birds existed at the period of the new red sandstone, and left their footmarks on its layers; and accordingly, the community in general had only to look at the specimens in order to be satisfied that such was the origin of the tracks in question. Nay, many of these specimens were so striking, that scarcely any observer needed to be told that they were bird tracks. Four out of five, I presume, would draw this conclusion at once. Indeed I have sometimes enquired of boys from ten to twelve years of age, when showing them specimens for the first time, what they thought of them; and the usual reply has been, "they are bird tracks, made when the rock was soft." Accordingly, I have found that whenever these impressions have been found in the valley of the Connecticut, and at whatever period, this has been the almost invariable conclusion. In my first account of this subject in the Journal of Science, I stated that "this is the conclusion to which the most common observer comes at once, upon inspecting the specimens. But the geologist should be the last of all men to trust to first impressions" —and it was not till after long and careful investigation, that I felt prepared to maintain this opinon before the scientific public.

Among the means by which so general an acquiescence has at last been obtained in this interesting conclusion, was the appointment, by this Association, of a large and able committee to visit the localities. Their candid report was republished in Europe, and undoubtedly carried great weight with it. Another means was the visit of several distinguished foreign geologists to the localities, such as Dr. Daubeny and Mr. Lyell; who, on their return, expressed their adhesion to the views which I had advocated. But the recent discovery of the Dinornis of New Zealand, has probably done more than any thing else, to remove the objections of such men as Richard Owen, Dr. Mantell and Mr. Murchison; men eminently qualified to judge of the merits of such subjects.

The deep interest manifested of late in these footmarks on both sides of the Atlantic, has led to a more minute inquiry as to their original discovery, than was made during the period when more obloquy than honor was connected with the subject. Several individuals have complained to me that I have not, in my printed accounts of the footmarks, mentioned their names, as having discovered them earlier than any whom I have mentioned. In the newspapers of this country, also in several articles of the American Journal of Science,* in the London, Ed-

* Vol. XLIII, p. 241, and Vol. XLV, pp. 179 and 185.

inburgh and Dublin Philosophical Magazine,* in Mr. Murchison's Anniversary Address for 1843, before the London Geological Society,† and in the Proceedings of that Society, which are copied into most of the scientific journals of Europe, representations have been made which I feel bound to notice, because they convey wrong impressions, and, unintentionally I doubt not, do me injustice, and moreover imply injustice on my part towards others. It is there stated, that Dr. James Deane not only originally discovered but explored these footmarks; "expressing then his own belief, from what he saw in existing nature, that the footmarks were made by birds," and that when he communicated the discovery to Professor Silliman and myself, both of us "admitted the plausibility of his 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."

Now such statements, made in almost every instance by those whom, up to this hour, I have the honor to regard as my friends, certainly convey the impression, that when I commenced the study of footmarks, the subject had been so far investigated by Dr. Deane, that he was able, on scientific grounds, to form the grand conclusion that they were the tracks of birds; nay, that my scepticism was overcome by his efforts. If all this be true, then I have not given him the credit which he deserves, in my accounts of the footmarks. In my first paper on the subject, I say, that "my attention was first called to the subject by Dr. James Deane of Greenfield, who sent me some casts of impressions on a red micaceous sandstone, brought from the south part of Montague for flagging stones. Through the liberality of the same gentleman, I soon after obtained the specimens themselves, (which I then describe as containing impressions,) precisely resembling the impressions of the feet of birds. Indeed, among the hundreds who have examined these specimens, probably no one doubts that such was their origin." In my Report on the Geology of Massachusetts I added nothing more, except to attach Dr. Deane's name to a beautiful species.

In order to do justice to all concerned, I feel myself compelled to give a brief statement of the facts connected with the discovery

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* Vol. XXIII, p. 186. † P. 107.

and investigation of these footmarks. I do this with great reluctance, not only because it is difficult to speak of one's own labors unexceptionably, but especially because I shall be brought into some apparent collision with Dr. Deane, between whom and myself there has existed to this time an uninterrupted friendship. But really I do not see how I can do justice to myself or to others, without detailing the facts; and I cannot believe that any reasonable man will complain, if the facts are carefully stated.

About the year 1802, (possibly a year earlier or later,) Mr. Pliny Moody of South Hadley, in Massachusetts, then a boy, turned up with a plough upon his father's farm in that place, a stone, containing in relief five tracks of the *Ornithoidichnites fulicoides*, (see Plate 48, fig. 55 of my Final Report;) and it was put down as a door-step, because it contained tracks, and the neighbors used facetiously to remark to Mr. Moody, that he must have heavy poultry that could make such tracks on stone. After Mr. Moody (junior) had left home for school or college, Dr. Elihu Dwight of South Hadley purchased this stone, because it contained these tracks. It was retained by him nearly thirty years, when I purchased it for my cabinet, I think in 1839. Dr. Dwight used pleasantly to remark to his visitors, that *these were probably the tracks of Noah's raven.*

In 1834, some gentlemen in Greenfield united their contributions to obtain flagging-stones for one of their streets. Mr. William Wilson was the agent who procured them; and when they were brought from the quarry in Montague, in the spring of 1835, he noticed very distinct tracks upon them, which he referred to "*the turkey tribe*," though destitute of the hind toe. He pointed them out to several of his neighbors, among whom was Dr. Deane, as he thinks; though he does not claim having suggested to him that they were made by "the turkey tribe." That idea was doubtless original with both, as it seems to have been with others in different parts of the valley, and originated in a sort of resemblance—not very close indeed—between these tracks and the foot of a turkey; although of course no geologist would entertain such an opinion, since he knows that our present animals did not exist in the red sandstone days. It was about the same time that Dr. Deane called my attention to the subject in the following letter, dated March 7, 1835.

"In the slabs of sandstone from Connecticut river in Montague or Sunderland, lately brought here, I have obtained singular appearances, new to me, although I presume not to yourself. One of them is distinctly marked with the tracks of a turkey (as I believe) in relief. There were two of the birds side by side, making strides of about two feet."

"I was anxious to see the die from which these impressions were struck, and it has now arrived. The tracks, four in number, are perfect, and must have been made when the materials were in a plastic state, and at what period I leave you to tell. I am no geologist, but yet know that geologists derive much satisfaction from contemplating these remains. I do not know but they may be familiar to you; but if you desire it, I will endeavor to prevent their being converted to the use for which they were brought here."

My answer, bearing date March 15th, was as follows:

"It would be a most interesting fact, if the suggestions you make as to the impressions on sandstone, should prove true. For I recollect but a single similar fact in geology, and that is the tracks of a tortoise on the sandstone of Scotland, described in the American Journal of Science a few years ago. I am not without strong suspicion, however, that the case you mention may be a very peculiar structure of certain spots in the sandstone, which I have often seen in a red variety of that rock. The layers of rock having this structure, sometimes present an appearance resembling the foot of a bird. But I am satisfied that it is not the result of organization, though I confess myself unable to say precisely from what principle it has resulted. But perhaps the case you mention is not of this sort, and I should be quite glad to see the specimens; if you can prevent their being defaced for a month or two, until I shall visit Greenfield, I shall be much obliged to you."

The peculiar structure of the sandstone in some places, referred to in the above letter, still remains to me inexplicable. A specimen of it may be seen in the Massachusetts State Collection, No. 1793. This letter was written before Dr. Deane had sent me a cast of the tracks. And hence my scepticism, since I had repeatedly known a peculiarity of structure in rocks to be mistaken for tracks.

On the 20th of March, Dr. Deane replied to my letters, repeating his belief that these marks were "the real impressions of the feet of some bird, probably of the turkey species;" and stated that the tracks were in a row, and that the layers of rock were bent downward beneath them. In support of this opinion, soon after he sent me a cast of the tracks, with a reiteration of the same views. Ere long I visited Greenfield, and as soon as I saw the impressions, I perceived that an interesting field of research was opened before me. They certainly appeared to be tracks; and bore a striking resemblance to those of large Grallæ, or wading birds. But I knew that such opinions were opposed by very strong geological analogies; and I suspended my judgment until I could investigate the matter. It would not answer to rest so important conclusions upon a single specimen, however distinct: for I had too often found that first impressions in geology were fallacious. Dr. Deane had, indeed, expressed his opinion that they were bird tracks. But as he had declared himself in his letter unacquainted with geology, and had even referred the tracks to birds similar to those now living, showing thus that he did not appreciate the strong geological objections to his opinions, and since also his reasons for his opinions were only such as a casual inspection of the specimens would force upon every one, viz. the indentations made by the tracks, their general resemblance to the feet of birds, and their existence in a row—facts which I afterwards found produced the same convictions upon almost all who saw them—I confess, without meaning to detract at all from the high respect I entertain for Dr. Deane, that his opinion made no impression upon me. I took hold of this question, therefore, not as one already settled, but as one requiring the most careful examination to decide. I visited every accessible sandstone quarry, and soon brought to light several other species of tracks; among which were those enormous ones, which I have called *giganteus* and *ingens*—the former four times larger than the track of an ostrich. I also sought for the tracks of living animals upon mud and snow, and for their feet in menageries and museums. And this work was commenced *alone*, and for years has been continued *alone*. Who, indeed, could give me any instruction in the science of tracks? In what volume is it contained? In the volume of nature only; and there have I been obliged to eke it out as well as I could. A large part of several years has been devoted to this work. If in any thing I can lay claim to originality and original discovery, it is here. Dr. Deane, Dr. Barratt, Col. John Wilson, Col. David Bryant, N. P. Ames, Esq. and Prof. Henry Hanmer, have, indeed, very kindly sent me interesting specimens, several of them new; and Dr. Barratt gave the name to one species; but the rest have all been described by me, and most of them discovered by me, in no less than sixteen quarries scattered through the valley of the Connecticut.

Yet the labor of these investigations has not been the most trying part of the work. I had advanced opinions that seemed to most geologists improbable and extravagant; and the same incredulity could not but be extended to all my scientific efforts. He only who has been obliged to sustain an unpopular cause for years, and has felt the misgivings and heart-sickness of such a state, can appreciate my sufferings from this cause, during the long conflict. It cannot, therefore, be thought strange, that I should manifest a lively sensibility to any statements that seem to me to detract from my just claims on this subject.*

It seems to me from this full view of the case, that I may regard the following positions as established.

1. If to find the footmarks, and to form and express the opinion that they were made by birds, constitute their original discovery, then Mr. Moody and Dr. Dwight of South Hadley can fairly lay claim to it earlier than any others.

2. If to prove by long and laborious investigations, what is the true nature of these impressions, may properly be regarded as their discovery, in the sense in which that term is understood by scientific men, then I may lay claim to it—since the only help which I have received in these researches has been in the communication of specimens. In a popular sense, indeed, he who first finds a specimen in natural history, may be called the original discoverer; and in this sense I have always spoken of Dr. Deane, Mr. Moody, and Mr. Wilson, as original discoverers.

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* Prof. Silliman from the very first decidedly sustained my views, and they were fully adopted afterwards by Prof. Buckland in his Bridgewater Treatise. Had I not received the support of these gentlemen, it seems to me that I must have given up the contest in despair.

But up to the time of the publication of my Final Report, which contains the fullest account of these footmarks, I am sure that no other person but myself had attempted to examine them as a matter of science.

3. While we must admit that Mr. Moody, Dr. Dwight, and Mr. Wilson, were original discoverers of the footmarks, much higher credit is due to Dr. Deane. He did not content himself with speaking of them as objects of curiosity, but took measures to bring them under the notice of those whose professional business it was to examine such objects, and even took casts of them. Nor did his interest in them ever diminish; and though he modestly styled himself "no geologist," yet such a description would by no means apply to him at a subsequent date; and the Transactions of this Association, as well as the pages of the American Journal of Science, show, that within a few years past, he has actively explored and described several interesting cases of footmarks. And furthermore, I entirely acquit him, and indeed all others, of doing me any intentional injustice in this matter—as I trust they will acquit me of a desire to claim more than is my due. At any rate, the facts, as I understand them, are now before geologists, and to their decision I hope cheerfully to submit.

From this long and unpleasant digression, I return to the history of Ichnolithology, since the discovery of footmarks in Massachusetts and Connecticut.

I ought to remark, that although the account of the footmarks of this country was not published as early as that of the Saxon Chirotheria, yet no account of the latter had reached this country till my paper was in press. Their almost simultaneous discovery on both continents turned the attention of geologists to the subject, and, as was to be expected, many new cases have since been brought to light.

In the summer of 1838, tracks of Chirotheria, tortoises, and Saurian reptiles, were discovered in the new red sandstone of Storeton Hill, near Liverpool, in England. Another species of Chirotherium was found near Tarpoly, and in all, five or six species of smaller reptiles occur in the English quarries. In 1840, tracks of "amphibious quadrupeds, probably allied to crocodiles, monitors, or other Saurians," were found in a quarry in the city of Liverpool.* In 1839, Dr. O. Ward described foot prints and rain drops on the new red sandstone of Grinshill Hill, in Shropshire. These have only three toes armed with claws, and seem to correspond to those in the valley of Connecticut River.† In 1842, Mr. Hawkshaw described tracks in the same rock at Lymm, in Cheshire. They were those of the Chirotherium, of Crustaceans, and others "resembling the feet of birds." Some of them show the impressions of the papillæ of the feet of the animal—a fact noticed in my Final Report as occurring in one specimen of the Ornithoidichnites from Wethersfield in this country.‡

In Professors Leonhard and Bronn's Journal of Mineralogy, Geology, &c. for 1839, Dr. Cotta has described some singular footmarks in new red sandstone, some twenty or thirty miles from Leipsic, in Saxony. They are two toed, or rather somewhat like a horse-shoe. He did not find them in succession, and yet seems quite confident that they are tracks. He found them only in relief, and I cannot but have a suspicion that they are concretions, as I have met with some of this kind, which, when considerably weathered, bore a strong resemblance to a horse-shoe. It is hazardous, however, on such a subject, to risk an opinion without having seen a specimen. And they seem, moreover, to be recognized as tracks by M. Feldman, who has lately described a smaller species of the same kind near Jena, in connection with tracks of Chirotheria and "numerous tridigitated imprints, disposed parallel to one another."§

In Dr. Buckland's anniversary address before the London Geological Society, for 1841, we find an account of the tracks of deer and large oxen upon mud, beneath a bed of peat in Pembray, in Pembrokeshire, and to the east of Neath. This fact supplies an important link in the evidence by which the reality of fossil footmarks is proved. For here we are certain that tracks have been preserved upon unconsolidated mud for centuries, and we know that this mud needs only to be hardened to become rock with footmarks.

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* Rep. of British Association for 1840, p. 99.
† Ibid. for 1839, p. 75. ‡ Ibid. for 1842, p. 57.
§ Geologist for January, 1843, p. 18.

Before the London Geological Society in November, 1842, Mr. H. E. Strickland gave an account of certain impressions upon the lias bone bed in Gloucestershire, which, in his opinion, were made by fish, or invertebrate animals. The straight grooves and small pits, he thinks, may have been formed by fish, striking against the bottom, or probing the mud for food with its nose. The curved grooves he refers to an acephalous mollusk, the *Pullustra arenicola*, and certain tortuous tracks to annelidous worms.*

In June, 1843, Dr. Buckland gave an account before the London Geological Society of certain "*Ichthyopodolites*, or petrified trackways of ambulatory fishes upon sandstone of the coal formation," in Flintshire, England. They consist of curvilinear scratches disposed symmetrically at regular intervals on each side of a level space about two inches wide, which in his opinion may represent the body of a fish, to the pectoral rays of which animal he attributes the scratches. They follow one another in nearly equidistant rows of three scratches in a row, and at intervals of about two inches from the point of each individual scratch to the points of those next succeeding and preceding it.†

In this country two new localities of tracks have been discovered of late, which are of no small interest. In rocks of the carboniferous series in Nova Scotia, Mr. Logan has found tracks of a reptile of unknown species. This is the first example, I believe, of tracks below the new red sandstone; and, indeed, I am not aware that we have had any previous evidence of the existence of reptiles as low as the carboniferous group.‡

Our associate, W. C. Redfield, Esq., has during the last year discovered the *Ornithoidichnites tuberosus* in the new red sandstone of New Jersey, in connection with fossil fish. The specimen which he showed me is of the most decided character, and inspires the hope that other developments in regard to tracks may be expected from the red sandstone series extending through New Jersey, Pennsylvania, Maryland, Virginia, and North Carolina.§

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* Lond. Ed. and Dub. Phil. Mag. for Jan. 1844, Supplement, p. 531.

† Philos. Mag. for March, 1844, p. 230.

‡ Am. Journal of Science, Vol. XLV, p. 358.

§ Ibid. Vol. XLV, p. 134.

Another of our associates, Dr. David Dale Owen, has recently shown most conclusively, that the supposed human footsteps on the limestone of the Mississippi valley, are of artificial origin. This was a demonstration greatly needed; since some distinguished transatlantic geologists were inclined to regard them as the veritable footsteps of men, coeval with the rock that contains them, although I am not aware that that opinion was ever adopted in this country.

Having thus brought up the history of ichnolithology to the present time, so far as I know it, I proceed to detail some new facts that have fallen under my notice since the last meeting of this Association, in relation to the footmarks of this country. And first, I shall describe a few new species that have been found on the sandstone of Connecticut River.

The first is a large species of Ornithoidichnites, occurring upon the hard impure limestone of Chicopee Falls in Springfield, along with the *Sauroidichnites polemarchius*, *minitans*, and others. It will be seen in the sequel, that some interesting facts are associated with this track. I dedicate it to my friend, William C. Redfield, Esq., the successful investigator of our fossil ichthyology, and ingenious expounder of the world's meteorology.

DESCRIPTION.

Ornithoidichnites Redfieldii. Toes three, all pointing forward, spreading 70°; leptodactylous, yet having claws from an inch to an inch and a half long; length of the middle toe and of the foot, thirteen and a half inches; length of the step, thirty inches. Shown (of one fourth the natural size) on fig. 1.*

Although I have seen several tracks of this species, I have found only one example of them in succession, and therefore the length of the step may not be given very accurately above.

This is the first example in which I have found claws upon a narrow-toed or leptodactylous track. And in this case the claws do not appear upon the surface where the animal trod; but upon splitting off some of the rock beneath, through the layers that were depressed by its weight, the claws are obvious. Copies of the extremities of two of the toes of one track, with the claws,

* All of the drawings on the Plate accompanying this article are reduced to one fourth of the natural size.

are given on figs. 2 and 3, where the rock was split through. We must not judge from these figures that they show the actual width of the animal's toes, for the curvature of the layers of stone extends usually much farther laterally than the width of the toe; yet the tapering at the end shows that the toes had claws.

In the American Journal of Science for January, 1844, Vol. XLVI, Dr. Deane has given a description, with drawings, of some very interesting slabs of tracks, which he discovered at Turner's Falls. They were remarkably distinct and very numerous; and yet it was easy to trace the consecutive tracks, so as to show beyond question that they were all made by bipeds. Among them the most common was the *Ornithoidichnites fulicoides*, of which I gave an account two years ago to this Association, when I expressed some slight doubts whether it were made by a biped or a quadruped. This slab shows most unequivocally that it was by the former. So that, up to the present time, we have no certain evidence that more than one species of the tracks in the Connecticut valley (viz. the *Batrachoidichnites Deweyi*,) were made by quadrupeds.

Among the tracks figured by Dr. Deane, were two varieties resembling the *O. fulicoides*, but smaller; and he leaves it to me to decide whether they are distinct species. The variety of medium size he represents as exhibiting a stride almost twice as great as that of the *fulicoides*; and I have little doubt, from what I have seen of the tracks of living and extinct animals, that they must be specifically distinct. But, as I have no specimen, and the slab has been disposed of to the British Museum, I dare not attempt to describe it from casts. Of the smallest variety I have a specimen, and think it unquestionably distinct. It is a much more slender and delicate species than the *fulicoides*; the toes spread less by 20°; and although both of them are pachydactylous, I cannot discover in the small track any evidence of a membranous margin to the claw, which has led me to arrange the former under the *pterodactyli.* The new species I describe as follows.

Ornithoidichnites gracillimus. Toes three, all in front, spreading 60°: pachydactylous: claws and tuberous swellings distinct; impression of a double headed extremity of the tarso-metatarsal bone more or less obvious: length of the foot two and a half inches: length of the step six inches. Shown, with impressions of the heel, on fig. 4.

On several other tracks, at the locality from which this slab was taken, the marks of the heel, or extremity of the tarso-metatarsal bone, are obvious; and I am satisfied that the *Ornithoidichnites cuneatus*, named by Dr. Barratt, and given in my Final Report, is only the *O. fulicoides*, or *Sillimanii*, with this impression, that gives it the wedge-shape characteristic of the species; although I have never seen a specimen of the *O. cuneatus* distinct enough to show the phalangeal impressions. I think, therefore, that we must erase the *cuneatus* from the catalogue of species.

To the next species I attach the name of my friend Dr. Samuel L. Dana, too well known, by his various scientific labors, to need my encomium, but to whom, as will appear in the sequel, I am deeply indebted for his labors in relation to Ichnolithology.

Ornithoidichnites Danæ. Toes four; three in front, spreading 95°: leptodactylous: fourth or hind toe projecting opposite the external toe; short, and making but a faint impression: heel large, and making a deep impression: whole track thick, and apparently made by a heavy animal. Length of the foot, ten inches. Shown on fig. 5.

This track was found on gray micaceous sandstone, at a new locality, pointed out to me by Dr. Wright of Montague, about half a mile east of the bridge over Connecticut River into Greenfield, and on the Boston road. The same slab contains the *Ornithoidichnites elegans.* Only a small surface of rock is exposed, and I could not get sight of the next track; so that the length of the step cannot be given. And had it not been quite peculiar, I should not found a species upon a single specimen.

I am in some doubt whether to refer this species to the division *Ornithoidichnites* or *Sauroidichnites;* since it is not quite certain that a fourth toe exists, the impression being rather faint. But if that toe projects opposite the external toe, it approaches the *Ornithoidichnites tetradactylus*, though much larger, and with an enormous heel. But if the hind toe were a little farther back, and projected more nearly at right angles to the middle toe, it would ally the track to the *Sauroidichnites minitans.* And this leads me to remark, that I fear the terms Ornithoidichnites and Sauroidichnites convey an impression of a wider distinction than exists between these two subdivisions. For in fact they pass insensibly into each other, and, with a few exceptions, all probably were made by the same tribe of animals.

The next new species I have to propose, is a very distinct, though very anomalous one. I call it a Sauroidichnites, because it has somewhat the aspect of the foot of a Saurian; and yet it has but three toes, whereas a Saurian has always four, and generally five.

Sauroidichnites abnormis. Toes three, all directed forward: the lateral ones diverging about 30°, and connected by a base two inches long, the base and the inner toe appearing like a single bent toe: middle toe with a deep impression along its anterior part, but scarcely distinguishable on its posterior part. Heel extending backwards nearly an inch, on a line with the outer toe. Length of the middle toe, nearly three inches; of the foot, four inches; and of the step, eighteen inches. Fig. 6 shows the right foot, and fig. 7 the left foot.

This remarkable track was found by Dr. Deane at the same place as the specimens already referred to, viz. a little above Turner's Falls in Gill. It is very distinctly impressed upon a gray, perfectly smooth, micaceous sandstone; and fortunately there are several tracks showing the right and left foot most distinctly. At first, I thought it was probably a perfect example of the *Sauroidichnites tenuissimus*, described in my Final Report, of which I possess only imperfect specimens. But it is impossible to make them out identical.

Anomalous as is this track, there is a fact still more anomalous, in the very distinct specimens before me. In two instances, and these are all that I possess, while the middle toe of the left foot lies in the direction in which the animal moved, that of the right foot is turned nearly 45° towards the left hand, as is shown on fig. 8, which was copied by a pentagraph from the specimens. Had this singular inflection existed in both feet, I should have supposed it a law of the species. But nature is not so partial as to bestow a peculiarity upon one organ, and withhold it from the twin organ. And I suspect that the animal's right foot might have been injured, so as to give it an inflected position!

On the shale of Turner's Falls, Dr. Deane has recently found a specimen of what I have called, in the first volume of the Transactions of this Association, the *Sauroidichnites Deweyi*, (p. 261,) and it confirms still farther the opinion there expressed, "that this is the track of a quadruped." Fig. 10 shows the tracks as they appear on this specimen, and they very probably were made by a small *Batrachian;* so that, upon a review of the whole subject, I think this track ought to come under the *Tetrapodichnites*, and be denominated *Batrachoidichnites Deweyi*. I am further satisfied that the *Ornithoidichnites parvulus* of my Report is the same as the *B. Deweyi*, and I shall accordingly strike out the first named species.

COPROLITES OF BIRDS.

I now proceed to the most interesting discovery which has been made during the past year, in relation to the fossil footmarks of this country. I am able to state with great confidence, that the coprolites of birds have been found in connection with these tracks; and although the details of the subject are somewhat prolix, yet the curious results to which they lead, and the fact that no coprolites of birds have hitherto been found, will be my apology for giving them in full.*

These coprolites were found in connection with the *Ornithoidichnites Redfieldii*, in hard calcareous rock, at Chicopee Falls, in Springfield. The spot where they were found seems to have been a resort for the bird that formed this track; for the tracks interfere with one another, and occur in successive layers. In the midst of them I found a few ovoid flattened bodies, about an inch in diameter, and perhaps two inches long, of a dark color, and considerably softer than the enclosing rock, which is very hard and compact. When broken crosswise, they usually exhibit a more or less perfect concentric arrangement, or sometimes perhaps a little convoluted, as shown on figs. 9 and 10, which were drawn from specimens a little broken on one side. They adhere so strongly to the rock, that I have not been able to determine precisely their external appearance.

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* The *results* of Dr. Dana's chemical examination of the fecal relics will be given in connection with the present paper of Prof. Hitchcock; but we are reluctantly compelled, for want of room, to postpone until our next No. the full details of Dr. Dana's analysis, which will appear in Jan. 1845, as a distinct article.—Eds.

If this coprolite be examined with a glass, small black grains may be seen in some parts, which resemble small seeds, and which one cannot but strongly suspect to be seeds, that passed undigested through the animal. I have not been able to determine this point certainly, from having so small a quantity of the substance. But it strongly confirms the above suggestion, that the same conclusion was made by Dr. Samuel L. Dana, to whom I sent a small fragment for another purpose, as will appear in the sequel, though I said nothing to him of the seeds. I quote his remarks on the subject. "I want to say a word about the *black* grains, &c., in the coprolite. In the unbroken bit, about as big as a hazle nut, I think I can discover an evident tendency to convolution; so that these black masses would, if the thing could be unrolled, lie for the most part in the same plane, though in the interior of the bit the black grains are more promiscuous. I call these *grains;* for, if you examine them, they nearly all approach the form of an apple seed. They may be raised out of the little shell of carbonate of lime, sometimes crystalline, which surrounds each. Others have the form of stems. The black matter of these grains is carbonaceous. They consist, when that is burned off, of phosphate and carbonate of lime. Now allow me a word of speculation. I cannot but think these black grains are *seeds,* which have passed undigested through the intestines, and have assumed, in the passage, such position as these foreign bodies would, and often do, in the feces."

The external characters of these nodules corresponding so nearly to those of coprolites, I felt a strong desire to have them subjected to a most thorough and careful analysis. Some years ago, when I suspected that I had found some coprolites with the footmarks, Dr. Dana suggested to me, that, if dropped by birds, they might contain uric acid. This thought seemed to me worthy of being pursued; and since I had the highest opinion of Dr. Dana's analytical skill, I requested him to undertake the examination of this substance, with the suggestion respecting uric acid in mind. He consented, and after bestowing a great deal of time and labor upon the subject, he has presented me with some of the most unlooked for and beautiful results that I have ever seen derived from chemical analysis. Dr. Dana's earlier results gave—

Water, organic matter, urate and volatile salts of ammonia,	10.30
Chloride of sodium,	.51
Sulphates of lime and magnesia,	1.75
Phosphate of lime and magnesia,	39.60
Carbonate of lime,	34.77
Silicates,	13.07
	100.00

In subsequent experiments he verified the existence of uric acid, as well as of muriate of soda and of ammonia.

For many very curious and interesting comparisons and analogies, reference must be had to the paper in full, to appear in the ensuing No. of this Journal.

This appears to me to be a most beautiful example of the application of chemistry to geology. A few ounces of a dark colored substance are dug out of a quarry, in the sandstone of Chicopee river, and put into the hands of the chemist. Bringing to bear upon it the searching power of analysis, he is conducted to the very remarkable conclusion, that it is the excrement of an animal dropped perhaps hundreds of thousands of years ago. But the clue line leads him still farther. Detecting about half a per cent. of a peculiar acid—the uric, in the coprolite, he is led to infer, by fair reasoning, that it is the coprolite of birds, rather than of any other animal. Nay, he goes still farther, and shows that it must have been derived from a particular kind of birds, viz. the omnivorous. Truly this may be called a scientific miracle—a resurrection from the dead, and among the many analogous miracles wrought in the nineteenth century I know of scarcely any more marvellous than this!

I might add here, that if we are not mistaken in supposing the coprolite to contain seeds, the fact lends confirmation to the conclusion of Dr. Dana, that it was dropped by an omnivorous, instead of a carnivorous bird. Had it been mostly composed of comminuted animal matter, as some coprolites are, we should have found some discrepancy between the fact and the results of analysis. But now the mechanical composition harmonizes with the chemical; and I may add, both correspond with the conclusion deduced from the history of the footmarks.

The progress of light and evidence in respect to these footmarks it is interesting to trace. When first discovered, so striking was their resemblance to the tracks of living birds, that every one not familiar with geology, who had ever seen their tracks upon snow, or mud, at once pronounced the fossil footmarks to have had the same origin. A careful examination, both of the fossil and living footmarks, forced me to the conclusion that the first and most obvious impression regarding them was sustained by fair scientific analogies. But there were two objections to these views, that yet remained unanswered; and which prevented several of the ablest geologists and comparative anatomists of Europe from falling in with them. The first was, that the tracks were too large to have been made by a bird. The second was, that animals of so high an organization as birds could not have existed so early as the new red sandstone period. The discovery of the Dinornis, and examination of the anatomical structure of the Apteryx, and other struthious birds of southeastern Asia, have unexpectedly removed both these difficulties. In regard to them, says Prof. Owen, "the metatarsal bone of the *Dinornis Novæ Zealandiæ* 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. 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 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 sandstone of Connecticut and Massachusetts."* To all this we can now add the evidence of the coprolites; and I see not what more is wanting, except the bones, to complete the argument. Nor am I by any means certain but that we already have these,—the property of Prof. Silliman, and figured in my Final Report. It would not be strange, if these fragments should pass under the eye of Richard Owen,—the man on whom so deservedly the mantle of Cuvier rests, and who was able to construct the Dinornis from a single fragment of the shaft of a bone,—I say, it would not be strange, if out of these fragments he should be able to place before us some Dinornis of sandstone days.†

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* American Journal of Science, Vol. XLV, p. 186.

† July, 1844.—In the Lond. Ed. and Dub. Phil. Magazine for May of this year, we have an abstract of Prof. Owen's last paper on the Dinornis, read before the Zoological Society of London last November, founded on a second box of bones

While on this subject, may I be allowed to delay long enough to state one or two very curious facts, that have lately fallen under my notice, as related by Capts. Cook and Flinders. They relate to some nests of birds discovered by these voyagers on the coast of New Holland, of enormous size. I have long been in the habit in my lectures, of reading these accounts as a part of the poetry of footmarks, in connection with others manifestly fabulous. But since the history of the Dinornis has appeared, the question has arisen in my mind, whether the statements of these navigators must not be true. The nest seen by Capt. Cook, was on a small island in about 14° south latitude, on the northeast coast of New Holland. In his visit to the island he was accompanied by Sir Joseph Banks. "At two in the afternoon," says he, "there being no hopes of clear weather, we set out from Lizard Island to return to the ship, and in our way landed upon the low sandy island with trees upon it, which we had remarked in our going out. Upon this island we saw an incredible number of birds, chiefly sea fowl. We found also the nest of an eagle with young ones, which we killed; and the nest of some other bird, we knew not what, of a most enormous size; it was built with sticks upon the ground, and was no less than six and twenty feet in circumference and two feet eight inches high."—"To this spot we gave the name of Eagle Island," &c.*

Similar nests were found by Capt. Flinders in King George's Bay, on the southeast coast of New Holland, in about 35° south

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received from the missionaries. They have enabled him to establish five distinct species; the largest of which, ten feet high, he calls *Dinornis giganteus*. The smallest was four feet high, called the *D. didiformis*. "These data," says the abstract, "showed that the trifid foot-print of the *D. giganteus* must have exceeded in size the *Ornithichnites giganteus* and *O. ingens* of Prof. Hitchcock, and that the *Dinornis didiformis* must have left impressions as large as those called *Ornithichnites tuberosus*. The author warned his hearers against inferring identity of species, or even genus, between the extinct *Struthionidæ* of the alluvium of New Zealand, and those of the Trias of North America, on account of correspondence of size and number of toes, which the modern genera Casuarius, Rhea, &c. proved to be insufficient grounds." It seems then, that Mr. Owen regards the footmarks of this country as clearly referrible to the family Struthionidæ; and this probably is as specific an account of the authors of the footmarks as will ever be attained. Who could have imagined that light, on such a subject, could have come from New Zealand; and that too, as a fruit of missionary labor! Truly there is a web of harmony uniting all the parts of this world's history.

* Cook's first voyage in Kerr's Collection of Voyages and Travels, Vol. XIII, p. 318.

latitude. I quote his account from the 23d number of the Quarterly Review, p. 27.

"They were built upon the ground, from which they rose above two feet, and were of vast circumference and great interior capacity; the trunks of trees and other matter of which each nest was composed being enough to fill a cart."

Now I see no more reason for doubting this than any other fact related by these voyagers. I take it we may regard them as true, and no exaggeration. Equally certain is it, that we know of no other bird, except the Dinornis, that could have required so enormous a nest. But for that bird it would not be larger than would be necessary and convenient, as any one may see by drawing a nest of that size and an apterous bird ten feet high by the side of it. It was built too upon the ground, where an apterous bird would build. Is it not probable therefore, that this was the nest of the Dinornis; and if so, that this bird still survives in New Holland, if not in New Zealand? The north island of New Zealand is some 5° farther south than King George's Bay, and nearly 30° farther south than Eagle Island. In the warmer climate of New Holland therefore, this bird may be yet alive, although extinct in New Zealand. But I understand that there is no decisive proof that it does not still live in New Zealand. Mr. Owen does indeed express the opinion that it has been extinct perhaps two centuries. Yet some English sailors declare that they have seen it, and the missionaries do not attempt to decide the point. Capt. Cook's voyage was performed about the year 1770, and Capt. Flinders' in 1801. I do not yet despair therefore, of having the zoological cabinets favored with something more than the bones of the Dinornis; and possibly the menageries may stand some chance of getting this bird alive. If he be indeed still alive, we may expect, as Mr. Williams, the missionary who sent the bones to Dr. Buckland, facetiously remarks, that there will be a crusade got up among the naturalists to go and take him. When he reaches this country, I shall propose that he be taken to the banks of the Connecticut, to see if he can follow the footsteps of his great progenitor of sandstone fame.

The impressions of rain-drops, connected with the footmarks, deserve notice because they have so important a bearing upon our reasoning as to the circumstances under which the tracks were formed and preserved. But the whole history of the rain-drops deserves a separate communication, and a more careful collection of specimens than has yet been made. Suffice it to say in this place, that these impressions are quite common at many of the localities of footmarks, though not in them all. Large slabs can sometimes be obtained beautifully filled.

Some facts connected with the footmarks of the Connecticut valley, throw light upon the question whether the sandstone on which they are found has been tilted up since its deposition. But this subject will be more conveniently and pertinently introduced in another paper, which I propose to present to the Association.

Supposed Footmark on the Slate of Hudson River.

I now advance to a part of my subject which will probably be received with more hesitation than the positions already presented. In the year 1837, I suggested, in the American Journal of Science, that I had found some marks on the flagging stones of the city of New York, which might have been made by a *didactylous* quadruped. In company with W. C. Redfield, Esq. I visited very many of the streets of New York and Brooklyn, where the rock containing the supposed tracks is extensively used in the sidewalks; but the impressions were not found to be numerous, though occasionally to be met with. I obtained liberty to remove the best slab I could find, which was twenty eight by forty four inches, and it is now in my cabinet. However, upon re-examination I became sceptical in regard to my first views of the impressions, and I feared, also, that by bringing forward what appeared to myself a doubtful case, I should render the community still more incredulous in regard to the Ornithichnites; so that I judged it best to say no more about the New York impressions. Still the idea has always haunted me that they must be the result of organic agency; and a re-examination of them recently has so satisfied me on this point, that I venture, with no little diffidence, to bring the case before the Association.

The rock that contains these impressions is quarried in immense quantities for flagging stones in the counties of Ulster, Greene, and Albany. It belongs to the *Erie division* of rocks, as they are called in the New York geological survey, and as

Mr. Redfield informs me, to the *Hamilton group*. It contains one peculiar fossil in considerable abundance, exactly resembling an annelidous worm, but whose nature has not been ascertained, although it is often very distinct. This group of rocks also contains plants, according to Prof. Mather, who has described the rock in his report. It is a hard, gray, rather thick bedded sandstone, of a very enduring character.

Fig. 9, is an exact reduced copy, with the pentagraph, of the slab in my possession. The impressions upon it appear to have been made with a blunt object, nearly of the size of a man's finger, and as much rounded. I regard these marks as resulting from the agency of animals for the following reasons.

1. The impressions are for the most part arranged in nearly parallel rows; the axis of the impressions lying nearly crosswise to the direction of the rows. Thus on fig. 9, we can trace the rows if I mistake not, A B, C D, E F, G H, L M, N O, P R, S T; although some of the impressions are a little out of line.

2. The impressions, like the Ornithoidichnites, appear to have been produced by some body pressing on a surface of mud, rather than by a body interposed between two layers of mud.

3. There is such a general resemblance between the impressions, as to prove them to have originated from the same general cause. And yet they are of different sizes in the different rows, but uniform for the most part in the same row.

4. A large part of them are in pairs; one of each pair being considerably shorter than the other, and the axes of the two impressions diverging pretty uniformly about 40°. On several parts of the slab they are but imperfectly preserved. Had they all been retained, I apprehend that they would be found universally in pairs, since those most distinct are so.

5. I know of no other agency but the feet of animals, to which these impressions can be referred. They belong to no variety of ripple marks, nor to the mud furrows and wave lines of Mr. Hall, nor could they have resulted from lateral pressure, or the deposition of vegetable or animal remains. But they exhibit a general resemblance to the tracks of animals.

But what class of animals could have produced such tracks? Most probably an animal with didactylous feet in which one of the toes is longer than the other. It had occurred to me that they might possibly have been made by Crustaceans. But I am informed especially by Mr. James D. Dana, that these animals never advance in such a manner as to produce impressions resembling those on the rock. It would seem as if these must have been made by an animal extending its didactylous feet almost at right angles to its body, so as to make a row of tracks on each side. For we shall find that several of the rows of tracks on fig. 9 correspond to each other. Thus the row A B corresponds to E F; C D to L M, and G H nearly to N O. There are indeed a few tracks upon this slab which could not be brought into such an arrangement, but they may belong to other rows partially obliterated. Yet I have so little confidence in any suggestions I can make as to the tribe of animals by which these impressions were made, that I shall describe them without a name, presuming however that it must have been some animal that crawled along the bottom of the ocean.

Description.—Rows of tracks two; parallel; about a foot apart. Feet didactylous; toes diverging about 40°; unequal in length, blunt; length from two to three and a half inches; lying nearly at right angles to the direction in which the animal moved.

I ought not to omit to mention, that in many points there is a striking resemblance between the impressions just described, and the Ichthyopodolites described by Dr. Buckland, and noticed in another part of this paper. The bluntness of the impressions in New York seems, moreover, to be a strong objection to their having been made by the fins of any such fish as now inhabits the ocean.

I have been struck, also, with a paper read by Mr. Pearce before the London Geological Society, in March, 1843, on the locomotive and non-locomotive Crinoidea. The foot in the former class is sometimes bifurcated and terminated in a minute blunt point. It is possible that here we may have the origin of the marks under consideration.*

CLASSIFICATION OF FOOTMARKS.

Having now given all the facts concerning footmarks with which I am acquainted, I offer the following systematic arrange-

* Phil. Mag. for January, 1844, p. 58.


B
A
C

ICHNOLITES.

ment of the species. I include them all in the class *Ichnolites*, or stony tracks; and this class is subdivided into four orders, founded on the number of feet in the animal that made them.

Class Ichnolites.

I. Order Polypodichnites, or *many-footed tracks.*

1. On the forest marble near Bath in England.
2. On the slate of Hudson River.

II. Order Tetrapodichnites, or *four-footed tracks.*

1. Several species made by Chirotheria, or the Labyrinthodon most probably, in Germany and England.
2. By Saurians in England.
3. By Tortoises in Scotland and Germany.
4. Other Batrachians besides the Labyrinthodon in Germany.

(Nos. 2–4: 10 or 12 species in all.)

5. *Batrachoidichnites Deweyi*, at Middletown, Connecticut, and Gill, Massachusetts.

III. Order Dipodichnites, or *two-footed tracks.*

(1.) In Massachusetts, Connecticut, and New Jersey.

a. Sub-Order *Sauroidichnites*, or *tracks resembling those of Saurians.*

1. S. Barrattii.
2. S. heteroclitus.
3. S. Jacksonii.
4. S. Emmonsii.
5. S. Baileyi.
6. S. abnormis.
7. S. minitans.
8. S. longipes.
9. S. tenuissimus.
10. S. palmatus.
11. S. polemarchius.

b. Sub-Order *Ornithoidichnites.*

1. *Pachydactyli.*

1. O. giganteus.
2. O. Sillimani.
3. O. expansus.
4. O. gracillimus.

2. *Pachydactylo-Pterodactyli.*

1. O. Lyelli.
2. O. fulicoides.

3. *Leptodactyli.*

1. O. ingens.
2. O. elegans.
3. O. elegantior.
4. O. Deanii.
5. O. tenuis.
6. O. macrodactylus.
7. O. divaricatus.
8. O. Redfieldii.
9. O. isodactylus.
10. O. delicatulus.
11. O. minimus.
12. O. tetradactylus.
13. O. Danæ.
14. O. gracilior.
15. O. Rogersi.

In all thirty two species.

(2.) In Europe.

1. In Shropshire and Cheshire, England, three-toed; but I have no other evidence of their biped character.

2. Two species in Saxony of unknown animals.

IV. Order Apodichnites, or *footless tracks.*

1. By fish.
2. By mollusks.
3. By annelidous worms.

} In Gloucestershire and Flintshire, England.

Two or three remarks may perhaps be needed concerning the preceding tabulation of the footmarks, especially those of the valley of the Connecticut. The number of species may seem to some so large as to excite the suspicion that the characters on which they are founded are merely imaginary. But it should be recollected that these tracks have been obtained from a region eighty miles long and several miles broad, from more than sixteen quarries, and that they were not produced by birds that were cotemporaries, but which existed through a long series of centuries. Thus, at Turner's Falls we find tracks on layers dipping 40°, and separated from each other not less than eighty rods on the surface, and of course forty rods in perpendicular thickness; and how long it would take to deposite layers of fine sandstone and shale forty rods thick, let those familiar with the rules of geological arithmetic calculate. Taking the rate at which lakes fill up in Scotland as the basis of the estimate, it would require more than one hundred and thirty thousand years; and admitting a much more rapid rate of deposition, we should have time enough to expect a great variety of animals to have trod upon the different layers. I may have founded some species upon uncertain characters, and it would be strange if better specimens should not remove some of them from the list. But in general, when new specimens are brought under my eye, I find little more difficulty in distinguishing the different species, than in distinguishing the species of plants or animals; and hence I feel a good degree of confidence that the characters of these footmarks are constant and distinct. And if an attempt should be made to mark out the European footmarks into species, I predict that their number will be found greater than geologists now imagine.

My next remark relates to the large number of the names of distinguished scientific men, which I have ventured to attach to the footmarks of New England, in most cases without consulting them, as a testimony of respect. My apology is, that as advancing years admonish one how few more opportunities he will have to bear witness to the valuable services of those scientific brethren with whom he has been long allowed to labor, and as long acquaintance enables him better to appreciate how great must be his labors and sacrifices, especially in this country, who devotes his life to science,—as experience teaches all this, I say, he feels an increasing desire to give the world some token, however feeble, that he highly honors those who are doing so much for the welfare of the human family.

It may be proper for me in this connection to state a few facts in relation to the collections that have hitherto been made of the fossil footmarks of New England. I early transmitted a few specimens and a few casts to the London Geological Society, and more recently a much larger collection of casts and specimens to the Hunterian Museum in London; another to the Rev. W. B. Clarke of England; another to the Mineralogical Institute at Heidelberg; another to Dr. Tamnau of Berlin; and another to the Garden of Plants in Paris, although I have never learnt whether the latter has been received; and I have mentioned that some very fine slabs had been recently translated to the British Museum by Dr. Deane. In this country the best collection, except my own, is in the Massachusetts collection of rocks in Boston. A very good collection of casts and specimens is possessed by the Military Academy at West Point, and by Francis Markoe, Jr. Esq. at Washington. The collection of Prof. Silliman is also considerably full, as is that of Prof. Adams of Middlebury College. But I may be allowed to say, that my own collection in Amherst College is the only one that approaches to completeness yet made. That contains the originals from which the thirty four species have been described and figured. It consists of specimens of all sizes, from two or three inches square to a slab twenty three feet long, containing seven most distinct tracks of the huge *O. giganteus* in succession. There are also the rain-drops and the coprolites. At present the number of specimens is one hundred and fifty.

As it is now becoming very difficult to obtain good specimens, such as are wanted in large collections, I may be allowed to say, that I know of a few places where probably with considerable labor very good specimens may be obtained, and I will attempt it if desired.

Such is the history of footmarks. When Dr. Duncan in 1828 gave a brief account of the tortoise tracks of Scotland, he was by no means aware what a curious field he was opening to geologists. And the numerous cases that have been brought to light within the last sixteen years, now that the attention of geologists has been directed to the subject, illustrates a quaint remark of Dr. Macculloch, that we need to be taught to see. And now that geologists have their eyes open to this subject, we may anticipate many more curious facts and results.

And really this new field promises much fruit to geologists. It has already learnt them to be cautious in asserting the non-existence of land animals from the absence of their remains in a formation. In the valley of the Connecticut, for instance, more than thirty species of such animals, some of them of giant size, have left no other certain evidence of their existence save their footprints and a few coprolites. And we can hardly believe that birds were the only vertebral animal that dwelt in that valley during the red sandstone period. It would be illogical, indeed, to infer from hence that we know almost nothing respecting the fauna of the ancient world from exhumed relics. But it does teach us caution in our inferences as to the proportions of different classes.

This subject, too, has more than one valuable moral. It shows us that the most trivial movement of ours may make an impression on the globe that shall be brought out ten thousand ages hence with unimpaired freshness—that shall in fact be immortal. No geologist will think it at all extravagant to speak of the period when these tracks were impressed on the new red sandstone as tens and even hundreds of thousands of years ago. And yet they are as distinct and fresh as if made yesterday upon the mud; while mixed among them, we see the petrified rain-drops that fell at that same remote era. Really, in such facts one sees almost a realization of the ingenious thought of Prof. Babbage, founded on the equality of action and reaction, that "the air is one vast library, on whose pages are forever written all that man has ever said, or woman whispered; while the waters and the more solid materials of the globe bear equally enduring testimony of the acts we have committed."

In these footmarks, also, human ambition may read a lesson of an opposite character. A desire to leave our names inscribed with honor upon the world's history, is the almost universal passion. And to gratify it, what immense sacrifices of peace, health and life have in all ages been made! But among those who have been most influential at court, and most bold and successful on the battle field, how few have had their names transmitted to posterity. Over by far the greatest part has the wave of oblivion rolled; and even though Babbage's principle be true in theory, their intrigues, their valor, and their ephemeral renown will never be recovered. "Not a track remains," says Dr. Buckland, "or a single hoof, of all the countless millions of men and beasts whose progress spread desolation over the earth. But the reptiles that crawled upon the half finished surface of our planet, have left memorials of their passage enduring and indelible." And we may add, that the proudest monuments of human art will moulder down and disappear; but while there are eyes to behold them, the sandstone of the Connecticut valley will never cease to remind the observer of the gigantic races that passed over it while yet in an incipient state.

> "Reptiles and birds, a problem ye have solved,
> Man never has,—to leave a trace on earth,
> Too deep for time and fate to wear away."

And is it strange that the geologist should manifest a deep and even enthusiastic interest, when he discovers and attempts to decipher these curious archives of new, unknown, and peculiar animals that peopled the world untold ages before man became its possessor? How deep the interest of the antiquary, when he discovers and attempts to unroll some ancient papyrus that discloses a new and earlier chapter in a nation's history, or tells of the former existence of some race before unknown! And if an event deepens in interest the farther back it lies in the hoary past, how vastly in this respect do geological researches take the precedence of historical! For the chronologist can ascend the stream of time only a few thousand years; while the geologist makes his starting point the commencement of chronological dates, and the period of man's existence on the globe is too short as yet to form even an unit by which to measure the almost immeasurable past. And yet the solid strata reveal to him the history of those ages, so near the birth of time, with all the distinctness of yesterday; and he finds the laws by which Jehovah governed the universe then, engraved, like those given on Sinai, upon tables of stone.

Art. VIII.—*Discovery of more Native Copper in the town of Whately in Massachusetts, in the valley of Connecticut River, with remarks upon its Origin;* by Prof. Edward Hitchcock, LL. D.

(Read before the Association of American Geologists and Naturalists at Albany, April, 1843.)

In my Final Report on the Geology of Massachusetts, (Vol. II, p. 422,) as well as in some previous publications, I have mentioned the occurence of a mass of native copper, weighing seventeen ounces, in the drift of Whately, and I there express the opinion that it was derived from the trap of the Connecticut valley, in which, as well as in the associated sandstone, there exist veins of copper ore, such as the red oxide, the green carbonate and the pyritous. But within a few days, Dr. Bardwell of Whately has shown me another specimen of the native copper weighing sixteen ounces, found in drift, in such a part of that town as makes it excedingly difficult to see how it could have been derived by the force which accumulated the drift from any trap range; and I have been led to take a new view of the subject, which may have an important bearing in an economical respect.

In order to make intelligible the situation of these masses of copper when found in the drift, I must describe the position of Whately. The town extends westward from Connecticut River some six miles, and its eastern half is nearly level and underlaid by sandstone, covered by drift. In the western half, we find hornblende and mica slates, syenite and granite. Where these rocks emerge from the sandstone, the land rises, though not very rapidly, into hills a few hundred feet high. And it was along the line of junction between the sandstone and primary rocks, that the first piece of native copper was found; but the last one occurred in a ploughed field (as did the first) two miles farther to the west, that is, among hills and in the primary region. Now had they been brought from any trap range within the valley of the Connecticut, they must have been carried, especially the one last found, in a nearly southwest direction. But the direction taken by the drift in all the region was southerly, or usually a little southeast, as is shown by the boulders and the striæ on the rocks; and I have never met with a single example in which any block has been transported southwesterly. Nor do any fragments of trap or sandstone occur as far west in the primitive region as the locality of the recently found native copper. I incline, therefore, to the opinion, that neither of these specimens originated in the trap or the sandstone, but in the primary rocks to the north of the places where they were found. The region is one in which the primary slates have been greatly disturbed by the protrusion of syenite and granite, and therefore one where we might look for mineral veins as well as for the powerful agency of heat. Large veins of galena, with some pyritous copper and manganese, do in fact occur in the northwest part of Whately and Williamsburg, and a powerful vein of quartz with manganese exists in Conway, nearly north from the localities of the copper.

If this view should turn out to be correct, it may be that a valuable deposit of copper will one day be found in the region above described; and it is chiefly to turn the attention of observers to that region, that I have presented these considerations.

The native copper above described is mostly coated by the green carbonate and the red oxide. (?) The last specimen discovered was partially covered also with crytals of quartz deeply implanted. The extremities of these have all been broken off in efforts to cut the specimens into pieces, but their lower extremity may still be seen in the only fragment of this fine specimen which can now be found, and which I lay upon the table with the former specimen uninjured.

Art. IX.—*Secular Acceleration of the Moon's Mean Motion;* by James H. Coffin, late Tutor in Williams College.

It is one of the discoveries of modern astronomy, that the moon is slowly gaining time; that is, that it performs its revolutions now in less time than formerly. It is not two hundred years since Dr. Halley wishing to know the precise length of a lunation, went back to the ancient Chaldean observations, intending to ascertain how many new moons had occurred between that time and his own, and then to divide the time by this number, which would give the average length of each. But he was surprised to find that a lunation in those days was considerably longer than now. By comparing the Chaldean, Alexandrian, Arabian, and present observations, he found that the lunar period grew successively shorter.

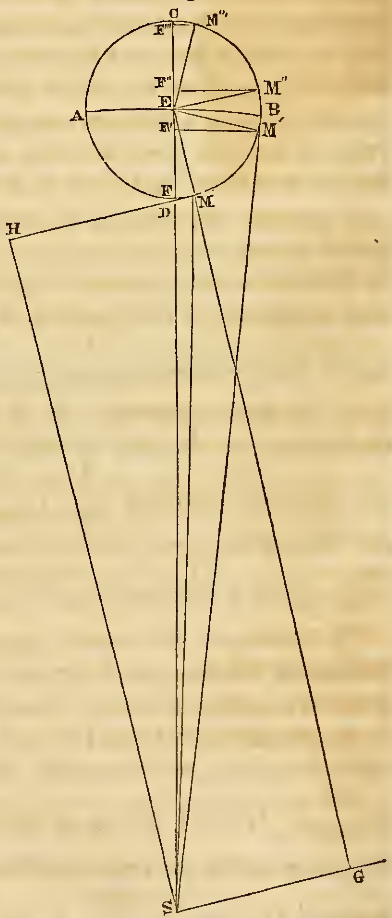
When incredulity in regard to the fact was succeeded by conviction of its truth in the minds of astronomers, it became an interesting question to account for it. The most plausible theory, and one which was generally adopted for about a century, was that the moon revolved in a resisting medium, which would cause it gradually to fall toward the earth, and thus by reducing the size of the orbit, make the periodic time less. But the fact that bodies so extremely tenuous and vapory as comets are proved to be, pass through this medium with little or no resistance, seemed and was truly an objection to the theory.

It was reserved for La Place, about sixty years ago, to explain the true cause of the acceleration, and by a refined and skillful analysis to calculate its amount theoretically. I propose in this article to investigate the cause, and arrive at the result by a different and perhaps more simple process, though I believe equally rigid.

Owing to the attraction of the other planets, the earth's orbit is gradually becoming less and less elliptical, or nearer and nearer to a circle; so that the sun is every year about 39½ miles nearer to the centre of the ellipse than it was on the year before. At this rate the orbit would become a circle in 40315 years; an event however that can never take place, for long before such a period of time shall elapse, the change in the shape, which is only an inequality of long period, will have reached its limit, and the eccentricity of the orbit will again increase.

It is well known that the sun's attraction diminishes the moon's gravity toward the earth, and thus increases its periodic time; and if it can be shown that the effect is greater than it would be if the earth revolved in a circle at the same mean distance from the sun, it is manifest that so long as the present change in the shape of the earth's orbit goes on, the moon's periodic time must grow less and less.

Fig. 1.



Let S (fig. 1) represent the sun, E the earth, AEB a portion of the earth's orbit, and BCAD the moon's orbit regarded as a circle.

As gravitation varies inversely as the square of the distance, when the moon is at D it is drawn away from the earth by the difference in the attractive forces of the sun on the earth and moon, and when it is at C, the earth is drawn away from it by the same cause. In both cases the tendency is to *diminish* the moon's gravity toward the earth. But when the moon is at A or B, its distance from the sun is the same as that of the earth, and consequently the sun's attraction on them is equal, but not being in the same direction it tends to draw them nearer together, and thus to *increase* the moon's gravity toward the earth.

The moon being thus drawn toward the earth in some parts of its orbit, and from it in others, it might seem rather difficult to determine whether on the whole it was drawn toward or from the earth. If we reflect however, that by increasing the sun's attractive power, it might draw the moon entirely away from the earth, causing it to assume an independent orbit, like Venus, it is obvious that the influence, whether great or small, is on the whole to draw the moon away from the earth, or diminish its gravity toward it as already remarked.

We will demonstrate that such must be the influence of the sun's attraction in a more rigid manner, and show how great it is compared with the earth's attraction on the moon. And let us first see what ratio exists between the attractive forces of the sun and of the earth on the moon when it is near quadrature, as at A or B. These points are selected because the moon is then at its mean distance from the sun, and consequently the sun's attraction is a mean, and equal to its attraction upon the earth.

If we take the quantity of matter in the earth as the unit by which to measure other quantities of matter, that of the sun will be 354936, which number we will call S. Then since gravitation is directly as the quantity of matter and inversely as the square of the distance, $\frac{1}{EB^2} : \frac{S}{SB^2 \text{ or } SE^2} ::$ the earth's attraction : the sun's attraction. Or if we take the earth's attraction on the moon as the unit by which to measure other attractions, the proportion becomes $\frac{1}{EB^2} : \frac{S}{SE^2} :: 1 :$ the sun's attraction on the moon at the point A or B, or its mean attraction for the whole orbit, which is therefore equal to $\frac{S \times EB^2}{SE^2}$ (1).

To investigate the subject generally, let the moon be at any point M of its orbit, and let the sun's attraction on it at that point be represented by m. Resolving this force into two others in the directions ME and ES, the proportion for the former or *addititious* force will read SM : ME :: m : the addititious force $= \frac{ME}{SM} m$. To find the mean addititious force for the whole of the moon's orbit, we may substitute for m its mean value above obtained (1), viz. $\frac{S \times EB^2}{SE^2}$, and for SM its mean value SE, and for ME its equal EB. The expression for the mean addititious force will then read $\frac{S \times EB^3}{SE^3}$ (2).

The proportion for the other force into which m was resolved, viz. in the direction ES, will read SM : SE : : m : the force required $=\frac{SE}{SM}m$. But the earth is attracted by the sun in the same direction ES, and it is the difference of the attractions only that exerts any disturbing influence on the moon in this direction. We will therefore find how much the attraction of the sun or the earth is, and subtract one from the other. By the laws of gravity $SE^2 : SM^2 :: m$: the sun's attraction at the distance SE. Hence the earth is attracted with a force equal to $\frac{SM^2}{SE^2}m$. Reducing the fractions to a common denominator and subtracting we have $\frac{SE^3-SM^3}{SE^2\times SM}m$. Now $SM=SE-EF$ very nearly; and by involving both sides of this equation, rejecting the third and fourth terms in the right hand member on account of their smallness, and transposing, we have $SE^3-SM^3=3SE^2\times EF$. Substituting this value in place of the numerator of the above fraction, it becomes $\frac{3SE^2\times EF}{SE^2\times SM}m$, which is equal to $\frac{3EF}{SM}m$, and is the disturbing force in the direction ES. Resolving this force into two others, one in the direction EM and the other at right angles to it, i. e. in the directions EG and GS, the proportion for the former or *ablatitious* force will read SE : EG or (since the triangles EGS and EFM are similar) ME : EF : : $\frac{3EF}{SM}m$: the ablatitious force $=\frac{3EF^2}{SM\times ME}m$.

To find the mean ablatitious force for the whole of the moon's orbit, take three other points M′, M″ and M‴ at the same distance from B and C that M is from D. Connect these points with E, and from them draw M′F′, M″F″ and M‴F‴, perpendicular to CD. According to the expression last found, the ablatitious force for the four points M, M′, M″ and M‴, is $\frac{3EF^2}{SM\times ME}m$, $\frac{3EF^{2\prime}}{SM'\times ME}m$, $\frac{3EF^{2\prime\prime}}{SM''\times ME}m$ and $\frac{3EF^{2\prime\prime\prime}}{SM'''\times ME}m$. To get the mean we must add them together, and divide by 4; or since the denominators are nearly equal, we may without much error take the mean of the numerators and of the denominators as the mean value of the fractions.* Now since AB may be considered a straight line, EF′ and EF″ are each equal to FM, and EF‴ to EF, so that the numerators added together make $6EF^2+6FM^2=$(Euc. 1. 47) $6ME^2$, which divided by 4 gives for the mean of the numerators $1\frac{1}{2}ME^2$. The mean value of the denominators is $SE\times ME= SE\times EB$, and the mean value of m is (1) $\frac{S\times EB^2}{SE^2}$. Hence the mean value of the above expressions for the ablatitious force at the four points taken, is $\frac{1\frac{1}{2}EB^2}{SE\times EB}\times\frac{S\times EB^2}{SE^2}=\frac{1\frac{1}{2}S\times EB^3}{SE^3}$. As this expression is independent of the position of M, it must be true for the whole orbit, and is the mean ablatitious force required.

The mean addititious force (2) was $\frac{S\times EB^3}{SE^3}$, which subtracted from the mean ablatitious force leaves for the net mean force diminishing the moon's gravity toward the earth, $\frac{\frac{1}{2}S\times EB^3}{SE^3}$.† (3).

Restoring the numerical value of S, viz. 354936, and giving to EB and SE their mean numerical values, viz. EB=237577 miles and SE=95024608 miles, the value of the expression last found becomes .0027735. But this value needs a correction, for the reason that the sun does not lie in the plane of the moon's orbit, as we have thus far assumed, which renders its disturbing influence less. Applying the necessary correction from this cause, the value of the expression is reduced to .0027689, or about $\frac{1}{361}$, so that the moon gravitates toward the earth only $\frac{360}{361}$ as much as it would do if the sun were absent from the system.

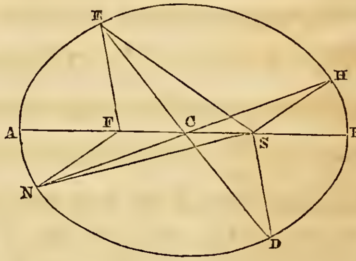
In the fractional expression $\frac{\frac{1}{2}S\times EB^3}{SE^3}$ the numerator is constant, since the moon's orbit is regarded as a circle in this investigation; but SE, the distance of the sun from the earth, varies in different seasons of the year. Hence the value of the expression must be reciprocally proportional to SE^3, and consequently

* The error when a maximum amounts to only about $\frac{1}{2500}$.

† Most writers who have treated of this subject, have preferred to represent the earth's attraction on the moon by $\frac{1}{EB^2}$, instead of representing it by unity as I have done, which converts this expression into $\frac{S\times EB}{2SE^3}$, or, as it is usually expressed, $\frac{mr'}{2r^3}$.

the mean of the reciprocals of the cubes of the sun's distance throughout the year, is to the reciprocal of the cube of the mean distance, as the actual diminution in the moon's gravity from the sun's disturbing influence, is to the diminution if the earth revolved in a circle at the same mean distance. To find the value of the first term of this proportion, let ADBE (fig. 2) represent the earth's elliptical orbit, and S the sun placed in one of the foci. Draw any two conjugate diameters ED and HN, and join SH, SE, SN and SD. Also from the other focus draw FE and FN.

Fig. 2.



Let the semi-transverse axis $=a$.

Let the semi-conjugate axis $=b$.

Let the eccentricity $=e$.

" $SE=a+x$.

" SD or $EF=a-x$.

" $SN=a+y$.

" SH or $NF=a-y$.

Then $\frac{1}{SE^3}=\frac{1}{(a+x)^3}=\frac{1}{a^3}-\frac{3x}{a^4}+\frac{6x^2}{a^5}-\frac{10x^3}{a^6}$, &c.

And $\frac{1}{SD^3}=\frac{1}{(a-x)^3}=\frac{1}{a^3}+\frac{3x}{a^4}+\frac{6x^2}{a^5}+\frac{10x^3}{a^6}$, &c.

By adding the two series together, and omitting all terms except those written down on account of their smallness, we have

$$\frac{1}{SE^3}+\frac{1}{SD^3}=\frac{2}{a^3}+\frac{12x^2}{a^5}.$$

In like manner $\frac{1}{SN^3}+\frac{1}{SH^3}=\frac{2}{a^3}+\frac{12y^2}{a^5}$.

Hence $\frac{1}{SE^3}+\frac{1}{SD^3}+\frac{1}{SN^3}+\frac{1}{SH^3}=\frac{4}{a^3}+\frac{12(x^2+y^2)}{a^5}$, and the mean value of the four is $\frac{1}{a^3}+\frac{3(x^2+y^2)}{a^5}$.

Now by the properties of the ellipse, $CH^2=SE\times EF=(a+x)\times(a-x)=a^2-x^2$.

And in like manner, $CE^2=a^2-y^2$.

Therefore $CH^2+CE^2=2a^2-(x^2+y^2)$.

Or by transposition $x^2+y^2=2a^2-(CH^2+CE^2)$.

But by properties of the ellipse, $CH^2+CE^2=a^2+b^2$, and $b^2=a^2-e^2$.

Therefore $x^2+y^2=2a^2-(2a^2-e^2)=e^2$.

Substituting e^2 in place of (x^2+y^2) in the foregoing expression for the mean value, it becomes $\frac{1}{a^3}+\frac{3e^2}{a^5}$. This expression contains only constant quantities, and since ED and HN are any conjugate diameters, it is true for the whole orbit. Therefore the first term in our proportion, which we wished to find, viz. the mean of the reciprocals of the cubes of the radii vectores of an ellipse, is $\frac{1}{a^3}+\frac{3e^2}{a^5}$.

The mean distance of the earth from the sun is equal to the semi-transverse axis of its orbit, and the second term in our proportion is therefore $\frac{1}{a^3}$. Multiplying both these terms by a^3 to render the proportion more simple, and stating it in form, it reads thus, $1+\frac{3e^2}{a^2} : 1 ::$ the actual diminution of the moon's gravity : the diminution if the earth revolved in a circle at the same mean distance, which we have shown to be $\frac{1}{361}$ of the earth's attraction. In this proportion the second and fourth terms are constant; therefore if the first varies, the third must vary in the same ratio. Now we remarked near the commencement of the article, that e, the eccentricity of the earth's orbit, is diminishing about $\frac{1}{40315}$ every year, and as the squares of numbers where the difference is small vary about twice as much as the numbers themselves, e^2 diminishes about $\frac{2}{40315}$ in a year. Further, e is about $\frac{1}{60}$ of a, and consequently $\frac{3e^2}{a^2}$ is about $\frac{1}{1200}$ of 1; therefore the annual diminution of the first term in the proportion is $\frac{2}{40315}$ of $\frac{1}{1200}$ of the second, and the third must diminish by the same fraction of the fourth. But the fourth term is $\frac{1}{361}$ of the moon's gravity toward the earth. Hence the annual diminution in the third term is $\frac{2}{40315}$ of $\frac{1}{1200}$ of $\frac{1}{361}=\frac{1}{8732229000}$ of the moon's gravity toward the earth. That is to say, the sun diminishes the gravity of the moon toward the earth less and less every year by the value of this fraction, which therefore expresses the annual increase of gravity.

This variation in gravity affects the moon's periodic time in two ways; 1st, by contracting its orbit so that it has a less distance to travel; and 2d, by increasing its absolute velocity. The first is obvious, and to show its amount let g represent the moon's gravity toward the earth on any given year, and r the mean distance; also let m represent the annual increase of gravity, and x the diminution in the distance occasioned thereby. Then will $r-x$ represent the distance on the succeeding year, and $\frac{r^2g}{(r-x)^2}+m$ the gravity, viz. g increased by the laws of gravitation in the inverse ratio of the square of the distance, and also by the quantity m. Now it has been demonstrated,* that in circular orbits where the radius varies from a variation in the force which retains the revolving body in its orbit, the centrifugal force will vary in the inverse ratio of the cube of the distance; and since the centripetal and centrifugal forces must be equal,

Therefore $\frac{1}{r^3} : \frac{1}{(r-x)^3} :: g : \frac{r^2g}{(r-x)^2}+m.$

Expanding, omitting the higher powers of x on account of their smallness, and multiplying by r^3, we have

$$\frac{1}{r} : \frac{1}{r-3x} :: g : \frac{gr}{r-2x}+m.$$

Hence $$\frac{g}{r-3x}=\frac{g}{r-2x}+\frac{m}{r}.$$

Clearing of fractions and omitting the higher powers of x,

$$gr^2-2grx=gr^2-3grx+mr^2-5mrx.$$

Hence $grx=mr^2-5mrx.$

And $gx=mr-5mx.$

Reconverting the equation into a proportion,

$$g : m :: r-5x : x.$$

And by multiplication and composition,

$$g+5m : m :: r : x.$$

Now m is $\frac{1}{8732229000}$ of g, and therefore x is $\frac{1}{8732229005}$ of r. Hence the contraction of the lunar orbit, even if the moon's absolute velocity remained unchanged, would reduce its periodic time by the amount of the latter fraction annually. But we shall presently show that the absolute velocity is increasing annually by the same fraction. If this is so, it follows that from both causes combined its sidereal period must decrease annually by about double the aforesaid fraction, so that the moon must pass over a greater number of degrees every year than it did on the year preceding by about $\frac{2}{8732229005}$.

At the commencement of the present century the moon's mean motion was such as to carry it through 13 sidereal revolutions

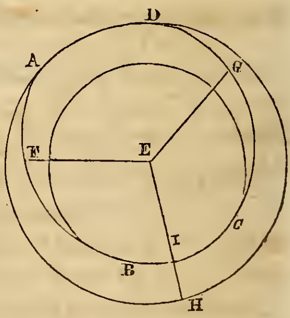
* See Stewart's Mathematical and Physical Tracts.

and 132° in a year. Hence the annual acceleration is equal to $\frac{2}{8732229005}$ of 13 revolutions and 132°, which is about $\frac{1}{252}$ of 1″; a quantity quite too small to be noticed in a single year, but which increasing like other uniformly accelerated motions as the square of the time, becomes quite conspicuous in the lapse of centuries. According to our calculation it would amount to over 4° in 2000 years, though from the comparison of ancient with modern observations this appears to be too great.

It remains to demonstrate the truth of the principle which was employed above hypothetically.

To show generally that the absolute velocity of the moon is increased when the orbit contracts, and diminished when it dilates, let us suppose it, revolving in the larger circle DAH, to have arrived at the point A, when owing to an increase in the attractive power of E it is drawn along the curve AB into the circle BIC. As a line drawn from E to any point F in the line AB, makes the angle EFB acute, the motion must be accelerated along AB; so that when the moon arrives at B it will be moving with a greater velocity than when it left A. And this increased velocity will be retained so long as it revolves in the smaller circle. When it has arrived at C, let the attracting power of E be diminished so as to allow the moon to recede along the curve CD into its former orbit. The angle EGD being now obtuse, the attraction of E holds the moon back and retards its motion just as much as it accelerated it along the curve AB.

Fig. 3.



The principles of central forces enable us to find the precise ratio of the velocities in the two circles, which we will suppose to be the orbit of the moon in two successive years.

We have already observed, that the centrifugal force varies inversely as the cube of the distance. But it is well known that it is also proportional to the square of the velocity divided by the distance. Therefore if V represent the velocity in the outer circle, and V′ in the inner, we have

$$\frac{1}{EH^3} : \frac{1}{EI^3} :: \frac{V^2}{EH} : \frac{V'^2}{EI};$$

from which we can easily deduce

$$EI : EH :: V : V'.$$

But we have shown that EH is greater than EI by $\frac{1}{8732229005}$; therefore V' is greater than V by the same fraction; or in other words, the velocity of the moon increases $\frac{1}{8732229005}$ annually, and our former assumption is thus proved to be correct.

Art. X.—*Review of Alger's Phillips' Mineralogy, and Shepard's Treatise on Mineralogy.**

The past six months have been remarkable in the history of American mineralogy. In the April number of this Journal, we noticed the first production of the season, by J. D. Dana, of this place. Early in June last, Phillips's Mineralogy, by Fr. Alger, made its appearance at Boston, and towards the latter part of July, the second edition of Prof. C. U. Shepard's "Treatise" left the New Haven press.

Mr. Alger's work exhibits great labor in searching out the late discoveries from foreign and American journals. The original

* 1. An Elementary Treatise on Mineralogy, comprising an Introduction to the Science. By William Phillips, F. L. S., M. G. S. L., &c. &c. Fifth edition, from the fourth London edition; by Robert Allan: containing the latest discoveries in American and Foreign Mineralogy with numerous additions to the Introduction; by Francis Alger, Member of the American Academy of Arts and Sciences, of the National Institute for the Promotion of Science, of the Boston Society of Natural History, etc. 8vo, pp. 150 and 662. Boston, William D. Ticknor & Co. 1844. $3.

2. A Treatise on Mineralogy. By Charles Upham Shepard, M. D., Prof. of Chemistry in the Medical College of South Carolina, &c. &c. Second edition. 12mo, pp. 168. New Haven, A. H. Maltby, 1844.

Note.—As a portion of this edition is bound up with the descriptive part of the first (1835) in two volumes, it may be supposed that a new edition of the *entire* work has been prepared, particularly as the title on the back reads "*Treatise on Mineralogy, by C. U. Shepard, second edition*, 1844, *price* $1.50." Although it might appear to the author that the facts were obvious, still we think that the exact state of the case should have been mentioned. The following appears to be the author's explanation on this point.

"In giving the characters of the species, (p. 106 to the end,) I have appended (within parentheses) to each, the most interesting information of various kinds, which has been brought forward since 1835, with a view to supply the principal deficiency which the use of the treatise might occasion to such as wish to employ it, in connection with my general work on descriptive mineralogy of that date."—*Adver. to the new edition of the Introduction, New Haven, June*, 1844.

Persons still desiring the two treatises can obtain them bound together as a single volume.

Treatise by Phillips has been enlarged by three hundred pages, and more than one hundred additional figures. The introductory chapters have been somewhat extended, a table of formulas for chemical composition added, and full accounts inserted of American localities.

Mr. Alger's successful labors in the additions he has made, lead us the more to regret that he should have engrafted them upon a work so old as the Treatise of Mr. Phillips. With all his assiduity and skill, the faults of the original are too apparent, and we feel assured that the editor would have done a greater favor to the science, if he had abandoned his assumed patron, and given us his own ideas in his own way.

Mr. Phillips's great good sense and accurate knowledge made his Treatise, in 1819 and 1823, the most popular one on mineralogy which had ever appeared in the English language, perhaps we might say, with truth, in any language. It is certain that the same qualities which gave character to the first three editions, would, after the lapse of nearly a quarter of a century, have led to the adoption of a method more in accordance with the present advanced state of knowledge.

The classification of the species in Phillips was based on the chemical principles of the day, and bears at the present time strong marks of antiquity. So great have been the changes in chemical science, that we can now hardly imagine how the species should ever have fallen into such strange associations, unless perchance they had cast lots among themselves for their places. The first two classes are, 1. *Earthy minerals*, 2. *Alkaline earthy minerals*. When an earthy mineral contains a trace of an alkali, like feldspar, it goes into the second division; otherwise, it falls into the first. As the alkalies are isomorphous with certain bases not alkaline, it occasions singular unions, and as strange disseverings of families and species. The following paragraph explains the system of arrangement in Class I, (p. 1.)

" This class includes those minerals which consist of one earth or more, united with definite proportions of water, and sometimes with common metallic oxides, as of iron and manganese, and rarely with an acid. These latter substances, however, are frequently to be regarded as mere mixtures of accidental and variable constituents of the species described. We shall begin with silica in its purest form, as being the oldest and most abundant mineral, and as affording the most simple arrangement; and then proceed to such as, by the most authentic analyses, appear to consist chiefly of silica. Those of which alumina forms the greatest proportion succeed, as being the earth next in age and abundance. Magnesia follows; then such minerals as consist primarily or in part of zirconia or glucina, or lastly of yttria and thorina." Independently of the loose principles of arrangement here laid down, it sounds like the fancies of days long gone by, this making silica the "*oldest*" mineral, and alumina "next in age!"

The work begins with the species in the following order:—*Quartz*, *opal*, *karpholite*, *alumocalcite*, *garnet*, *idocrase*, *xanthite*, *Gehlenite*, *Prehnite*, *stilbite*, *Heulandite*, *Davyne*, *Laumonite*, *zoisite*, *epidote*, &c.; a strange commingling of minerals, whether we consider the principles of chemistry or natural history. From quartz and opal, the first transition is to *karpholite*, a hydrous silicate of alumina and manganese; next to *alumocalcite*, a variety of opal; then to the complex anhydrous earthy mineral *garnet:* soon follows *Prehnite*, stilbite, Heulandite, &c., hydrous species; and then the wanderers, zoisite and epidote.

Take another place in the system, where we find the following order, (p. 192, &c.)—*Feldspar*, *ryacolite*, *petalite*, *spodumene*, *Latrobite*, *agalmatolite*, *glaucolite*, *mesotype*, *Thomsonite*, *Peristerite*, *leucophane*, *mesole*, &c. After feldspar and ryacolite we find nothing of the other feldspar minerals, *anorthite*, *albite*, &c., so ably investigated by Abich; these occur many pages off: but there follows *spodumene*, and soon after, the zeolites, *mesotype* and *Thomsonite;* next, Thomson's *Peristerite*, (nothing but an iridescent feldspar from Perth, U. C.;) after this, another zeolite, *mesole*.

On page 221 and beyond, *Elæolite* is followed by *Hauyne;* then *hydrous anthophyllite*, a hydrous mineral near *steatite* in its characters; next *antrimolite*, a zeolite; *Pericline*, one of the feldspars, (which by the way has been shown of late by Gmelin to be identical with albite;) *Labradorite*, *albite*, other feldspar minerals; and then the hydrous silicate, *analcime*. There is still some appearance of system in this arrangement, for these minerals contain an alkali. Such incongruous connections prove the fallacy of adopting any general principle as an implicit guide in classification. They cannot be looked upon with much complacency in the present state of science. We observe farther, that anthophyllite, shown of late to have the same general formula with hornblende, is separated from it by fifteen or twenty species of very various characters; and many more, separate bronzite, hypersthene, acmite and Cummingtonite, from augite. Automolite and dysluite, having the same general formula with spinel, are yet twenty pages off. But farther illustration is unnecessary. Whatever may be said of *chemical systems* of classification, surely this has little to recommend it, failing as it does of all the advantages of system, and not even securing the benefits of an alphabetical arrangement. The remaining classes are less objectionable. The class of metals and metallic ores might have been conveniently subdivided into orders, including each the ores of a metal, instead of forming, as now, an unbroken series. The order in which the metals succeed one another is quite at variance with the teachings of chemistry.

We are confident that Mr. Alger has not done justice to his own science in the classification adopted; and certainly not to Mr. Phillips, in presenting the views of twenty years since under his name in 1844.

The table of formulas for the composition of minerals added to the volume by Mr. Alger, are given mostly in mineralogical symbols. Concerning these formulas, we read in the Preface: "Chemical formulas have occasionally been given when employed by the analysts themselves, or in stating the composition of some of the more complex species, particularly of the metals; but for the purposes of mineralogy, they should not be generally introduced to the exclusion of the mineralogical signs, which answer all the ends desired in making known the proximate constituents of the species, and do not involve a knowledge of chemistry beyond that which most students should possess." Justice is hardly done to the mineralogical symbols, in implying that they make known only the *proximate constituents* of the species: for the information conveyed relative to the composition is as definitely stated as by chemical symbols; and a better knowledge of chemistry appears to us necessary to understand the former than the latter. The mineralogical formula for Boltonite (Alger, p. cxxx,) MgS^2, conveys as definite an idea of its constitution to the chemist, as the corresponding chemical formula, $\dot{Mg}^3\dddot{Si}^2$. The index $(^2)$ after S, implies that the atoms of oxygen in the silica are double the number in Mg, (magnesia,) and in translating it into the chemical formula, MgS^2 becomes, as above, $\dot{Mg}^3\dddot{Si}^2$, (3 parts of magnesia and 2 of silica,) an expression less easily printed, but more readily understood by the uninitiated. The mineralogical formula would be read by the student as bisilicate of magnesia; but few would know that this bisilicate of magnesia consists of 3 atoms of magnesia and 2 of silica, as is read at once from the chemical symbols.

Thomson and Beudant are in general the authorities referred to for the formulas, although Thomson's many errors are well known. The late chemical treatise of Rammelsberg has contributed somewhat to these tables, and so far they are unquestionable authority.

The descriptions of the species are given with fullness, and valuable remarks added respecting the uses of such as are employed in the arts. In no part of the work do Mr. Alger's labors appear to better advantage than in the numerous interesting items of information which he has brought into his descriptions, evincing a minute and familiar acquaintance with minerals both in the closet and field. The extensive enlargements of this part of the work must have required great labor, and in general evince discrimination and judgment. The long lists of American localities render it a convenient work for the field, and the completeness of the information given relative to the chemical, physical, and economical characters of minerals, makes it as well a valuable text-book for the lecture room.

Nearly all that is new regarding American species has already been noticed in the late numbers of this Journal. The following are the characters of the Beaumontite of Dr. Jackson, cited from pages 497, 498.*

Beaumontite.

Crenated Hydro-silicate of Copper. *C. T. Jackson.*

"This mineral was obtained from the old carbonate of copper mine of Chessy, France, in 1832. It was regarded as a hydrated silicate of copper, but subsequent analysis proved it to be a crenated hydro-silicate of copper. The analysis of a specimen which had effloresced by exposure to the air yielded to Dr. Jackson:—Silica, 21·0; deutox-

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* See also this Journal, Vol. XXXVII, p. 398.

ide of copper, 46·8; crenic acid, 15·8;* water, 10·0; alumina and iron, 4·4; carbonic acid and loss, 2·0; = 100·0.

"It has the following characters. Forms stalactitical; color blue-green, when fresh, but greenish-white when dry. Specific gravity 1·88. Soft pulverulent when dry.

"In the close tube gives off an abundance of water, when heated; the recent specimens giving the largest proportion. Heated to redness a portion of the copper is reduced, and the mineral in the tube is found to be a mixture of portions of copper with yellow and white powder. Before the blowpipe, on charcoal, it first decrepitates a little, then shrinks, leaving numerous cracks in it, and at last partially fuses and becomes yellow and white. On being levigated, yields particles of metallic copper. With soda, fuses, giving a button of copper. With acids, it gelatinizes, and the solution is green and turns blue by excess of ammonia. When mixed with water, and sulphydric acid gas is passed through it, the copper separates as a sulphuret, and crenic acid and silica may be obtained by evaporating the solution to dryness. The crenic acid may be separated from the silica by a solution of carbonate of ammonia.

"It occurs in stalactites on the roof of the mine, and is continually forming by infiltration through the porous sandstone rock. When recently obtained, it contains a much larger proportion of water, which it loses by exposure to dry air."

The hydroborate of lime or borocalcite of Mr. Hayes, brought by Mr. Blake from Tarapaca, Peru, is named Hayesine by Mr. Alger, in honor of Mr. Hayes.

The account below of Glauberite from Tarapaca is cited from page 616.

Glauberite.

"This mineral was among the specimens brought by Mr. Blake, from the province of Tarapaca, in Peru. From a qualitative examination, Mr. Hayes ascertained that it contained sulphuric acid, soda and lime. He afterwards submitted it to analysis, and found it to be identical with glauberite. It gave him the following results:

			Ratio.
Sulphuric acid, - -	57·220	11·44	2
Soda, - - -	21·324	5·33	1
Lime, - - -	20·680	5·90	1
Protoxide of iron, -	·444		
	99·668		

* With a small portion of phosphoric acid.

" There is a slight deficiency in the quantity of soda, but this salt evidently consists of one atom sulphate of soda, one atom sulphate of lime. This is the composition of the variety from Villa Rubia, in Spain, originally analyzed by Brongniart. Formula : $N\dot{S}l+Cal\dot{S}l$, or as stated by Mr. Hayes, $\dot{Na}\dddot{S}+\dot{Ca}\dddot{S}$.

" It occurs in extremely brilliant, colorless and transparent crystals, imbedded in hydrated borate of lime, or Hayesine. They are in the form of elongated oblique rhombic prisms, simply replaced on their obtuse terminal edges, by single planes."

The Ledererite of Dr. Jackson, which Mr. Alger retains as a distinct species, is described as follows, on pages 214, 215.

Ledererite.

C. T. Jackson. (*Am. Jour. of Sci.*, Vol. xxv, 78.)

" This mineral is composed, according to the analysis of A. A. Hayes,* of silica, 49·47 ; alumina, 21·48 ; lime, 11·48 ; soda, 3·94 ; phosphoric acid, 3·48 ; oxide of iron, 0·14 ; water, 8·58.

" Sp. gr. 2·10. H.=6.

" It occurs in crystals which are sometimes colorless and transparent, but usually white and opake, or only translucent on the edges, some of them being of a pale salmon color. The crystals are in the form of hexahedral prisms, deeply replaced on their terminal edges, or terminated at both extremities by hexahedral pyramids, having at their summits a small plane termination, perpendicular to the axis of the prism ; indicating a regular hexahedral prism for the primary form. This form is further indicated by the separation of faces of cleavage made visible by exposure to heat. Before the blowpipe, according to Hayes, it becomes white, and divides at the natural joints ; at a higher temperature it fuses into a white enamel, which can be rendered more vitreous by continuing the blast ; a few bubbles are disengaged when it is thus treated. In the matrass, a slight empyreumatic odor is perceptible. Its inferior hardness and specific gravity, but more especially its pyrognostic characters and chemical composition, clearly separate it from the species hydrolite, or gmelinite, to which it has been referred : one consisting of bisilicates of alumina and lime, silicate of soda, with six per cent. phosphate of lime, and only 8·58 water ; the other, by Mr. Connell's analysis, of bisilicate of alumina, tersilicate of lime, soda and potash, no phosphoric acid, and 21·66 per cent. water.

" Some of the crystals are elongated, and measure one third of an inch in the direction of the prismatic axis, but most of them possess

* Am. Jour. of Sci., Vol. xxv, p. 84.

nearly equal dimensions in the opposite direction, or they are sometimes even in low flattened prisms. The secondary planes on the edges incline on M, at an angle of 130° 5′, and towards each other, at 142° 10′, as determined by the reflective goniometer, by M. Dufrénoy, of the School of Mines in Paris. Ledererite was discovered by Dr. Jackson and the editor, between Cape Split and Cape Blomidon, Nova Scotia, in the cavities of amygdaloid, accompanied by calcareous spar, mesotype, analcime and stilbite. It has become a very rare mineral, and is no longer found at the locality."

There seems to be no sufficient reason for supposing the phosphoric acid, detected by Mr. Hayes, essential to this mineral. According to Rammelsberg, its chemical formula differs from that of gmelinite only in presenting one third as much water. The inclination of the pyramidal planes on M, in gmelinite, according to Rose, is 130° 27′, which is near the same angle in Ledererite, as above given.

The new mineral *Pyrrhite* is stated on pp. 625, 626, to have been recognized among some minerals brought from the Azores by Prof. Webster; the following is the account of it.

Pyrrhite.

"This exceedingly rare and beautiful mineral, hitherto found only in Siberia, and of which but a single specimen comprising eight crystals, is known to mineralogists, has been recognized among the interesting substances recently brought from the Azores, by Prof. J. W. Webster. The specimen was placed in Mr. Teschemacher's hands for examination by Prof. Webster, and was supposed, by both of these gentlemen, to be a new substance. On comparing it with pyrrhite, as described at page 176, Mr. Teschemacher was at once convinced of its identity with that substance. He has furnished the following description. Form, beautifully perfect regular octahedrons; color, deep orange-yellow; transparent on the edges, with a brilliant vitreous lustre. Hardness equal to that of feldspar. The crystals are from one half to two lines in length, and they are superimposed on a white feldspar, or albite. The minutest crystals are quite transparent. One of these exposed to the oxidating flame of the blowpipe, became opake, retaining its orange color, but duller. Changed to the reducing flame, it melted without frothing, and assumed a deep, dull indigo-blue color, which could only be distinguished from black in a bright light, and on the minute edges of the fused crystal. With borax, it melts into a dark brown glass, apparently colored by iron.

"It is probable that the mineral contains some titaniate, and that the blue color almost instantly assumed by the assay, is owing, as Kersten has shown, in the case of some of the titaniferous silicates, to the formation of blue oxide by the loss of oxygen in titanic acid. Prof. Webster has the subject in hand for a chemical analysis, and he has taken measures to procure a larger supply from the locality."

Several recent analyses were made for the treatise in the course of its progress, which were also sent by the analysts to Mr. Dana, and were quoted in reviewing his work.

We could have wished that Mr. Alger had used his judgment more unsparingly in condemning species that are bad. We run over his book, alluding to some errors which seem of sufficient importance to demand at least a passing remark—especially those that may convey abroad a wrong impression with regard to American species.

Cyprine, (p. 22.) This mineral is united by Mr. Alger with garnet instead of idocrase, in consequence of an analysis by Mr. Richardson,—evidence of no weight, inasmuch as garnet and idocrase are admitted by chemists to have the same composition. The structure of Cyprine is identical with that of idocrase, from which it differs only in its light blue color.

Xanthite, (p. 32.) Xanthite is retained as a species. We have seen crystals the same in form with those of *idocrase*, and there appears to be no doubt of their identity.

Fibrolite, (p. 108.) This mineral is united with kyanite. The fibrolite of Count Bournon from the Carnatic has long been considered Bucholzite, with which it is probably identical. The name fibrolite was early applied in this country to a variety of kyanite in short fibrous prisms, supposed at the time to be identical with Bournon's mineral.

Davidsonite, (p. 121.) Rammelsberg states that this mineral, according to the investigations of Breithaupt, Plattner and Lampadius, is nothing but beryl. Lampadius obtained for its composition, silica 66·10, alumina 14·58, glucina 13·02, magnesia 1·16, peroxyd of iron 0·52, water 0·80.*

Hudsonite, (p. 127.) The Hudsonite of Beck, here admitted to the rank of a species, is identical with augite in structure, and is near Hedenbergite in chemical characters.

Thulite of Thomson, (p. 131.) This species, here retained, though hesitatingly, is now considered a rose-colored epidote.

* Handwörterbuch, i, 189.

Later analysts do not discover the cerium supposed to be detected in it by Dr. Thomson.

Polyadelphite of Thomson, (p. 135.) This mineral, from Sussex Co., N. J., is nothing but a yellowish granular garnet, as is readily seen at the locality. Compare the analysis, p. 135, with the composition of garnet, on pages 23, 24 and 25 of our author.

Marmolite of Nuttall, (p. 153.) This mineral, from Hoboken, N. J., is still continued as a species, although long since determined by Shepard to be identical in composition with serpentine. The analyses of Vanuxem* and Lychnell† lead to the same conclusion.

Boltonite, (p. 154.) This mineral has been united by Mr. Alger with Picrosmine, and we think without sufficient reason, as the Boltonite, according to Thomson's analysis, is *anhydrous*, while Picrosmine contains nearly *seven and a half per cent. of water*, and its resemblance in other respects is not very close.

Boltonite.		Picrosmine.	
Silica,	56·64	Silica,	54·88
Magnesia,	36·52	Magnesia,	33·34
Alumina,	6·07	Protox. Mang.	0·42
Protox. Iron,	2·46	Protox. Iron,	1·39
		Water,	7·30
	101·69, Thomson.		97·33, Magnus.

Talc, *Chlorite*, *Soapstone*, (pp. 155, 150.) Talc and chlorite are united in one species by Mr. Alger, and steatite separated from talc. The investigations of Lychnell have shown that steatite (*soapstone* in the arts, *speckstein* of the Germans) is identical with talc in composition, and that both are *anhydrous*. The soapstone of Cornwall, (*saponite*,) confounded by Mr. A. with steatite, is a *hydrous* species, quite peculiar in its butter-like consistence when moist, and its white earthy appearance on drying. Chlorite contains 8 to 12 per cent. of water, and is wholly distinct from talc. Von Kobell separates chlorite on chemical grounds into the two species, *chlorite* and *ripidolite*.‡ *Nacrite*, which is united, in the work before us, with talc, has the unctuous feel of this mineral, but is aluminous instead of magnesian.

Danburite of Shepard, (p. 172,) is a doubtful species, as stated in Vol. XLIV, p. 384, of this Journal.

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* J. Acad. Sci. Philadelphia, iii. † K. Vet. Ac. Hand. 1826.

‡ Erdmann's Jour. xvi.

Esmarkite, (p. 176.) This mineral, described by Erdmann, is identical with Dr. Jackson's Chlorophyllite, (see p. 62,) which differs in composition from Bonsdorffite, (or Hydrous Iolite,) and Fahlunite, only in containing less water.

Nuttallite, (p. 220.) Nuttallite has been considered a distinct species on the ground of Thomson's analysis. But its crystallization is the same with scapolite, and, until examined by another chemist, it should not rank as a species. In Mr. Alger's work it occupies a distinct place, some fifteen pages in advance of scapolite, from which it is separated by elæolite, Hauyne, hydrous anthophyllite, periclin, Labradorite, albite, analcime, sodalite, &c.

Elæolite, (p. 221.) Elæolite, which is here continued as a species, Scheerer has shown to be identical with nepheline, both in composition and structure.*

Calstronbaryte, (p. 306,) *Emmonsite*, (p. 309.) These are admitted mechanical mixtures, and might better be rejected than retained with a query.

Göthite, (p. 355.) Distinct from Limonite, with which it is united, in containing less water. The formula of Limonite (Brown Iron Ore,) is $\dddot{Fe}^2\dot{H}^3$, while that of Göthite is $\dddot{Fe}\dot{H}$.

Titaniate of Iron (*Menaccanite*,) *Crichtonite*, *Ilmenite*, (pp. 378—380.) These minerals are admitted as distinct species. The compounds of titanic acid and iron are so various, that many more might be added; and as they are all isomorphous, it appears preferable to unite them under one species, making the different compounds varieties. The analysis given of Hystatite, (p. 379,) is that of Menaccanite, according to Von Kobell, the author quoted. Von Kobell gives Mosander's analysis of Hystatite as follows: Titanic acid, 24·19; protoxyd of iron, 19·91; peroxyd of iron, 53·01; lime and magnesia, 1·01; silica, 1·17.† The *Washingtonite* of Shepard (p. 603) is identical nearly with the variety *Hystatite.*

Torrelite of Thomson, (p. 384.) The Torrelite from Middletown is the common columbite of that place. In crystallization it is identical with the Bodenmais columbite. Mr. Alger has not distinguished the kimito-tantalite (Ferro-tantalite) from the co-

* Poggendorf's Annalen, xlix, p. 359, 1840.

† Grundzüge der Mineralogie, p. 318, Nürnberg, 1838.

lumbite of Bodenmais, lately shown to be distinct in crystallization as well as composition.*

Silicates of Manganese, (p. 395.) The silicate and sesquisilicate of manganese from Franklin, N. J., established as species by Dr. Thomson, and here retained, are believed to be only impure or half decomposed varieties of the common bisilicate of manganese, (Kieselmangan of the Germans.)

Torrelite of Renwick, (p. 418.) This supposed species was long since shown to be an impure ferruginous jasper.

Arsenate of Iron, (p. 373,) *Scorodite*, (p. 599.) These are one species, the analyses of the former being the correct composition of the latter. One of these analyses is repeated under Scorodite.

Ter-arseniet of Cobalt, (p. 446.) This species is considered by Berzelius as a mechanical mixture, probably of cobaltine and sulphuret of bismuth.†

Pimelite, (p. 455.) This mineral is clay colored with oxyd of nickel, and not a distinct species.

Biotine, (p. 607.) Brooke has studied the crystals of this mineral, and shown them to be identical with anorthite.‡

Breislakite, (p. 608.) This mineral was supposed to contain copper as a principal ingredient, as is stated in the work before us. But late examinations have shown that it is not an ore of copper, and is not far removed from the hornblende family.§

Pickeringite, (p. 616.) The analysis by Thomson of specimens supposed to be this species, should not be permitted to throw doubts over the results of our own chemist, Mr. Hayes. Dr. Thomson must have either made some error in his analysis or have examined some other species.

Stellite, (p. 624.) The Pectolite of von Kobell and Stellite of Bergen Hill, N. J. have been united by Alger, as was done also by Dana. To these Mr. Alger has proposed to add the stellite of Thomson; and the three united he calls *stellite*. But we should

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* See this Journal, Vol. XXXII, p. 149, on the identity of Torrelite of Thomson with columbite, by J. D. Dana; also Rammelsberg's Handwörterbuch, vol. ii, p. 195, where he states that it has essentially the same composition with the Bodenmais columbite.

† Rammelsberg's Handwörterbuch, ii, 274. Berz. Jahresb. vii, 175.

‡ Phil. Mag., vol. x, 1837.

§ Rammelsberg's Handwörterbuch, supplement, p. 32.

hesitate to place the stellite of Dr. Thomson, containing six per cent. of water, with pectolite, believed, as Mr. Alger states, to be anhydrous. He also suggests that the mineral is an *anhydrous* lime-mesolite, an unfortunate name, as it creates confusion to extend the name mesolite to an anhydrous mineral; for on no principle yet discovered, can the one (hydrous) be isomorphous with the other. The crystals of Bergen Hill, examined by Mr. Teschemacher, are stated by Mr. Alger to have had the form of mesolite, and on this ground the name is proposed. Frankenheim suggests that pectolite is a tremolite with part of the lime replaced by soda. Still farther examination is required before the identity of the Bergen Hill mineral with pectolite can be considered as established.

The important subject of polarization of light is very briefly treated by Mr. Alger, and is scarcely mentioned by Dana and Shepard. It may be said, that as a branch of optics it has only remote relations to mineralogy, and ought to be reserved for the class room of the professor of natural philosophy. To say nothing of the elegant and attractive nature of the subject, and the ease with which some of the more striking phenomena are made conspicuous to a class of mineralogical students, we cannot deny that it has a most important bearing on the determination of mineralogical species. By its means we often derive important aid in determining the system of crsytallization to which a mineral belongs, and in distinguishing species that otherwise might be thought identical.

A single instance may serve to illustrate our remarks. The species mica, as is well known, has been divided into hexagonal and oblique or common mica, belonging to different systems of crystallization. This division, which was based on crystallographic grounds, has been confirmed by the optical properties of the two minerals. But further, the hexagonal mica of Henderson, N. Y. has been found to have the optical relations of binaxial mica, and therefore, instead of having a hexagonal primary like other hexagonal mica, its primary is a right rhombic prism, and it is hence entitled to rank as a distinct species.*

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* Mr. Alger has so placed it as *magnesian mica*, from the large proportion of this earth found in it by Meitzendorf, (Pogg. LVIII, 157, 1843.) The same name has been given to hexagonal mica, as magnesia is its characteristic ingredient. Dana proposes the less objectionable name of *rhombic mica.*

This discovery sets in a strong light the importance of making the optical properties of minerals, the subject of inquiry in deciding on identity, when from the nature of the case the evidence can be obtained.

We regret that this subject has been so summarily treated in these three works.

We might easily extend these remarks, for many things of minor importance are passed over without notice. Our object is not to undervalue Mr. Alger's labors, which we esteem highly, and we take pleasure in assuring the student of mineralogy, that he cannot open the book without finding much to reward him for his perusal. The adoption of false and antiquated principles of classification, leading to absurdity in some cases, and the want of a discriminating courage in the rejection of bad and doubtful species, are its most prominent faults.

Had Mr. Phillips lived to this day, he would not we are sure have retained the divisions of the science which were current in England when he wrote, and although we will not pretend to say that he would have followed either Mohs or Necker, or any other modern author, it is certain he would not have closed his eyes to the beautiful natural relations of species, which have been pointed out by the progress of mineralogical science during the last ten years.

But we must hasten to say a few words relative to the "Treatise" by Prof. Shepard. This is a second edition of the first part of the treatise by the same author, published in 1832, with some additional information comprised in the table of species. The classes and orders of Mohs are retained, but there is no division into genera.

"While so much uncertainty exists relative to many of the species, and while such different views are likely to prevail respecting the genera, the application of a systematic nomenclature to minerals can scarcely be received with approbation. Indeed it has thus far been met with decided tokens of discouragement, not only, as might have been expected, from learners, but from the more experienced cultivators of the study. An additional reason for its disuse appears to be, that the number of species in the mineral kingdom is so small, that the memory requires less artificial help to retain a distinct conception of the relations of the species, than in the departments of zoology and botany, where the cases are not few in which a single order contains several times as many species as the entire mineral kingdom. It is for this latter reason a matter of doubt, whether a systematic nomenclature, analogous to that in the other departments of natural history, will ever be adopted in mineralogy.

"The trivial names only, therefore, are employed in the present work; and in those few cases where none such have ever been applied to well established species, the attempt has been made for the sake of uniformity, to supply the deficiency. In the descriptive part of the work, the leading synonyms will follow (in smaller type) the trivial names adopted for each species."—p. 94.

It is well known that Prof. Shepard is a disciple of Mohs, in relation to the determinations and arrangement of minerals. Chemical evidence has therefore with him no decisive weight, unless it is corroborative of natural history characters. But happily Prof. S. has not been so far consistent as to banish the blowpipe and the test-case, and he still resorts to analysis in determining the nature of minerals, although in regard to classification, he would supersede its results if they were at war with the characters of natural history. Difference of constitution is with this class of naturalists an accident, with which the mineralogical student has little to do. He knows quartz and diamond by their hardness, gravity, and lustre, rather than by the silica of the former and the carbon of the latter. It is however no valid objection to the chemical method, that in describing a bird or a fish we do not dwell on the carbon, hydrogen, nitrogen, and oxygen, which enter into their composition, or the phosphate of lime which gives stability to their bones; for, there is a wide and irreconcilable distinction between the results of *vital force* in the production of organized forms, and those of *molecular attraction* which govern the characters of crystallized minerals. The one is *organic* and *vital;* the other *purely chemical.* On this difference are founded two great systems of natural knowledge, and any attempt to overlook it or undervalue its importance, must lead to error.

Gravitation is not more essentially connected with astronomy, than chemistry with mineralogy; and the student who is ignorant of the composition of his minerals, might with equal profit possess a cabinet of glass of divers hues. It is said that many blind persons have a delicacy of touch, which enables them to distinguish, not form only, but colors, and that they may even appreciate the beauty of a picture, yet we see in this fact no argument for the rejection of vision in estimating the value of works of art.

Chemical characters alone are perhaps as insufficient as characters purely physical, in the determination of minerals. Were all minerals distinctly crystallized, the naturalist would have a strong argument in support of his views, rendered doubly forcible by the principles of isormorphism and dimorphism.

To the chemist, rutile and anatase, Sillimanite and kyanite, white iron and iron pyrites, carbonate of lime and arragonite, garnet and idocrase, are identical substances. But the laws of dimorphism (the development of which the mineralogist owes as much to chemistry as to physics) establish them as distinct. We have already expressed the opinion, that a new method was in store for mineralogy, which should combine all that was essential and truly important, both in chemistry and natural history, without a blind adherence to either, and the results of every year seem to add new strength to the opinion.

We do not wish to be considered as criticising the arrangement of species in the table contained in Prof. Shepard's little volume, which is in the main unexceptionable. Our remarks are aimed rather at the exclusive adherence to a so called natural history method on the one hand, and the neglect of chemical evidence on the other. The volume before us bears evidence of the errors to which the former must of course lead, and also to the impossibility of a consistent adoption of the latter principle.

The introductory chapters of the book will be found clear and useful to the general student, but the tables for determination are too brief to enable the learner, who is wholly unacquainted with the science, to determine a species with facility, and by no means supply the want of full descriptions and figures, and yet we infer that the small volume is intended to go alone as a complete treatise. Take an example of the mode of arrangement of characters.

		H.	G.			
*13.	*Apatite*,	5·0	3·2	Hex. prs., mas. gran.	L. vit.	C. various.

(Splendid xls. at Hammond, St. Lawrence Co., N. Y.)

This is all the information which the author thinks necessary to enable the *learner* to determine this species.

In carrying out his views of having a trivial name for every species, Prof. Shepard has found it necessary to drop several old and well established names, chiefly because they convey a notion of the chemical constitution of the mineral. We observe the following changes of this sort. Oxacalcite for oxalate of lime, of

Brooke, (the expurgation of chemistry in this case, is quite incomplete,) kraurite for green iron ore, Selbite for carbonate of silver, Beresofite for red lead ore or chromate of lead, Carinthite for yellow lead ore or molybdate of lead, &c. We think an author who feels compelled in justice to Count Bournon, to revive the obsolete trivial name of *fibrolite* and substitute it for the well established and universal *Bucholzite*, should *hesitate* before rejecting other original and unexceptionable terms for the mere "sake of uniformity."

A glance at the list of species will enable us to show how the principles adopted by the author work in practice. If the science of chemistry had never been heard of, we think this list might have been somewhat shorter even than now.

Quincite. This earthy hydrous silicate of magnesia is here retained as a species, although even on *chemical* grounds it is hardly distinct from Meerschaum.

Lincolnite. This species, which was proposed by Prof. Hitchcock, has been generally considered as only Heulandite. Mr. Alger has written a very satisfactory paper on this subject in Vol. XLVI, p. 235, this Journal; the specimens on which Mr. Alger's opinion was founded and which were measured by Mr. Teschemacher with the reflecting goniometer, were received from Prof. Hitchcock himself, and others were obtained from the State collection, where they were deposited by Prof. H. Mr. Shepard cannot object to this conclusion, founded solely on crystallographic characters.

Fibrolite. Under this name Prof. Shepard includes the species Bucholzite and Sillimanite. We have already presented (Vol. XLVI, p. 382) the chemical evidence which in our opinion would be sufficient to *unite* kyanite and Sillimanite, (as Mr. Connell had previously proposed,) but the crystallographic evidence for their *separation* is as good as in the case of Arragonite and calcareous spar. Prof. Shepard says, (p. 138,) "The American Bucholzites belong to Sillimanite; nor have we any valid ground for maintaining Sillimanite distinct from fibrolite," &c. As the question must be decided, as far as it concerns our author, on *purely natural history grounds*, without regard to *any chemical evidence*, (however cogent that may seem to others,) we would only say, that until the diagonal cleavage and distinct crystalline form of Sillimanite (a figure of which we have given l. c.) are shown to be identical with the fibrolite of Bournon, or the Bucholzite of others, we must maintain its claim to rank as a distinct species.

Danburite. We are at a loss to decide whether this species is proposed by the author on chemical or natural history grounds; if on the former, the evidence is inadmissible by the principle laid down; if by the latter, it requires further investigation, as the result of Mr. Shepard's analysis is far from satisfactory.

Goshenite. This is a new name for the same mineral which the author has before described (this Journal, Vol. xxxiv, p. 329) as phenakite, and which he afterward (Vol. xliii, p. 364) recalled. It was first noticed by Col. Gibbs in this Journal, as a "beautiful rose emerald," (Vol. I, p. 351;) it is also white and bluish. Prof. Shepard considers it as belonging to the rhombohedral system, and its planes seem to render it probable that the inference is correct. Without more perfect crystals we cannot consider it distinct until an analysis shall decide its composition.

Ledererite. The sphene of Grenville, Canada, and Hammond, N. Y. is still retained under this name. The greatest confusion exists in most of the books in relation to the figures of this mineral, but a comparison of the crystals from Hammond with figure 229 of Mohs's Mineralogy, (2d ed.) will convince the most skeptical that the identity is complete. The figure given by Prof. Shepard in this Journal (Vol. xxxix, p. 357) will be seen to correspond with the figure of Mohs, only that it is inverted and the planes differently lettered.* The slight discrepancy of the angles (about 1°) is no doubt due to the imperfection of the specimens measured, forbidding the use of the reflective goniometer and rendering the common instrument more uncertain than usual. We believe Prof. Shepard is alone in maintaining this mineral as a distinct species.

Washingtonite. This name for a variety of axotomous iron should not be retained, as the mineral is identical in composition with the hystatite of Breithaupt.

Chathamite. Here we have a new species proposed, on as far as we can see, *purely chemical grounds*, and these we conceive will be found untenable. But we copy the description, (p. 158.)

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* Or see this Journal, Vol. xlvi, p. 136, where this figure is placed in its normal position.

"*Chathamite.* H. 5·5. G. 6·226. Mas. fine gran. Fract. uneven. C. tin-white to steel-gray; rarely tarnished; bluish gray. Streak grayish-black. (Lo. Chatham, Ct. cobalt-mine. Before the blowpipe emits fumes of arsenic, and with borax gives a blue bead. Arsenic, 70; iron, 17·70; nickel, 12·16; cobalt from 1·4 to 1·3 p. c.)"

In this Journal, Vol. XXIX, p. 242, Mr. Booth has given an analysis of an ore of nickel and cobalt, from Riechelsdorf, which gave, nickel, 20·74; cobalt, 3·37; iron, 3·25; arsenic, 72·64, from which he deduces the formula, $(Ni,Co,Fe)As^2$. Now it is well known that nickel, cobalt and iron may replace each other in any proportion, and hence the hypothetical formula RAs^2 may be used to express the composition of *white nickel*, which is undoubtedly the ore analyzed by Prof. Shepard, and here called *Chathamite.*

From these few notes it will be seen how difficult it is to follow out consistently the principles espoused by Prof. Shepard, and we regret this the more, because few men in this country have cultivated mineralogy with equal zeal, activity and success. American mineralogy, in particular, is greatly indebted to him for many personal explorations made with much labor and a wide range of travelling. His tact in bringing out, fitting up, and arranging American minerals is unrivalled; but we cannot discern any valid reason why we should insist on keeping closed one half the shutters of an ill-lighted apartment, admitting the rays only through the leaded diamond-glass of a single Gothic window.

Yale College Laboratory, August 1, 1844.

Art. XI.—*Discovery of the Yttro-Cerite in Massachusetts;* by Prof. Edward Hitchcock, LL. D.

(Read to the Association of American Geologists and Naturalists at Albany, April, 1843.)

In looking over recently some neglected duplicates of rocks and minerals collected many years ago in Massachusetts, my eye accidentally fell upon one exactly resembling specimens of yttrocerite, which I had received from Sweden. It occurs in thin veins running through granitic gneiss, and presents the peculiar and striking purple color of the Swedish mineral. It corresponds also in hardness and in structure; and I have little hesitation in pronouncing them identical from these characters alone. Unfortunately, however, the label is lost; and my memory does not serve me to recall the locality, except that I have an impression that it was found in Worcester County, and the specimen corresponds with some of the rock there. I notice also in it fragments of pyrope, which occurs in that county. I have an indistinct recollection of throwing it aside as a poor specimen of purple fluate of lime; not being then familiar with the yttro-cerite. I place the specimen from Massachusetts and one from Sweden on the table, that the members of the Association may compare them.

I have made a few chemical trials to ascertain whether this mineral contains hydrofluoric acid, cerium, and yttria, and now give the results.

The powder was moistened with concentrated sulphuric acid in a platinum crucible, covered with a glass plate, and subjected to a moderate heat, when the glass was decidedly corroded by the hydrofluoric acid.

Before the common blowpipe the mineral melted into a dark greenish glass, without decrepitation. With borax, its powder formed an orange yellow bead when hot, which, on cooling, lost nearly all its color, passing through one or two shades of yellowish green.

In nitric and hydrochloric acid, the mineral was more or less soluble by long digestion and the application of heat. A solution in nitro-muriatic acid exhibited the following effects with reagents.

Potassa gave a white abundant precipitate, insoluble in excess.

Carbonate of potassa gave a white abundant precipitate, slightly soluble in excess.

Bicarbonate of potassa gave a similar precipitate, slightly soluble in excess.

Ammonia gave a white abundant precipitate, soluble in excess.

Carbonate of ammonia gave a like precipitate, nearly all soluble in excess.

Red prussiate of potassa gave a yellow abundant precipitate, insoluble in hydrochloric or nitric acid.

Yellow prussiate of potassa gave an abundant blue precipitate, insoluble in hydrochloric and nitric acid.

Hydrosulphate of ammonia gave a gray precipitate, which became darker on standing.

Oxalic acid gave a white precipitate, insoluble in hydrochloric acid.

Phosphate of soda gave a white abundant precipitate, soluble in nitric acid.

It is well known that the above reagents produce almost precisely the same effect upon cerium and yttria, and the results correspond with their behavior in solutions of these substances, as given in the late work of Parnell on Chemical Analysis, except that the precipitate by ammonia is insoluble in excess, instead of being soluble; that by hydrosulphate of ammonia is gray, instead of white; that by oxalic acid is insoluble, instead of being soluble in hydrochloric acid; that by yellow prussiate of potash is yellow, instead of white, and there is a blue precipitate by the red prussiate of potash, whereas neither cerium nor yttria produces any. There can hardly be a doubt, however, that this last result proceeds from the presence of iron; and perhaps the other deviations from the usual action of the reagents on cerium and yttria, may be explained by the presence of foreign ingredients, as it was impossible to separate the mineral entirely from its gangue. It may be too, that the same cause rendered the mineral fusible, although when pure it is infusible *per se*. Upon the whole, too many characters correspond to the yttro-cerite to allow us to refer it to any other species; and yet the facts that have been stated, throw some doubt over this conclusion. But as the existence of this mineral in New England is a matter of great interest, I have thought the specimen deserved this brief communication.*

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* Dr. C. T. Jackson has analyzed this mineral since the above paper was written, and found it to correspond essentially with the Swedish yttro-cerite. Its composition is as follows:

Lime,	0·347
Yttria,	0·155
Oxides of cerium and lanthanium,	0·133
Alumina and oxide of iron,	0·065
Silex and silicate of cerium,	0·106
Fluorine,	0·194
	1·000

Proceedings Bost. Nat. Hist. Soc. 1844, p. 166.

Art. XII.—*Review of the New York Geological Reports.*

(Continued from Vol. xlvi, p. 157.)

New York System.—In the first part of the review of the New York Reports, it has been shewn that the older palæozoic or protozoic rocks of Murchison form the characteristic geological features of the state of New York, and hence they are designated the New York system by the state geologists. These, therefore, especially demand, and will now receive, a more detailed notice.

As a vast multitude of facts have been accumulated by the able individuals who have prosecuted this survey during the last seven years, we can of course only cull from these, those formations which, either from their geographical extent, economical importance, or peculiar geological interest, will occupy most of our attention, while the minor, partial formations can receive only a passing glance.

The following list presents, in a tabular form, the divisions and subdivisions of all the rocks embraced in the New York system:

Geographical subdivisions.	*Systematic subdivisions, founded upon the fossil and lithological characters.*	
Champlain Division.	1. Potsdam sandstone.	
	2. Calciferous sandrock.	
	3. Black River limestone group, embracing the Chazy and Birdseye.	
	4. Trenton limestone.	
	5. Utica slate.	
	6. Hudson River group.	
	7. Grey sandstone.	
	8. Oneida or Shawangunk conglomerate.	
Ontario Division.	9. Medina group.	
	10. Clinton group.	
	11. Niagara group, including shale and limestone.	
Helderberg Series.	12. Onondaga-salt group.	
	13. Water-lime group.	
	14. Pentamerus limestone.	
	15. Delthyris shaly limestone.	
	16. Encrinal limestone.	
	17. Upper Pentamerus limestone.	
	18. Oriskany sandstone.	
	19. Cauda-galli grit.	
	20. Schoharie grit.	
	21. Onondaga limestone.	
	22. Corniferous limestone.	
Erie Division.	23. Marcellus slate.	
	24. Hamilton group,	Moscow shales. Encrinal limestone. Ludlowville shales.
	25. Tully limestone.	
	26. Genesee slate.	
	27. Portage or Nunda group,	Portage sandstone. Gardeau flagstones. Cashaqua shale.
	28. Chemung group.	

With regard to the geographical divisions of this table, they are to be regarded rather as grouping for local convenience than as a natural classification founded upon the distribution of fossils.

The time has, perhaps, hardly arrived for the construction of a perfect chronological palæontological table, but it is probable that it would recognize seven divisions, or rather three principal divisions, with intervening transition series, thus:

PROTOZOIC ROCKS, OR NEW YORK SYSTEM.

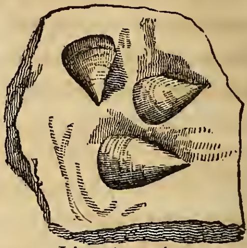
Transition Series.	1. Potsdam sandstone.
FIRST OR LOWER DIVISION.	2. Calciferous sandrock. 3. Black River limestone. 4. Trenton limestone. 5. Utica slate. 6. Hudson River group.
Transition Series.	7. Oneida conglomerate. 8. Medina sandstone. 9. Clinton group.
SECOND OR MIDDLE DIVISION.	10. Niagara group. 11. Onondaga salt group. 12. Water limestone. 13. Pentamerus limestone, and Catskill shaly limestone. 14. Oriskany sandstone.
Transition Series.	15. Caudigalli and Schoharie grits. 16. Onondaga limestone. 17. Corniferous limestone.
THIRD OR UPPER DIVISION.	18. Marcellus slate. 19. Hamilton group. 20. Genesee slate. 21. Portage group.
Transition Series.	22. Chemung group.

Taking into account the persistency of leading formations throughout the United States, the uniformity of condition, exclusiveness and peculiar character of species in the strata embraced in the three principal divisions; and at the same time considering the doubtful position which the transition series holds in the system, and the apparent connecting links which they form between the lower and upper rocks, this seems to us, at present, the most consistent and satisfactory classification of these American palæozoic rocks.

We proceed with the description of the minor divisions in the ascending order.

Potsdam Sandstone.—(No. 1 of Pennsylvania and Virginia Reports.) This rock, the base of the New York system, and the connecting link between the non-fossiliferous rocks below and the fossiliferous above, is interesting as being the oldest rock containing organic remains at present known in this country, and as making us acquainted with the earliest forms of organic existence to which geological science can point back.

Plate 68, p. 268, Emmons's Report.



Lingula antiqua.

Here is a representation of one of the lowest and oldest fossils now known in this country. And what is worthy of note, though a *species* peculiar to the Potsdam sandstone, it belongs to a *genus* which has survived all the changes upon the earth, and, as Mr. Conrad justly remarks, has lived through all ages of organic existence; even at this day, it is an inhabitant of the ocean.

At Birmingham in Essex Co., N. Y., at a place called the High Bridge, this *Lingula* is abundant, though obscure, in the Potsdam sandstone ranging through the strata to the depth of seventy feet. There is another fossil in this rock between Wilna and the Natural Bridge, in Jefferson Co., N. Y., which Prof. E. says resembles the so-called *Fucoides demissus*, but no figure is given of it.

The lower part of this rock is usually a conglomerate containing sometimes, as Dr. E. informs us, masses of quartz as large as a peck measure. The upper part is usually a white friable sandstone, a yellowish brown compact sandstone or a hard quartz rock. The entire thickness of the mass on the Canada slope is three hundred feet, and the belt of country which it occupies averages about fifteen miles.

At De Kalb in St. Lawrence Co., the Potsdam sandstone is elevated by disturbing forces acting from beneath, and "seems also to have been subjected to lateral pressure, by which the strata are folded around each other;" but in general this rock is even bedded. It rests directly either on gneiss or granite, and borders the hard crystalline rocks on the N. and N. W. along the line of road leading from Ogdensburg on the St. Lawrence, to Plattsburg on Lake Champlain. The most interesting locality for the geologist to examine it, is on the Au Sable River, near Birmingham and Keesville, where he can observe its characters in fossils, and contemplate its stratification in frightful gorges.

Its western equivalent is not yet fully determined. Along the Wisconsin River, towards its mouth, a sandstone is visible closely resembling the Potsdam sandstone of New York in external aspect; and since it crops out three hundred feet below beds of fossiliferous limestone, known to be the equivalent of the Trenton limestone of New York, there is a great probability of the identity of the two rocks; still the palæontological evidence is wanting, since the Wisconsin sandstone has not yet yielded any fossils. From the reports on Michigan this formation seems to exist on Lake Superior.

At its junction with the overlying calciferous sandrock the Potsdam sandstone puts on a variety of aspects; sometimes it is a calcareous breccia, sometimes a dark brown iron mass, in hand specimens resembling a graywacke; and again a dark slaty sandstone with impressions of fucoids. "At the Falls of Montmorenci, this rock is stained with carbonate of copper, which gives it the aspect of one of the varieties of new red sandstone." (Emmons's Report, p. 103.)

The typical mass, which gives name to the rock, occurs at the Potsdam quarries, on the De Grasse River, St. Lawrence Co.—They furnish the most valuable building stone in the state; indeed, Prof. E. maintains that few materials could compete with it, if situated near a market, on account of its being so perfectly workable and manageable, and at the same time so even-bedded.

Calciferous Sandrock.—(No. 2 of Pennsylvania and Virginia Reports.) The predominating rock of this formation is, as the name denotes, a sandy limestone. It has usually a fine crystalline structure, intermixed with earthy matter and small masses of of calcareous spar. It consists, according to Emmons, of the following divisions: Fucoidal layers, calciferous sandrock, drab-colored layers, or water limestone, cherty beds, geodiferous strata filled with a species of Orthis, encrinital beds, mass containing Bellerophon and other univalves, oolitic layers; in all attaining a thickness of between two hundred and fifty and three hundred feet. Sometimes the entire mass is absent.

Fossils are both rare and obscure in this formation. Those which have been observed and are confined to this rock are represented on the succeeding page.

Plate 2, fig. 1, *Ophileta levata.* Fig. 2, *O. complanata.* Fig. 3, plate of the head of an *Encrinite*, very abundant in the upper part of the calciferous sandrock at Chazy. Fig. 4, *Orthoceras primigenium.*

Plate 2, p. 36, Vanuxem's Report.

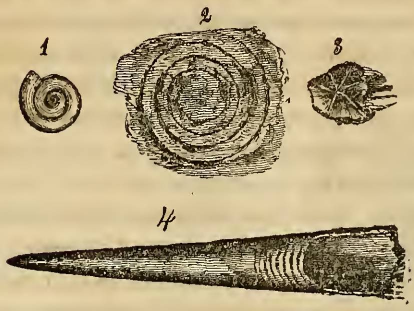
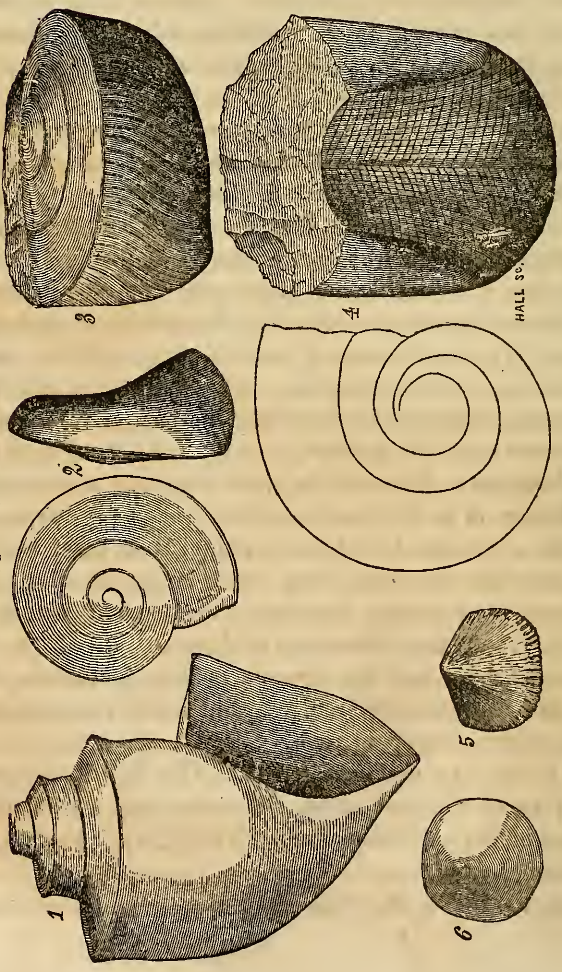


Plate 84, p. 312, Emmons's Report.



Besides these, there have been found in the calciferous sandrock, *Lingula acuminata*, *Pleurotomaria*, *Scalites angulatus*, (Pl. 84, fig. 1,) *Maclurea labiatus* (fig. 2,) *M. striatus* (fig. 3,) *Bellerophon sulcatinus* (fig. 4,) *Orthis* (fig. 5,) and *Orbicula*, (fig. 6.)*

The best localities for studying this rock and its fossils are in the valley of the Mohawk near Canajoharie, Fort Plain, and at Chazy, Clinton Co.

On the south, the calciferous sandrock ranges from the termination of the lakes bounding New York and Vermont to East Canada creek; its outcrop being chiefly north of the Mohawk, and resting immediately on the primary, concealed however to a considerable extent, by alluvion. West of this, in Herkimer Co., it shows itself, on the surface, only over a very limited area on the margin of W. Canada creek. Further west it disappears until it emerges again to the surface between New Hartford and Whitestown in Oneida Co., at first in a very narrow strip, but after reaching Fish Creek its superficial area suddenly expands in a sweep towards the north, and then, contracting again, it reaches the southeastern shore of Lake Superior. On the north it rests on the Potsdam sandstone, and is coextensive with that formation, forming a belt along the St. Lawrence ten miles wide, and extending thence into Canada.

Anthracite in the form of drops occurs occasionally in the calciferous sandrock, and excavations have been made near the Noses to the depth of sixty feet, in the hopes of striking a workable bed, without success. So long as anthracite coal was considered a product of more ancient date than bituminous coal, there might have been some encouragement given to such a work; but now that geological research has demonstrated the fact that both these varieties of coal belong to one and the same geological epoch, it is certain that all attempts to discover anthracite associated with this rock, or indeed with any of the members of the New York system, must be equally fruitless with the explorations made for bituminous coal within the same geological limits.

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* The original figures illustrating the subject of this paper having been kindly loaned to us, and some of them being in groups, we have been obliged to copy a considerable number of figures not referred to in this review, because they could not be detached from the tablets in which they are contained without considerable trouble and expense. Moreover, they may be of some interest, being additional figures of the same deposit.

Here we have a striking example of one of the grand negative truths of geological science—one, the vast practical utility of which, because of a passive character, and because it merely furnishes experience to *save* the expenditure of immense sums of money and labor, without amassing wealth, is but little appreciated.

West of Little Falls some fine cabinet specimens of copper pyrites have been procured in blasting the rock for the coal. It yields the finest quartz crystals known. Sulphate of barytes is also found in it at the west end of Little Falls. Many of the warm springs of the United States have their origin, according to Vanuxem, at the bottom of the calciferous sandrock.

This formation seems to have a great range, extending in a N. W. and S. W. direction through the Atlantic states.

Black River Limestone, (No. 2 of Pennsylvania and Virginia Reports,) including the Chazy, Birdseye, and Mohawk limestone, viz. those rocks which form cliffs on Black River from its head to its mouth.

The lower part, or Chazy limestone of Emmons, is a dark, thick-bedded limestone, from thirty to one hundred feet thick, characterized by the *Maclurea*, pl. 73, fig. 1, closely allied to Euomphalus, a *Trochus*, and *Columnaria sulcata*, (fig. 2.)

Above this lies a dove-colored limestone about ten feet thick, every where distinguished by its peculiar fossil—the so-called *Fucoides demissus*, (fig. 4, next page,) now considered by Dr. E. to belong to the Polyparia, on account of its internal structure and the perforation of the outside of the hanging stem.

The tail of a trilobite (Pl. 73, fig. 3) was also found in the Birdseye limestone; also *Orthoceras multicameratum* and *Ellipsolites ?*

The Birdseye limestone passes upwards into a dark limestone, which affords at Isle La Motte a beautiful marble.

On the Mohawk the upper layers, or transition to the Trenton limestone, have an interlamination of shale, and contain the remains of large *Orthoceras*, the *Actinoceras* of Bigsby and *Diploceras* of Conrad ; also *Strophomena alternata* and *Cyathophyllum* like *ceratites*, *Columnaria sulcata.*

The middle portion, or Birdseye limestone, is the most constant in its character, and affords generally a fine rock for construction and the manufacture of pure lime.

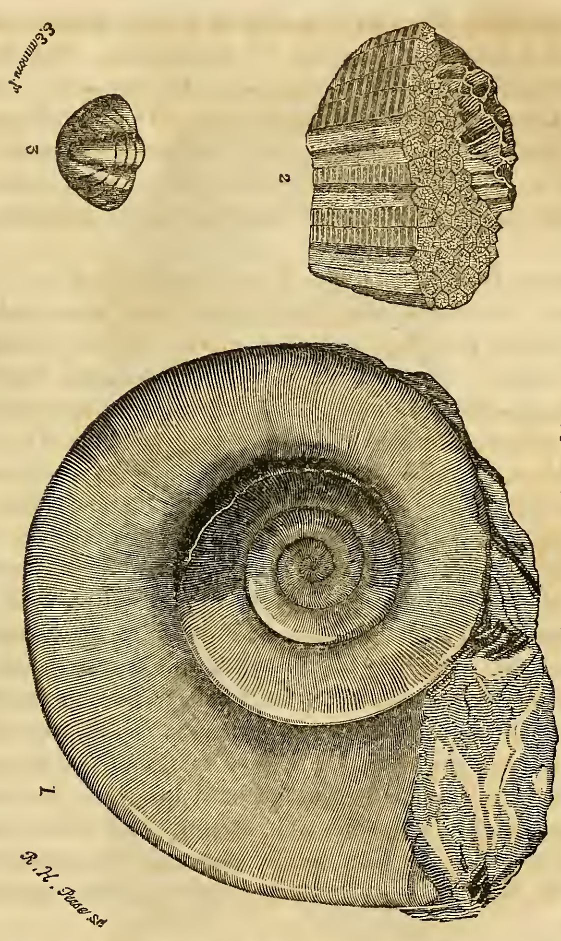


Plate 73, p. 276, Emmons's Report.

4 Plate 3, p. 39, Vanuxem's Report.

At Frankfort, Kentucky, a rock of a very similar aspect occurs and is quarried extensively for tombstones. Mr. Vanuxem considers it identical with the Birdseye limestone of New York.

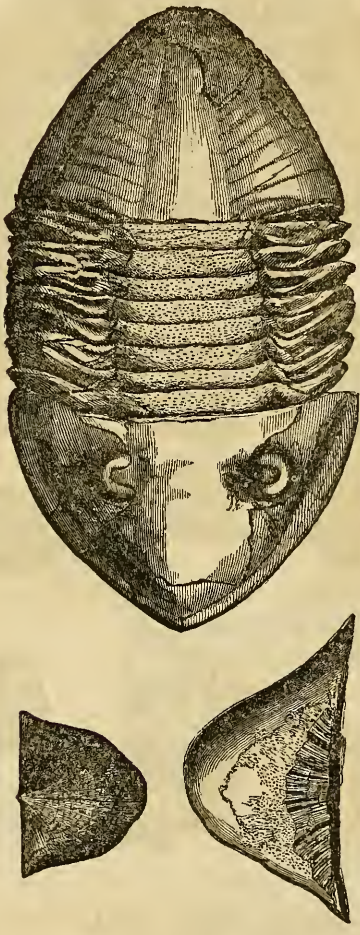
The lower magnesian limestone of the Wisconsin River, interposed between the sandstone and fossiliferous limestone already referred to, and containing oolitic layers, is most probably the equivalent of the Black River limestone or the calciferous sand-rock.

Trenton Limestone.—(No. 2 of Pennsylvania and Virginia Reports.) No rock of the New York system seems to be better characterized than this, and none affords a richer harvest to the palæontologist. The inferior rocks, we have seen, furnish occasionally organic remains, but they are few and far between, and for the most part obscure; but here the catacombs of an entombed world of animated existence suddenly bursts upon our view; not mutilated, dispersed and unintelligible, as one might expect, considering the date and condition of the deposite,—but standing out in bold relief, even detached and complete, with the minutest details of organization preserved in all the perfection of life, inviting us to the contemplation and study of the anatomy and physiology of races, the aborigines of this terrestrial globe, and leading us thence to deduce inferences regarding the condition of our earth at this remote palæozoic period. The western extension of this and the overlying beds of the Champlain divisions, are indeed but a calcareous aggregation of Mollusca and Polyparia, and have furnished to the cabinets of the curious a greater variety of fossil specimens, and more prolific subject for the research of the naturalist, than any of the superincumbent, more recent formations.

The Trenton limestone in New York is, for the most part, a dark or black fine-grained limestone in thin layers, separated by beds of black slate. It passes also into a grey, crystalline limestone, particularly the upper beds; and then affords a good material for architectural purposes. The black variety is usually too shaly to be a durable building rock, but where it is free from argillaceous matter, as in some localities, it is compact, and then susceptible of a good polish.

The organic remains peculiar to this formation, which have the widest range, are here represented.

1. *Isotelus gigas.* 2. *Strophomena deltoidea.* 3. *Favosites Lycoperdon.*



The same or very analogous species as figures 1, 2, and perhaps 3, occur in the lower part of the blue limestone formation of the West.

Plate 100, fig. 2. *Calymene senaria.* 6. *Ceraurus pleurexanthemus.* 7. *Trinucleus tessellatus?*

Fig. 2, closely resembles the Dudley trilobite, *Calymene Blumenbachii*, but supposed to be distinct from it and described by Mr. Conrad under the name of *C. senaria;* it is one of the most abundant fossils of this rock. Bucklers and post-abdomens of it

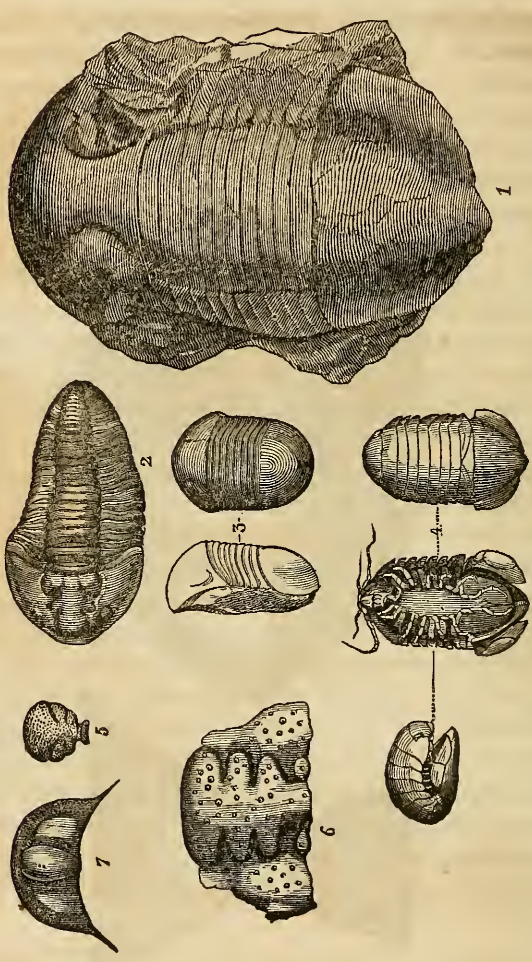


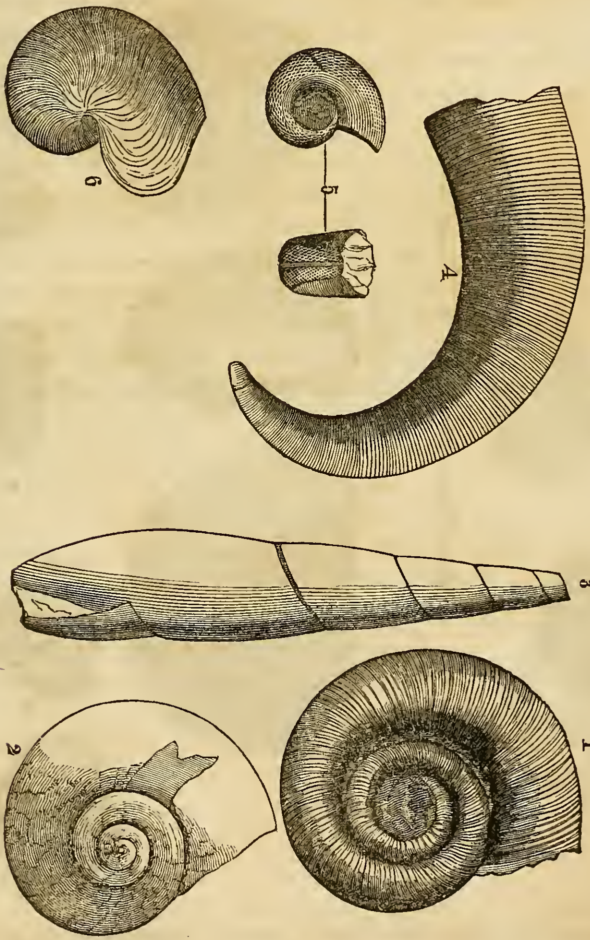
Plate 100, p. 390, Emmons's Report.

are especially numerous. Dr. E. thinks it is confined to the Trenton limestone. A trilobite occurs in the West so like this, that it is hardly possible to distinguish them; it is found at Cincinnati, Ohio, and also at Madison, Indiana, even three hundred or four hundred feet above the Graptolite beds and strata containing *Triarthus Beckii.* If it really is the same species, it holds a higher position or has a greater range than in New York.

Fig. 7, is also exceedingly abundant, particularly near Glen's Falls. We think it somewhat doubtful whether it be the true *tessellatus.* The western tessellated trilobite, found near high water of the Ohio, opposite Cincinnati, has been referred to the same species; it is however most probable that it is distinct both from this and the *T. caractaci.*

Fig. 6 is not so common as the preceding, but it evidently has a wide range, as it has been found in Wisconsin on Sec. 5, T. 5, N. R. 1 W. of 4th P. M.

These are the univalves of the Trenton limestone most worthy of note, on account of their wide range.



[Plate 101, p. 392, Emmons's Report.

Plate 101, fig. 2, *Pleurotomaria lenticularis.* 3, *Subulites elongata.* 6, *Bellerophon bilobatus.*

Fig. 3 occurs also in the hills at Cincinnati. Fig. 6 is, to all appearance, the same as the Caradoc fossil of the same name represented in Murchison's Silurian System. If so, it is one of the few species whose absolute identity on both sides of the Atlantic can be established.

A Pleurotomaria, considered the same as fig. 2, has also been found both in Iowa and Ohio. Here are some of the bivalves of the Trenton limestone.

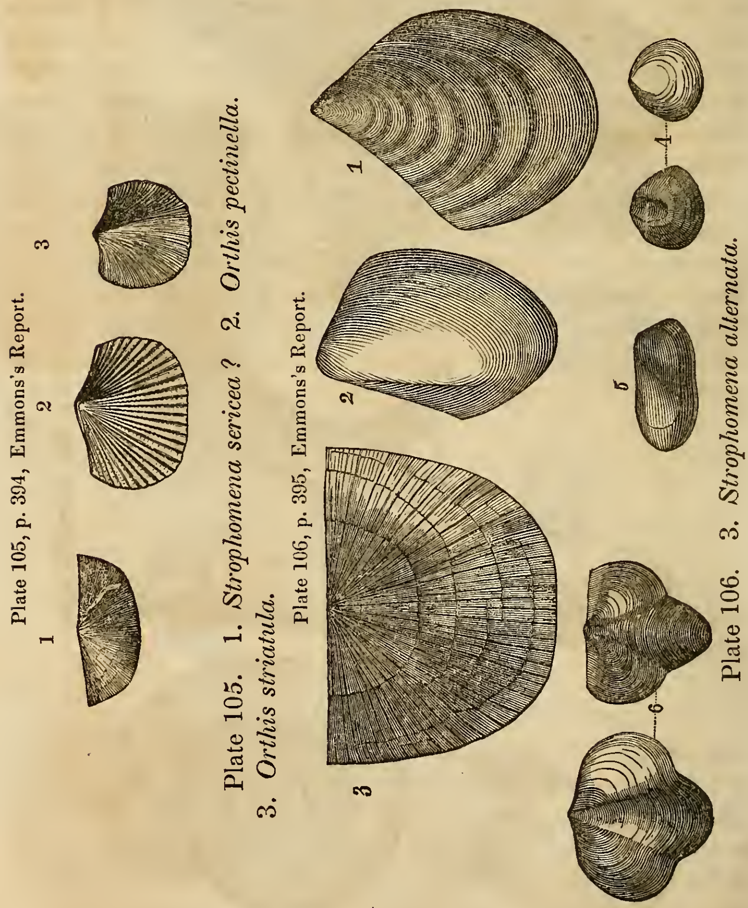


Plate 105, p. 394, Emmons's Report.

Plate 105. 1. *Strophomena sericea ?* 2. *Orthis pectinella.* 3. *Orthis striatula.*

Plate 106, p. 395, Emmons's Report.

Plate 106. 3. *Strophomena alternata.*

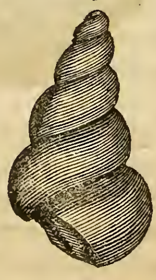
The true *S. sericea* is a Caradoc fossil; if this be identical with it, then there is another point of specific identity established between the American and English fossils. A fossil closely allied to this, occurs in abundance near high water of the Ohio at Cincinnati. *Orthis pectinella* abounds, according to Dr. E. in the Trenton limestone of Jefferson County. This fossil differs only in size and the number of ribs from one found in the hills at Cincinnati. It is also closely allied to *O. callactis* of the Caradoc of England ; the interval between the ribs is greater and the ribs fewer in the *callactis* than the *pectinella.*

The *Orthis striatula* cannot be distinguished from a small and delicately ribbed shell found in great numbers in the lower part of the hills of Cincinnati. Dr. E. informs us that it is as constant at all the localities of the Trenton rock, which have been examined, as any fossil hitherto observed. Both the western and eastern fossil are hardly distinguishable from the *O. canalis* of the Caradoc.

It is possible that several species have been confounded under the name of the *Strophomena alternata :* as Dr. E. remarks, alternate fine and coarse markings are possessed by more than one species. A vast number of individuals, embracing four or five distinct forms, are found in the hills on the Ohio between Cincinnati and Madison, which have more or less of this character. Several of the same occur also in Iowa and Wisconsin.

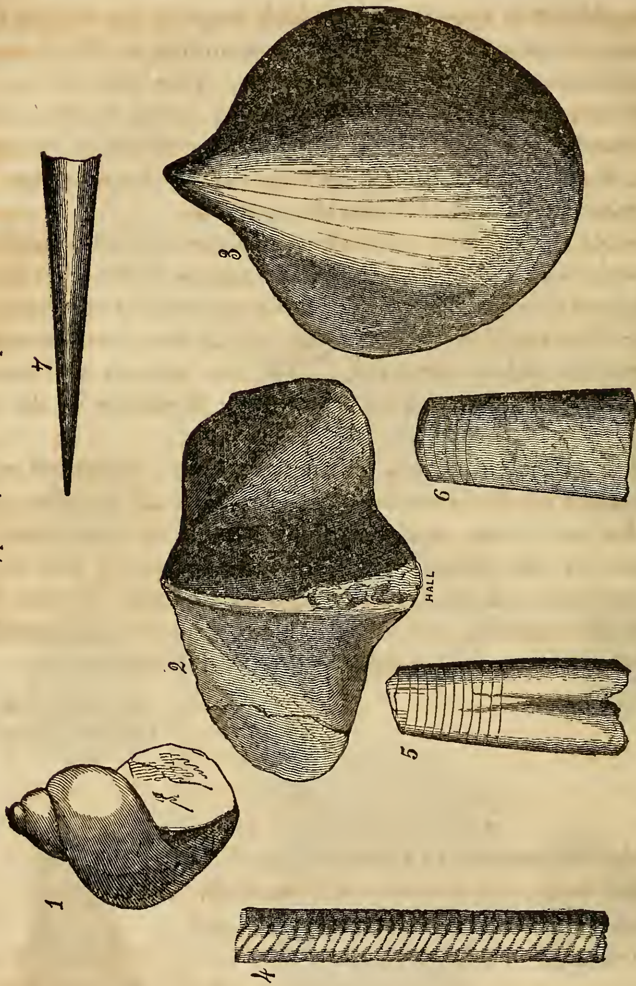
Plate 107, p. 396,
Emmons's Report.

6



This *Pleurotomaria* (Plate 107, fig. 6) is common in the grey limestone at Watertown. It is no doubt identical with a species found at Cincinnati.

Plate 109, p. 397, Emmons's Report.



1. *Pleurotomaria.* 2. *Delthyris expansus.*

Casts of a *Pleurotomaria* rather more slender and elongated than figure 1, occur in the lead-bearing rock of Iowa. The *Delthyris expansus*, (fig. 2,) appears identical with a species found at Eagle Point, Iowa.

The foregoing figures of the Trenton limestone, have been selected from their close analogy to western species.

Of the remaining fossils of this formation we can only give a list. *Isotelus planus*, *Bumastis Trentonensis*, (fig. 1, p. 364,) *Calymene* ——; *Illænus Trentonensis*, (fig. 3, p. 364,) *Trocholites* *ammonius*, (fig. 1, p. 365,) *Inachus undatus*, *Cyrtoceras pilosum*, (fig. 4, p. 365,) *Cameroceras Trentonense*, (fig. 4, p. 368,) *Orthoceras multilineatum*, (fig. 7, p. 368,) *O. Trentonense*, *Bellerophon punctifrons*, (fig. 5, p. 365,) *B. profundus*, *Nucula inflata*, (fig. 2, pl. 106, p. 366,) *N. faba*, (fig. 5, p. 366,) *Pterinea undata*, (fig. 1, p. 366,) *P. orbicularis*, (fig. 3, p. 368,) *Strophomena sericea*, (fig. 1, pl. 105, p. 366,) *Orthis leptænoides*, *Delthyris* —— ? *Atrypa extans*, (fig. 6, p. 366,) *A. bisulcata*. The above are figured and described.

The best localities for studying the Trenton limestone and its fossils, are Trenton Falls on West Canada Creek, Fort Plains on the Mohawk, Watertown on Black River, Glen's Falls on the Hudson River, Plattsburg and Essex near Lake Champlain. Its principal outcrop is along West Canada Creek and the Mohawk, extending from three miles west of Little Falls up the valley of the Mohawk to Boonville, thence down the west and south terrace of Black River to Lake Ontario. It usually rests either on the calciferous sandrock, or Birdseye limestone. In Lewis and Herkimer counties it borders on the primary, but its junction cannot be seen by reason of the diluvium. On the east side of the primary range it appears only in isolated patches, on the west shore of Lake Champlain, Glen's Falls, and a few other places in the valley of the Hudson.

Its greatest thickness is four hundred feet. To the east it thins out and becomes blended with the overlying shales.

Ores of lead and zinc are not uncommon in the east and west joints of the Trenton limestone, hence Eaton's name of metalliferous limerock; but they have not been found in profitable quantities.

From the facts here given regarding the range of the organic remains, it appears evident that the Trenton limestone has its representative both on the Ohio and Mississippi Rivers, but as there is some difference of opinion on this subject amongst geologists, the arguments will, probably, be more fully discussed hereafter.

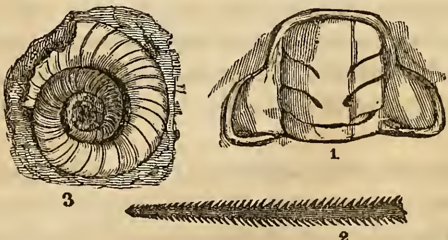
The Trenton limestone has usually been considered equivalent to the Caradoc sandstone of England, but Dr. E. is inclined to regard it as the American representative of the Bala limestone; in proof thereof he cites the following list of fossils from Murchison's work, p. 308: Orthis anomala, *Schlot.*, O. Actoniæ, O. canalis, O. compressa, O. Flabellulum, O. lata, O. Pecten, O. protensa, O. testudinaria, *Dalm*, Bellerophon bilobatus, and Leptæna sericea. If fossils are to be received as evidence, these, says Dr. E., go far to confirm this view.

Now on comparing this list with the fossils of the Trenton limestone given in the New York Reports, we find only two common to the rocks in question, viz. *Leptæna sericea* and *Bellerophon bilobatus*, and closer observation might perhaps even prove these to be distinct from the English fossils of this name. Moreover these very fossils are not peculiar to the Bala limestone, but are also Caradoc and Llandeilo species. Indeed the very list referred to is given by Murchison to show that the Bala limestone cannot be considered as belonging to a system distinct from the Silurian, inasmuch as these species are common also to the lower Silurian, viz. to the Caradoc sandstone and Llandeilo flags. However, Dr. E. founds his principal proof of the geological position of the Trenton limestone on other grounds; inasmuch as it lies beneath rocks equivalent to the Llandeilo flags, viz. the Utica slate. But when we investigate the evidence of this equivalency, we find it rests solely on lithological character.* The truth is, there is not yet a sufficient number of facts before us to enable geologists to decide upon their minuter points of identification.

Here terminate the important limestone deposits of the Champlain division; above are chiefly shales and sandstone.

Utica Slate, (No. 3 of Pennsylvania and Virginia Reports.) Lithologically this deposit is not distinguishable from the black slate intercalated with the Trenton limestone; in fact, the geologists look upon it as a continuation of the same sedimentary action, but distinguish it as a separate formation in consequence of its embracing a few peculiar fossils.

Plate 8, p. 57, Vanuxem's Report.



1. *Triarthus Beckii.* 2. *Graptolites dentatus.*

* Mr. Hall says he has detected a small Lingula in the Utica slate, very similar to one found in the Llandeilo flags.

The *Triarthus Beckii*, one of these, does not seem to differ essentially from a small trilobite found in great abundance in a gray marlite near high water of the Ohio River at Newport, opposite Cincinnati.

The *Graptolites dentatus* is another of these.* At Cincinnati there are two Graptolite strata; one some ten or fifteen feet above high water, and one near the reservoir, some two hundred and twenty-five feet higher, containing probably two distinct species; and all probably differ slightly from the Utica slate species here figured.

Some of the state geologists regard this curious fossil as a marine plant. Mr. Mather in his Report, (p. 393,) speaking of this organic remain of the Utica slate says, "These are mostly plants (Graptolites) of which there are at least five species. One species is serrated on one side, another on both sides like the teeth of a saw. One species is serrated with a long and extremely delicate awn-like appendage, extending from the smaller extremity of the serrated leaf, and one of them is branched." Mr. Conrad recognizes two species as the *Fucoides serra* and *F. dentatus*. Two other species slightly resemble *F. lineatus* and *F. ramulosus.*

And Mr. Vanuxem has the following paragraph regarding them, (p. 63;) "The illustration of this rock by its Graptolites, is therefore left for the geologist of that district. We shall merely state that the ramose nature of two of the species of these singular bodies, found at Alexander's bridge below Schenectady, at Norman's-kill below Albany, and at Hudson City, shows that their origin is vegetable, not animal as conjectured by some naturalists. Their chemical composition confirms their vegetable nature; no animal ever existed whose material was almost entirely carbon, as is the case with these fossils."

These gentlemen view these organic remains in a very different light from all former naturalists who have described them, as will appear from the following extract from Mr. Murchison's work on the Silurian System, (p. 694.)

" *Graptolites.*—These fossils have been alluded to as good tests of the age of the strata in which they occur. It has further been shown that they are usually found in deposits, which from their

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* This fossil has occasionally been found in the slaty part of the Trenton limestone.

structure were well suited to the habits of the family of 'sea pens,' of which they form a genus. They were named *Graptolites* by Linnæus, and have since been partially described under different names, by Wahlenberg, Schlotheim, Hisinger, Nilsson and Bronn. The Danish naturalist Beck, who is preparing a monograph of them, has supplied me with the following sketch. From his remarks it appears that one of the species, very characteristic of the upper Silurian rocks, occurs abundantly in Norway and Sweden. Dr. Beck intended to name this species *Graptolithus virgulatus*, but not yet having printed his monograph, he authorizes me to use any other term, and therefore I adhere to the name of *G. Ludensis*, which was adopted before I received the description of the learned Dane. It does not however, appear certain that there is any real distinction between this fossil of the Ludlow rocks, and the *Prionotus Sagittarius*, Hisinger. The fossil fig. 3, is not adverted to by Dr. Beck. It seems more like *Prionotus foliam*, (Hisinger,) but differs from that species in the number of foliations, and I therefore venture to suggest the name *G. foliaceus*. This species was found in the calcareous flags of Meadowtown, near Shelve, Salop, (Llandeilo flags.) Fig. 4 of the same plate being unknown to Dr. Beck, he has, as above stated, named it after me. The *G. Murchisonii* occurs in the lower Silurian rocks and volcanic grits of the Llandrindod Hills, Radnorshire."

"These pen-like, serrated fossils have a great vertical range in the older or 'protozoic' rocks, being found from the lower part of the Ludlow formation, down to very ancient beds in the Cambrian System, in which they were collected for example, by Prof. Sedgwick in Abereiddy Bay, North Pembroke. They there prove that the lines of slaty cleavage coincide with the original laminæ of deposition, along which these fossils are arranged."

Note on Graptolites; by Dr. Beck.

"*Graptolithus*, Linn., Iter. Scan. Wahlenberg, Hisinger, &c. Esquisse d'un Tab. des Petr. de la Suède, p. 28.

"*Orthoceratites*, Wahlenberg, Schlotheim, Nacht. Pet. I, p. 56 to 58, f. 3.

"*Priodon*, Nilsson, Bronn, Lethæa Geognostica.

"*Lomatoceras*, Bronn, ib. p. 55.

"*Prionotus Nilsson*, Hisinger, Lethæa Suecica, pp. 113, 114.

"Very different opinions have been entertained as to the place which the Graptolites hold in the series of living beings, but that of Prof. Nilsson may come nearest to the truth, who conceives the Graptolite to be a polyparium of the ceratophydian family. Yet I am more inclined to regard them as belonging to the group *Pennatulinæ*, the Linnæan *Virgularia* being the nearest form in the present state of nature to which they may be compared.

"I am now acquainted with six or seven species of Graptolites, all occurring in the oldest fossiliferous strata, where they are associated with Trilobites, Orthoceratites, &c. Of the species above alluded to, five belong to Scandinavia, and of the other two, one is peculiar to Bohemian and the other to French strata. The three specimens given me by Mr. Murchison belong to two species, Nos. 1 and 2 being identical, and agreeing with the Norwegian species, which in my monograph I have named *Graptolithus virgulatus;* but as the memoir is still unpublished, Mr. Murchison may change the name if he thinks it desirable. The species No. 4 is new, and Mr. Murchison's name is adopted."

GENERIC CHARACTERS OF GRAPTOLITHUS.

Class POLYPI.
Order OCTACTINIA (?) Ehrenberg.
Family PENNATULIDES?
Genus GRAPTOLITHUS, Linn.

"*Polyparium indivisum, elongatum, sublineare, acuminatum, obtusiusculum, stata fossili, compressissimum, serratum.*

"*Polypi alternantes cum tubulo communi centrali communicantes, in fossili statu sæpissime secati, rarius bifarii, oblongi, acuminati.*

"When the stem is cut off, the distinct bodies of the single polypes are seen alternating, and showing different forms when cut in different directions.

"In the first edition of his Systema Naturæ, (1736,) Linnæus published a generic group under the name Graptolithus. The first species he described several years afterwards in his travels in Scania, (p. 147,) where also a rude figure is given. This is the most common form of Graptolites in the Scandinavian transition formations, and as described and named first may be taken as the typical form of the genus. When Linnæus introduced specific names, this species of Graptolite was also named for the first time, in the twelfth edition of the Systema Naturæ, Vol. III, p. 174, No. 7, as *G. scalaris.*

"In the last mentioned work, the genus Graptolithus is reproduced; but several fossil bodies, and even inorganic markings and veins in the rocks being united as species under the same generic denomination, the real typical form was nearly lost by this intermixture. This confusion was carried still further in the thirteenth edition by Gmelin, where even all the true Graptolites were omitted. Wahlenberg restored the genus, all the forms given by him being those fossil bodies which belong to the typical species of the transition formations, but he only gave a superficial account of the subject. Schlotheim referred them to the genus *Orthoceratites*, and several other authors who followed added no original matter. Prof. Nilsson of Lund, undertook a monograph of the species of Graptolites found in Sweden. But he was prevented by circumstances, into which I need not here enter, from continuing his investigations on fossil remains, and some brief remarks only were published by him on this interesting genus, in the proceedings of the Physiographical Society of Lund. In that notice he proposed a new name for the genus, altering it to that of *Priodon*, a name not only objectionable as being unnecessary, but as having been already employed by Cuvier for a genus of Acanthopterygian fishes, of the family *Teuthedæ.*

"Since that time no attempt has been made to write a monograph. Prof. Bronn, of Heidelberg, in his Lethæa Geognostica, again however changed the name of the genus to *Lomatoceras*, a name already given to an insect."*

It appears then that the European naturalists class the Graptolites with the corals. The ramose nature of some of the species does not in our opinion form a serious objection to this view of the genus, since the branching form is eminently characteristic of many Polyparia. The chemical argument in favor of their vegetable origin, presents perhaps a greater difficulty; but may not the carbonaceous matter have resulted from the peculiar condition and circumstances attending the deposit? The Utica slate has often a notable quantity of carbon in its composition, and

* The above extract regarding an obscure point in natural history, will be interesting to those of our readers who may not have access to Mr. Murchison's work, since there is so great a diversity of opinion respecting the true nature of Graptolites.

might not these fossils, originally perhaps of a fleshy nature, have been transformed into carbon, somewhat in the same way that calcareous bodies have become siliceous during the process of petrifaction? Or might not, by the action of some chemical affinity, the less stable elements of the Polyparia have been removed and the carbon alone left?

Prof. Emmons gives five figures of fossils of the Utica slate besides those already enumerated, viz. *Nuculites scitula*, *Cypricardites sinuata*, *Nuculites poststriata*, *Avicula* ——, *Lingula rectilateralis*. Besides these there occur in it *A. Trocholites.*

According to Mr. Vanuxem the following fossils are common to the Utica slate and Trenton limestone; *Orthis striatula*, *Strophomena alternata*, *Lingula ovalis*, *Favosites Lycoperdon*, *Isotelus gigas*, *Calymene senaria.*

The range of the Utica slate is more extensive than that of the underlying Trenton and Black River limestone, since it is not only coextensive with these formations on the Mohawk and Black River, but extends further east, and forms, by Mr. Mather's account, a large portion of the slates of the Hudson valley. Indeed there are red and brown slates on the east side of the Hudson, which he refers to this formation, that range from Canada through Vermont, New York and New Jersey across Virginia.

Near Fort Plain, Utica, Hudson, the deep gorges at Rodman and Loraine, and Glen's Falls, are the best localities for the examination of the Utica slate.

In the valley of the Mohawk and Black rivers, the Utica slate forms the surface rock; through it, by uplifts, the inferior masses protrude.

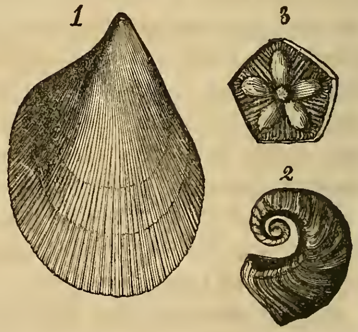
Some of the best grass and dairy lands of the state, have been derived from the degradation of these slates.

North of Little Falls, thin layers of fibrous sulphate of strontian have been observed by Vanuxem, running parallel with the slate. Some small seams of lead ore traverse the Utica slate, but no regular veins exist. Various unsuccessful explorations have been made in this formation in the valley of the Hudson for coal, and considerable sums have been expended in consequence of having found thin layers and small lumps of anthracite in it. Of course all such attempts must inevitably result in disappointment and loss, for reasons previously stated. The thickness of the Utica slate is not accurately known. It is supposed to be from seventy-five to one hundred feet.

Hudson River Group, (No. 3 of Pennsylvania and Virginia Reports,) embraces the shales and sandstones of Loraine, Frankford and Pulaski, together with the Salmon River rocks. This is the group to which the name grauwacke was originally applied; it has a very wide range and great thickness. Its maximum thickness is not less than seven hundred feet. In New York the group consists of shales and shaly sandstones, with their courses of limestone, the upper portion very fossiliferous. Its lithological character changes, however, going west. The beds in Ohio, which contain similar fossils, consist of grey and greenish grey marlite, interstratified with blue fossiliferous limestone. In the upper Mississippi, it appears as a magnesian limestone.

The fossils by which the shales of Pulaski and Salmon River are readily recognized, according to Vanuxem, are here represented.

Plate 9, p. 65, Vanuxem's Report.



1. *Pterinea carinata.*
2. *Cyrtolites ornatus.*
3. *Pentacrinites Hamptonii.*

Both the *Pterinea carinata* and *Cyrtolites ornatus* occur in the hills at Cincinnati.

We are not aware of this *Pentacrinites* having yet been found in the West.

Plate 113, fig. 2. *Avicula demissa.* 3. *Pleurotomaria*, (*Turritella obsoleta ?*) 4. *Orthis testudinaria ?*

This long spiral *Pleurotomaria*, which is found in the soft argillaceous mass near the top of the rock, (grey sandstone of Loraine,) is evidently the same which is found in Wisconsin in great numbers, in a stratum near the junction of the magnesian limestone and underlying grey limestone.

The *Avicula demissa* belongs to the lower part of the grey sandstone, and is also a Cincinnati fossil.

With regard to the *Orthis testudinaria ?* it is impossible to decide from figures on its identity with fossils from other localities; the New York geologists have not yet been able to determine whether it is distinct from a Trenton species, which it closely resembles.

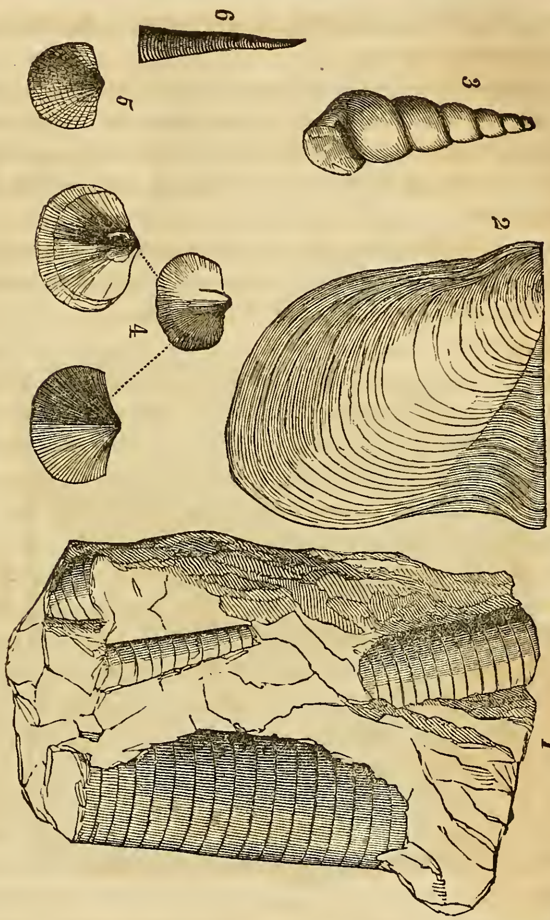


Plate 113, p. 404, Emmons's Report.

Plate 112, p. 403, Emmons's Report.

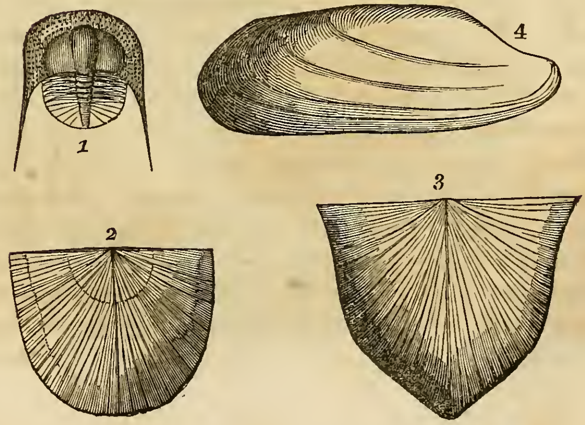
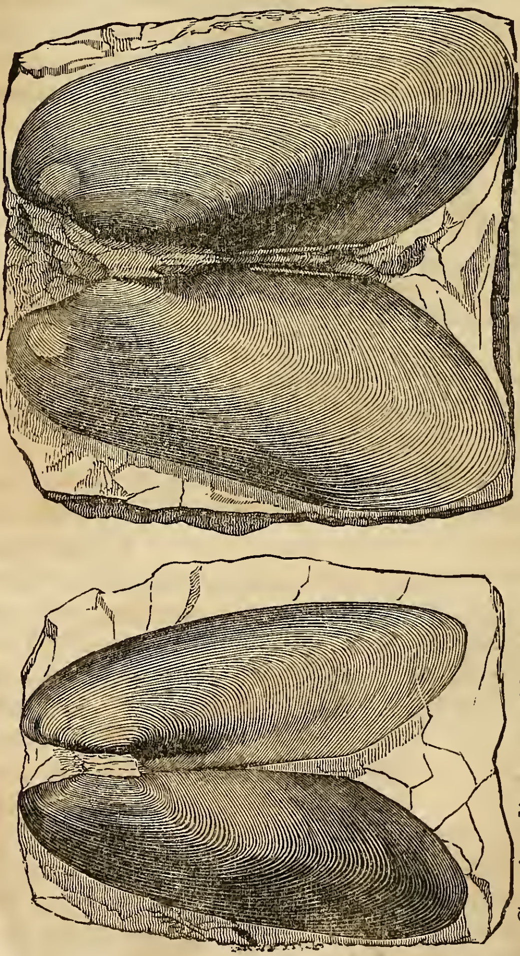


Plate 112, fig. 1. *Trinucleus caractaci.* 2. *Strophomena.* 3. *Strophomena nasuta.*

The first of these fossils is found towards the upper part of the Loraine shales. Dr. E. thinks it is identical with the true *caractaci* of the Caradoc; it also resembles a Cincinnati species, but we are not at present prepared to subscribe to the identity of either of these species.

The two *Strophomena* are, as far as can be determined by comparing figures with fossils, the same as two species common both in Ohio, Indiana, Iowa and Wisconsin.



Cyp. ovata.

Cypricardia angustifrons.

Plate 114, p. 405, Emmons's Report.

The *Cypricardia angustifrons* is doubtless the same as the Cincinnati species with which it was supposed to be identical.

The following fossils are also considered characteristic of this group. *Cypricardia modiolaris*, (fig. 4, pl. 112, p. 377,) *C. ovata*, (p. 378,) *Nucula* ——, *Orthis Actoniæ*, *O. crispata*, (fig. 5, pl. 113, p. 377,) *Orthis æqualis*, stems of *Crinoidea*, *Graptolites serratus*, *G. scalaris*. Those which are common to this group and the Trenton limestone are, *Calymene senaria? Strophomena sericea*, *Orthis striatula*. The *Triarthus Beckii* has also been found in this formation.

The Hudson River group, as already remarked, has a wide range; it is, in fact, one of the universal deposites; but the two masses into which it has been divided, viz. the upper and lower, are not coextensive with one another. The lower division or Frankfort slate, is the most persistent mass, being developed both in the valley of the Hudson and Mohawk, while the upper Pulaski and Salmon River shales and sandstone are fully developed only in the latter region.

The width of the belt of country occupied by the Hudson River group, is considerably greater than that of any of the preceding members of the Champlain division. In the valley of the Hudson, its average width is about twenty-five miles.

Between New Jersey and the Great Bend of the Hudson, it ranges nearly north and south, passing thence in a curve to Lake Ontario, whose general direction is northwest, up the valley of the Mohawk and down that of Black River.

According to the report of Prof. M. an anticlinal axis ranges from near New Baltimore, by Saratoga Lake, to Baker's Falls. East of this axis the strata of this group are upturned and contorted, and dip to the east southeast. West of it they are but little disturbed. The same beds on the west side of the axis have a different aspect on the east side. The cause which deranged their position, has also modified their mineralogical character.

The lower argillaceous rocks of this group, afford in some places a fire-stone suitable for lining furnaces and fireplaces; some of it might also be employed for roofing slates, but none is wrought for this purpose. Anthracite in small particles occurs in the graywacke of this group. Veins traverse the rocks filled with quartz, carbonate of lime and satin spar. Near the falls of Salmon River, the gray sandstone of this group affords grindstones. Near Red Hook village and Hyde Park, the slaty grits are worked into flagging stones. The upper fossiliferous portion of the Hudson River group is doubtless represented in the West by the upper beds of blue limestone and marlite of Ohio, Indiana, Kentucky and Tennessee, and the lower part of the magnesian lead-bearing rock of Iowa and Wisconsin. The English equivalent must be some part of the Caradoc sandstone.

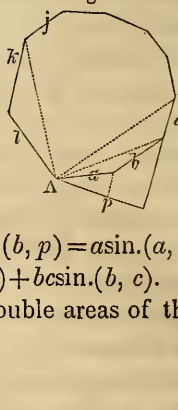
From the great change in the organic remains at the termination of this group, the New York geologists consider this the line of division between the lower and higher portion of the New York system. D. D. O.

(*To be continued.*)

Art. XIII.—*On the Measure of Polygons*; by Rev. George C. Whitlock, Professor of Mathematics and Experimental Science in the Genesee Wesleyan Seminary.

Let a, b, c, . . . j, k, l, (fig. 1,) be the sides taken in order of any polygon P. Divide the polygon into triangles, (A, b), (A, c), (A, d), . . . (A, k), by diagonals drawn from the junction A, of the first and last sides, to the extremities of the other sides. From A let fall the perpendicular p upon the production of any side c: we then have

Fig. 1.



$$2\,\text{tri.}\,(A, c)=cp.$$

But p is evidently the sum of the projections of the sides a, b, preceding c, upon a line perpendicular to c; therefore $p=a\cos.(a,p)+b\cos.(b,p)=a\sin.(a,c)+b\sin.(b,c)$; whence, $2\,\text{tri.}\,(A,c)=ac\sin.(a,c)+bc\sin.(b,c)$.

From what precedes it is obvious that the double areas of the triangles constituting the polygon will be

$$2(A,b)=ab\sin.(a,b).$$

$$2(A,c)=ac\sin.(a,c)+bc\sin.bc.$$

$$2(A,d)=ad\sin.(a,d)+bd\sin.(b,d)+cd\sin.(c,d).$$

&c. &c. &c.

$$2(A,k)=ak\sin.(a,k)+bk\sin.(b,k)+ck\sin.(c,k)+\ldots+jk\sin.(j,k);$$

∴ by addition there results for the double polygon,

$$\begin{aligned}2P=\,&ab\sin.(a,b)+ac\sin.(a,c)+ad\sin.(a,d)+\ldots+ak\sin.(a,k)\\&+bc\sin.(b,c)+bd\sin.(b,d)+\ldots+bk\sin.(b,k)\\&+cd\sin.(c,d)+\ldots+ck\sin.(c,k)\\&+\&c.\\&+jk\sin.(j,k).\end{aligned}$$

Therefore, the DOUBLE AREA of any polygon is equal to the sum of the products of its sides, save one, taken two and two, multiplied into the sines of the angles embraced by the sides forming the products severally.

This beautiful theorem is essentially that given by Hutton under the article Polygonometry. It is remarkable, that obviously useful as it is, it is not to be found in any other of our popular works on mathematics. The above demonstration is more analytical and vastly more simple than that given by Hutton.

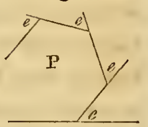
An interesting form for the mensuration of the regular polygon will immediately result from the preceding by putting

$$a=b=c=\ldots=j=k, \quad \text{and}$$

$$(a,\, b)=e,\ (a,\, c)=2e,\ \ldots\ (a,\, k)=(n-1)e,\ \&c.,$$

n denoting the number of sides, we have

Fig. 2.



$$2P=\left[\begin{array}{r}\sin.e+\sin.2e+\sin.3e+\ldots+\sin.(n-1)e\\ +\sin.e+\sin.2e+\ldots+\sin.(n-2)e\\ +\sin.e+\ldots+\sin.(n-3)e\\ +\ \&c.\\ +\sin.e\end{array}\right].a^2;$$

$$\text{or } 2P=[(n-1)\sin.e+(n-2)\sin.2e+\ldots+\sin.(n-1)e].a^2.$$

ART. XIV.—*On the Discovery of Fossil Footmarks;* by JAMES DEANE, M. D.

A DEFINITE settlement of the priority of claims to the discovery of footprints in the sandstone of Connecticut River, is due to the parties preferring these claims, and to the cause itself.

It is remarkable that these striking impressions, abounding in almost every sandstone quarry, should have escaped observation so long. It is true they have been noticed by many old quarrymen in the service of the canal companies, and by others, but without the slightest comprehension of their origin or character. Although they were formerly seen, they were nevertheless as much unknown to the learned world as when concealed in the depths of the earth. The eye of science had not seen, nor the light of intelligence dawned upon them until the present day. But when the obvious meaning was once established, when the master-key to unlock the mystery was found, the true discovery became a subject of profound interest.

In the spring of 1835 there was brought to the village where I reside, a quantity of stratified sandstone for economical uses. One of these slabs was deeply impressed with the feet of two kindred birds walking in parallel lines, which were exceedingly distinct and beautiful. These impressions were generally seen, and for a time attracted much curiosity; but the novelty soon subsided and they were forgotten, so far as I know, by all but myself. The remarkable phenomena presented by these astonishing indications of ancient existence, induced me to secure the title of the specimens; and the interpretation of this new language was so intelligible, that I straightway resolved to communicate my conclusions to several distinguished geologists. The acknowledged reputation of Prof. Edward Hitchcock of Amherst College, induced me in the first instance to correspond with him. Accordingly, on the 7th of March, 1835, I addressed him a letter, from which the following quotations are derived.

"In slabs of red sandstone, &c. I have observed singular appearances, new to me, although I presume not to yourself.* One of them is distinctly marked with the track of a turkey (as I believe) in relief. There were two of the birds side by side, making strides of about two feet.

"I was anxious to see the die from which these impressions were struck, and it has now arrived. The tracks, four in number, are perfect, and must have been made when the materials were in a plastic state, and at what period I leave you to tell."

The intelligent reader who honors me with a perusal of these statements, is requested to notice the unequivocal expression of my opinions; for so far as they go, they constitute the essential doctrine that the impressions were made by *birds*, and assert the condition of the rock at the period when the impressions were made. These are fundamental principles, and setting aside the hasty and erroneous opinion of the particular variety, the results of nearly ten years of investigation have not modified them, but they have been confirmed by every subsequent discovery.

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* Prof. H. was at that time geological commissioner to the state of Massachusetts.

Mr. H. replied, that the only authenticated instance of fossil footprints was that of a tortoise upon the sandstone of Scotland, but added, "I am not without strong suspicion however, that the case you mention may be a very peculiar structure of certain spots in the sandstone; which I have often seen in a red variety of that rock. The layers of rock having this structure sometimes present an appearance resembling the foot of a bird. But I am satisfied that it is not the result of organization, although I confess myself unable to say precisely from what principle it resulted. But perhaps the case you mention is not of this sort," &c.

My belief was not shaken by the incredulity of this answer. Conscious now that it was a genuine discovery, and that my conclusions were correct, I immediately despatched a second note to Mr. H. in which I made a positive declaration, (in italics,) "that *in my mind there is not a doubt but they are real impressions of the feet of some bird.*" To rebut the supposition of accident, I remarked that "there are more tracks to be seen in the lot, which I suppose to be a continuation of the line. This fact would be a strong argument against these appearances being formed from accidental circumstances." As an additional *fact* I stated, that "on examining the opposite side of the slab, I find that the weight of the bird did perceptibly elevate that portion opposite the tracks. This happens to them all." I likewise reiterated my unchangeable conviction "that these impressions are genuine, and if so they prove an interesting subject for the geologist." This declaration shows that although I was not a practical geologist, I was nevertheless able to appreciate the results indicated by these extraordinary relics.

No reply was returned, but the subject grew more intensely interesting as it was more intimately studied. In April, I addressed a third and elaborate communication to Mr. H. illustrated by diagrams and by accurate models of the impressions. Unfortunately I have no copy of this (to me) important essay, and the original is lost. I also addressed a duplicate to the senior editor of this Journal, who replied, "the facts you state, illustrated by the cast and by the diagram, are very striking, and appear to carry considerable probability with them that these impressions are real tracks. I know of no antecedent improbability, certainly of no impossibility in the case. While turtles and other amphibiæ were sporting in the shallows of islands, birds might flit or walk about upon the ground, or wade in a soft beach or bed of mud, leaving their tracks to be filled or consolidated."

Although it is now nearly ten years since this letter was written, its contents are vivid in my memory. It was a demonstration of facts; an attempt to prove from them by the aid of analogical reasoning that these splendid footprints were those of extinct birds. The truth was before me, and unbiassed by preconceived notions I believed it; and I still maintain that these three letters, written without a ray of knowledge other than was derived from philosophical inductions, contain the fundamental principles and doctrines applied to the science of these organic remains. It needed not the subtleties of technical learning to comprehend their meaning, and I from the first asserted the affirmative propositions, that the impressions were those of birds; that they were alternate and consecutive tracks; that each line of footprints was characterized by an individuality that carried with it unquestionable proof of animal origin;* that they were made when the stratum was in an impressible state; that the stratum was actually depressed beneath the weight of the bird, and that upon the superior stratum the feet were exhibited in relief, &c. &c. I did not indeed proceed to a mechanical arrangement into classes and orders, but the law of discovery was as fully exemplified by these perfect relics, as it now is after years of successful exploration. Subsequent researches have sustained the sentiments of these letters, and although the numbers and varieties of impressions have become greatly multiplied, still the irreversible principles that apply to the original examples, apply equally alike to all others.

Nor did Mr. H. reply to this third letter; but he subsequently saw the specimens, and admitted the correctness of my views, which he has since repeatedly done; views which have never been refuted by persons competent to appreciate them. The love of possession might have induced me to retain so rich a treasure, yet I presented it to Mr. H., permitting him however, at his pressing solicitations, to reimburse the purchase money. Self-love too, might have induced me to establish the honor of original discovery by recording its history, but I yielded this point to him in the implicit confidence that he would render the subject and myself impartial justice. In a letter dated Sept. 15th, 1835, he informed me

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* Third letter.

that in a paper he was about to publish in this Journal, he should "not fail to acknowledge his indebtedness to me for the first discovery." The performance of this pledge consisted in the remark, that "*his attention was first called to the subject*" by me, but no mention whatever was made of my relations to the discovery.

Had the facts in connexion with this discovery been duly accredited, the necessity of appearing upon these pages to vindicate my claims, and to recover a field too inconsiderately surrendered, would happily have been obviated. I look upon a controversy, as this will doubtless seem to be, with unmitigated aversion. To Mr. H. I am conscious of no unkindness. I am bound by many obligations to him; and he will understand that my motives are not to assail his reputation, but to sustain my own. This reclamation therefore, must be ascribed to the prerogative of self-defence, which will be justified by additional facts and particulars.

The grand results of the researches of Mr. H. were published in his Final Report to the Legislature of Massachusetts, which professedly embodied all facts related to the subject, up to the time of its publication in 1841, yet no allusions are made to the foregoing correspondence; every fact associated with my labors being omitted. The only mention of me in this voluminous essay, is in the description of the original slabs, as having been "*pointed out*" by me to him, and in dedicating a particular variety to my name as a testimony of respect for having "*first called his attention*" to the subject of fossil footmarks. I felt the coldness of these ambiguous compliments, for in his conclusions from the facts, and elsewhere in this learned work, its author was compelled by controlling necessity, to adopt facts, opinions and arguments which were emphatically expressed to him ere his scepticism had been dispelled; yet I did not complain.

My explorations about the year 1841 were crowned with the discovery of several varieties of bipedal, quadrupedal and vegetable impressions of peculiar beauty and value, which I presented to Mr. H., as has been my invariable custom with *every* new variety, that the collection in Amherst College might be complete. In alluding to my repeated remittances, he wrote, "if you do not stop discovering new specimens, my forthcoming paper will be as long as the article on footmarks in my Final Report." Now this paper, read before the Association of American Geologists, and published in the first volume of Transactions, contains but a bare allusion to the fact. Most of these specimens were of remarkable interest, and many of them were single examples. Of the expenditure of time and money in procuring these fossils, I need only say, that these items were by no means inconsiderable, but I was laboring for the love I bore the cause. The results of my exertions at this period, were well calculated to illustrate the perfect analogy between the extinct and existing races of birds, for no example had hitherto been seen that displayed the order of articulations, and the form and insertion of the talons.* Without these indispensable features, the true characters of no impressions can be determined; for all others are more or less imperfect.

During the sitting of the Association, &c. in May last, a communication appeared in the Northampton newspaper, circulating extensively in the neighborhood of Amherst, from a correspondent in Washington, alluding generally to the business before that learned body, and particularly to so much of the address of Mr. H. on footmarks as relates to their discovery. The obvious import of the allusion to this subject, was to weaken the validity of my claim, by conferring the honor of discovery on a Mr. Moody and some others; Mr. M. having seen imprints in the year 1802. The particulars relating to Mr. M. were full, while the only notice of myself was the somewhat equivocal expression, that the subject was first "*pointed out*" to *me* by a Mr. Wilson, in 1834, (I never saw them until 1835,) and by me in turn to Mr. H.! Thus in stereotype phrase, making me a mere negative instrument between the pretended discoverer and his historian. When I knew that the authorship of this letter was due to Mr. H., when its object was apparent, I could not repress the consciousness of my humble efforts to supply him with *materiel* for his periodical memoirs. I felt the injustice of this deliberate attempt to place me in a position, not only subordinate to himself, but to another to whom I declare I was never under an obligation of any nature whatsoever; and after all that had passed between Mr. H. and

* In the thirty-sixth plate of the Final Report, the claws of this enormous foot are drawn long and sharp like a bodkin, and are inserted far back into the *inferior surface* of the fleshy protuberance of the joint. These discoveries prove the mistake of this inference, the claws of this and most other varieties being thick and blunt, and comparatively short, and their insertion conforms to that of living birds without the least deviation.

myself, the manner and matter of this publication, filled me with vexation and astonishment. I supplied Mr. H. for that paper alone no less than three or four new varieties of these impressions, and since the publication of his Final Report, he has with perhaps a few exceptions derived from me, as I believe, his new varieties of sandstone fossils.

Antecedent to the delivery of this identical address, no other notice was taken of my correspondence or of my labors in this geological field; but from the force of circumstances, its author has at this late time of day, detailed some additional and important parts of the historical evidence, applying to it however, an exclusive interpretation. With singular zeal to mete out a fair equivalent of justice to the original observer, it is not a little unaccountable that he was so tardy in the performance of the act. The specimen of Mr. Moody was purchased in 1839, and although the Final Report and other able treatises appeared subsequently, still the paramount claims of Mr. M. have ever been overlooked.* In the year 1842, I remitted to Dr. Mantell of England, a small but very fine collection of footmarks, with a private communication detailing the obvious meaning of these fossils and incidentally alluding to my relation to the discovery.† The greatest scepticism then existed in England, as to the inferences drawn from this discovery, and it was therefore an unexpected compliment to me that my communication was presented by Dr. M. to the notice of the London Geological Society, and that this gentleman afterwards wrote to me in reference to its reception, "it cannot fail, sir, to be gratifying to you, to know that your brief but lucid description, illustrated by the highly interesting suit 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." Mr. Murchison, the president,

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* I am utterly unable to comprehend the claim of Dr. Dwight, resting solely on the fact of having purchased the specimen of Mr. Moody. With respect to the claims of the other gentlemen I have no remark to offer.

† At the instance of the senior editor of this Journal, through whom the communication was made. Always entertaining a firm belief in the ornithological origin of these impressions, Prof. Silliman was solicitous to dispel the incredulity of certain English geologists, through the instrumentality of perfect specimens, when such might be obtained. It was the more gratifying to comply with his wishes, inasmuch as they harmonized with my own.

in his annual address likewise alluded to the letter in handsome terms, and with entire impartiality defined the distinction between the claims of Mr. H. and myself; and he paid a just tribute of respect to Mr. H. "for the great moral courage exhibited by him in throwing down his opinions before an incredulous public." It may be merely a concurrence of circumstances, but until the publication of this correspondence, Mr. H. did not urge any exclusive claim on his own behalf, or on that of his numerous subordinates.

Now the most inexplicable part of this address is this, that having arrayed a company of original discoverers, Mr. H. should entirely cancel their claims, by appropriating to himself the honor of original discovery on the assumed ground of science! In Sept. 1835, after he had settled upon his scientific nomenclature, he acknowledged to me that I was the original discoverer, and the spirit of his early correspondence testified to the sincerity of this admission. The deliberate assumption, that although others had *found* these important fossils, *he* only had *discovered* them, penetrated me with a keen sense of its injustice.* It was enforced by allusions, degrading me on the ground of incompetency to understand a self-evident truth. In my first letter to Mr. H. I admitted that I was not a geologist, and this admission he turns into a keen weapon against me. I also, most unscientifically, suggested the variety of bird that made the impressions, and he alludes to this as corroborating evidence of incompetency; he even thinks that *Mr. Wilson* did not suggest this idea to me, and that it was original with me! Mr. H. should be slow to taunt an associate or an adversary on the score of hasty and erroneous conclusions. Even on the subject of these footmarks, Mr. H. himself is not quite clear of mistakes, for he has dropped several of his species, after a full and *scientific* description of them; and it is my deliberate opinion that the cause would suffer no injury if the list was still much more condensed. Nay, he has repeatedly committed the disqualifying error with which he charges me, "of referring the tracks to birds similar to those now living." In the twenty ninth volume of this Journal, p. 327, Mr. H. compares a particular variety of footmarks with those of the "*turkey*," as having "a similar foot;" and in the succeeding page, he affirms

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* See Report of the doings of the Association at page 113 of this volume.

that another variety "might have been produced by that portion of the Grallæ denominated *Cursores*." And in his Northampton letter he says that the Dinornis of New Zealand, which he conjectures is an existing bird, "was probably of a similar character to the bird that made the footmarks on the sandstone." In connection with my inability to comprehend the meaning of the original fossils, Mr. H. alludes to himself as one whose "professional business it was to examine such objects," and repudiates the idea that *my* opinions could make an impression upon him, although he had hitherto repeatedly acknowledged the correctness of my views.

I acknowledge that accidentally blundering upon a thing, irrespective of those mental relations, that appreciate causes from the results of causes and effects, does not constitute a claim to original discovery. But are we to infer that the history of these impressions would still be a blank, had not the scientific pen of Mr. H. recorded it? Was *his* agency an indispensable requisite in promulgating a knowledge of their existence and character? If this be true, his exclusive claim is impregnable. If the application of science to this subject, consists in arbitrary classification; in the adoption of terms of non-committal import in essential particulars; in applying to the acknowledged footmarks of birds, terms which belong exclusively to reptiles; in founding species upon distorted and doubtful examples; in throwing doubts around self-evident truths, and in the adoption of erroneous conclusions, and the assumption of theories, then the claim of original discovery rests upon a broad basis. But, if by science is understood the comprehension of an eternal truth, unbiassed by theory, then is this claim less unquestionable. Mr. H. performs an act of injustice to himself, if he entertains for a moment any belief that had he not published the history of this discovery, I should not have done so; and I now question him, if notwithstanding his science and my supposed incompetency, he was not under the lively apprehension that I should precede him in this matter? This is indeed true, and no fallacy of argument can overthrow the simple fact, that if I had not *found* or *discovered* the footprints, put it in either contingency, neither would Mr. H. nor either of his numerous company of claimants, have *found* or *discovered* them.

In the pride of honorable learning, Mr. H. has too far underrated my humble exertions to elucidate the history presented by the eloquent imprints upon the sandstones of the Connecticut River. Who first might have seen them is unimportant, so long as the world was none the wiser; who first proclaimed their true meaning, the candid reader must determine. I accord to Mr. H. the highest considerations of respect, for the ability and zeal with which he has followed up a subject which, personally, I must always maintain was begun by me with an earnestness that gave no indications of too hastily abandoning it. I have hitherto refrained, contrary to the advice of many friends, from entering upon the defence of my labors in this beautiful department of geological science; and it is with pain and reluctance, that I perform that service now, for by the common standard of observation, I am sensible that these statements must clash with other views, entertained by one whose friendship I appreciate, and should deeply regret to lose.

Greenfield, Mass., August 17, 1844.

Art. XV.—*Rejoinder to the preceding Article of Dr. Deane;* by Prof. Edward Hitchcock.

The editors of this Journal having kindly put into my hands a proof-sheet* of Dr. Deane's paper on the Discovery of Fossil Footmarks, I feel bound to rejoin a few remarks; seconding earnestly the desire of the editors, that this discussion may close with the present number.

The extraordinary claims advanced by Dr. Deane, and his severe personal crimination, render it necessary for me to be somewhat more specific and plain than I have been on some points. I avoided these details in my Report, (called my "Address" by Dr. D.) in order to save his feelings; and I now make them, as he says he made his, "not to assail his reputation, but to sustain my own."

* By special agreement between the writers of these discussions, their respective proofs are mutually read, each receiving that of his antagonist, the object being to close the discussion in the present number, and then to submit the cause.—Eds.

I now understand this gentleman to claim, not only the original discovery of these footmarks, which in a popular sense I awarded to him, but their first scientific investigation; that his "three (first) letters, written without a ray of knowledge other than was derived from philosophical inductions, contain the fundamental principles and doctrines applied to the science of these organic remains;" that in my Final Report on Massachusetts, I was "compelled by controlling necessity, to adopt facts, opinions and arguments, which were emphatically expressed to me ere my scepticism had been dispelled;" and that it was only an "implicit confidence" in my readiness to render him "impartial justice," that led him to yield to me the liberty to record the history of the footmarks. If this is indeed a correct view of the case, then I am far more culpable and dishonorable than Dr. Deane represents me; though his charges of injustice are very severe. But let us look for a moment at the facts.

Early in the spring of 1835, (not 1834, as Dr. Deane says is stated in my Report, of which unfortunately I have no copy, having returned the proof,) a cloven specimen of sandstone, containing peculiar impressions, was brought to Greenfield, through the agency of Mr. Wilson,* and laid by the roadside in the street. Dr. Deane, whom I had known as a respectable young physician, with a predilection for scientific pursuits, sent me an account of them; declaring his unhesitating belief that the impressions were "the tracks of a turkey," stating at the same time that he was "no geologist," and presuming that these appearances, though new to him, were not so to me; and expressing a willingness to have them preserved for me if I desired it. What now would be the conclusion of a geologist from such a letter:—a geologist who had sometimes been led away by respectable men long distances in vain, to see supposed tracks on stone? From the known scientific taste of such a man, he would, indeed, hope that the impressions were something more than diluvial furrows, or veins of segregation; but he would see at once that Dr. Deane was unacquainted with the history of organic re-

* How unfortunate have I been in my efforts to avoid intimating that Dr. Deane derived his opinions from Mr. Wilson! In consequence of a letter received from him just before my Report went to press, in which he manifested much sensibility on this point, I added those explanations in which he now sees only "a taunt to an associate." But jealousy is argus-eyed.

mains, or he would not have referred these markings to a living species or even genus of birds; secondly, that he was not aware but that the tracks of birds were common on stone; thirdly, that of course he could not know about the Scottish tracks, the only then known example in the world; and finally, that he had not made, nor intended to make, any scientific examination of these tracks, and therefore that his opinion concerning them was the result of casual inspection, and of no more consequence than the opinion of any respectable sagacious man who was not acquainted with the subject. Such certainly were my conclusions; and accordingly on the 15th of March I replied, expressing a desire to have the specimens preserved, and suggesting that perhaps they might prove to be something else than tracks. On the 20th of March Dr. Deane sent another letter, saying—"I received your letter this morning, which excites my curiosity more than ever, relating to those tracks." He then says, that he had examined them anew, and presents similar conclusions to those in his former letter. As he was stimulated to this examination by the facts in my letter, and as he could have had only a few hours for his new examination, I saw nothing to alter my impressions derived from his first note, or to need a reply. A few days afterwards, I received from him two plaster casts of the impressions, with a note, I think, though I have no recollection of its contents; and unfortunately the original cannot be found on the files of Prof. Silliman or myself. It is easy for Dr. Deane to magnify the importance of this lost document; but I am sure it contained no new facts or reasoning not in his previous letters. Certain I am that it made no impression on me; though the casts excited stronger desire to see the specimens. My doubts were not in the least diminished by any of his letters, just because his first letter showed conclusively that he was not enough acquainted with the subject to judge correctly concerning it, and had given it only the slightest examination. In a few days I visited Greenfield, and found that the specimens had not been removed from the streets; nor did Dr. D. express any unwillingness to let me have them; nor then, or at any subsequent time, did he intimate that he intended to investigate the subject, or publish its history; and since he asks the question, I state most decidedly, that at no time up to this hour, unless my memory deceives me, have I had the least apprehension or suspicion that he might anticipate me in giving an account of the tracks; or that he had any intention or wish to do so.* Indeed, excepting a single specimen, I had all the facts in my possession, and how could I fear that any one could publish them? I knew that Dr. Deane's examination consisted only of an occasional inspection of two or three specimens of one species as they lay in the streets. I knew that he had not visited a single quarry, nor had searched for the tracks of living animals in museums and by the rivers. His opinion, therefore, had no weight in removing my doubts; and even

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* *Postscript.*—On seeing this statement in my manuscript, Prof. Silliman kindly reminded me of the following extract from my letter to him of July 30th, 1835, which, without explanation, seems to justify the suspicions of Dr. Deane, and did probably originate them. After saying that I did not wish to announce my conclusions "until I well understood the case," I add: "My intention is, to offer you a paper on the subject for the January number of the Journal. I shall give to Dr. Deane the credit of having first put me on the *track* after these relics; but I hope you will delay his descriptions until you receive mine: as I am sure I shall be able to present a more full and satisfactory view of the case than he can do." And this was written in reply to the following request from Prof. Silliman, in a letter of July 22d, 1835:—"I suppose you have seen the so-called bird tracks on red sandstone, near the ichthyolite locality. Dr. James Deane of Greenfield sent me a plaster cast and a description, which I would publish if I were sure there is no mistake in the affair. Will you give me your opinion; for I should not like to make a stare about birds as early as the new red sandstone, and then be laughed at as —— was for his ——." From this letter I understood that the idea of publishing this description originated with Prof. Silliman, and not with Dr. Deane. And as I was proceeding with the investigation of the case, I thought I might request him, on the score of personal friendship at least, to delay his publication till I had finished my researches and made out my account, especially as my opinion seemed important to him in coming to a decision, and I did not wish to have that made public till I could mature and fortify it much more. I supposed that of course the descriptions must be those of Dr. Deane, similar to those he had sent me,—only first impressions from a single specimen,—and I had proceeded so far in my examinations, as to make me feel that it was no vanity to say, that my final account must be more satisfactory than any he could produce from the means I knew him to possess, or rather without any specimens. I confess, (and I hope naturalists will not judge me too severely here,) that I did feel a desire, when I found how rich a field I had entered, to bring out its first scientific description; and not to have geologists prejudiced against the whole subject by a premature account, which, even with Prof. Silliman's skill, with the means in his hand, must have been scanty and crude. A letter from him of August 6th, in reply to mine, could not but confirm my impressions that the idea of publication was his own. He says—"I am much gratified that you are seriously at work upon the turkey tracks or bird tracks of whatever kind they may be; and you may rest assured that I shall publish nothing upon the subject until I receive it from you. I will, therefore, expect you to do justice to Dr. Deane, as you are perfectly acquainted with the circumstances; and if you see Dr. Deane, I will thank you to

after seeing the specimens, I suspended my judgment till I could, in the first place, examine all the quarries of sandstone within my knowledge, to find every variety possible; secondly, till I had examined all accessible works on organic remains, and all the important collections of the same in the country, to see if these impressions could not be referred to some of them; thirdly, to explore in books and in nature all the cases of the sliding of strata upon one another, of veins of segregation, and clay veins, and of mud furrows; of concretions, ripple marks, and septaria, and unequal disintegration of rocks for the same purpose; fourth-

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intimate to him what I have said. My impressions are so strong in favor of the genuineness of the discovery,—judging only from the imperfect copy I have in plaster,—that I feel exceedingly desirous to have the matter investigated; and I do not know in whose hands it can be better placed."

It appears, then, that I was apprehensive Prof. Silliman, instead of Dr. Deane, was about to anticipate me in announcing the footmarks. For had I supposed the latter gentleman to have sent a communication to the Journal, I could have hardly had the impudence to request that it might be thus unceremoniously set aside; nor would Prof. S. have endured such an interference with his duties as an editor. And further, in a letter to Dr. Deane of Sept. 15th, I told him that I should not publish till the January number of the Journal of Science; which I should not have done, if I had feared he would anticipate me, since the October number was still open for papers. If Prof. Silliman should make the charge upon me which Dr. Deane has, I could hardly vindicate myself. But I repeat, that I am not conscious of ever having had fears that the latter intended to anticipate me, whatever may have been the fact as to his intentions. But admit that a treacherous memory has deceived me; nay, suppose he had actually published all that he ever wrote about that one cloven specimen; it would still be no less true than it now is, that I made the first scientific examination of the footmarks; which is all that I claim. I will add, that not until quite recently, although years of pleasant and friendly intercourse have passed between Dr. Deane and me, have I had an intimation that he was not fully satisfied with the credit which I have awarded him.

I am glad to have had my attention called to the early letters of Prof. Silliman on this subject; for the extracts above given show, that Dr. Deane's plaster casts and letters produced essentially the same effect upon him as upon myself; viz. a stronger desire to see the specimens; but they neither removed his "scepticism," nor prevented his "feeling exceedingly desirous to have the matter investigated." I have been also struck with the distinctness and accuracy of his early opinions upon the footmarks, formed, as he says they were, from an "imperfect copy in plaster" of one species, and with a full knowledge of all the geological objections to their *ornithic* character; which fact makes a world of difference in estimating the value of those opinions. And were he to demand of me, even at this late day, the *amende honorable* for omitting to notice those opinions in my papers, I could hardly refuse it; presuming that on his part he would admit the omission to have been unintentional. His attention was called to the subject about as early as mine; and had he taken the field, the public well know that my labors would have been unnecessary.

ly, to examine the feet and tracks of living animals in museums, menageries, and on mud and sand, for the same purpose. This was the work which must be gone through before my "scepticism" was removed, so that I could venture "to throw down my opinion" that these were bird tracks "before an incredulous public." In this work I spent a considerable part of the ensuing seven months; nor was it participated in at all by Dr. Deane to my knowledge after I obtained the specimens; and before that period he could not have done it in the few days that elapsed after his attention was called to them before he gave his opinion. Indeed, during the five succeeding years, in which I toiled alone in this untrodden field, I have no evidence that he did any thing on the subject, except occasionally to inquire what progress I made in it. Here was the tug of the war; and if he had intended to claim the first and highest honors of victory, he should have been there shoulder to shoulder, or rather before me in battle.

Now in view of this statement, I appeal to naturalists every where, (for they are the only competent jury in such a case,) whether I have not given to Dr. Deane all the notice and credit which belong to him? What could I have said more, unless I had stated what I know to be false, viz. that his reasoning and facts convinced me, and that he had scientifically examined the subject? He speaks slightingly of my affixing his name to one of the species. But naturalists know that this is one of the highest honors which they can render to those who aid them by specimens or otherwise; and they never do it unless they conceive the person has unusual merits, because they thus associate him with veterans in science. I appeal too to naturalists to say, whether the only honor I can justly claim in this "seven years' war," consists, as Dr. Deane maintains, in carrying out and illustrating, and very clumsily too according to him, his splendid generalization, "derived from philosophical inductions," that these markings are "real impressions of the feet of some bird, probably of the turkey species."

To show that I have always been of the same opinion, as to assistance derived from any who preceded me in this matter, I quote a sentence from a Report on Ornithichnites for 1836, which I sent at the close of that year to this Journal, but which was subsequently withdrawn. I say there, that "it would be strange, if on a subject so novel, where one has to grope his way *without any assistance from the labors or opinions of his predecessors*, some modifications of early opinions should not be necessary as new facts are brought to light."

Dr. Deane thinks it strange that I should have been so tardy in awarding justice to those who preceded him in the discovery of these tracks; and he speaks of them (Dr. Dwight, an aged and respectable physician; Mr. Moody, a farmer, but a man of public education; and Mr. Wilson, an ingenious mechanic) as not having "the slightest comprehension of the origin or character" of the tracks. I did not, indeed, think it necessary to name them till some of them intimated to me that they ought to have been mentioned. But one important object is hereby accomplished. However incompetent they are, they certainly discovered these tracks earlier than Dr. Deane, and came to the same conclusion as he did, as to their being bird tracks, and for similar reasons; and I might name fifty others, who, upon looking at my specimens, have expressed the same views at once; so that it does not require scientific investigation to reach this conclusion. But it does demand it to *establish* the conclusion; and this is what I claim to have done independently.

Dr. Deane also manifests great sensibility because I quoted his first letter to show what he terms his "incompetency." Let him understand that I charge him with no intellectual incompetency to investigate this subject. On the contrary, I have a high opinion of his ability for such a work. But I do maintain, that at the time he discovered the tracks, he did not understand the subject in its connection with geology, simply because he had not studied it. And my proof is, his first letter. If, as a geologist, he had examined the subject before I took it in hand, and given his opinion as the result of his investigations, I could have no apology for omitting to notice that opinion. I was compelled therefore to publish that letter, or lie under the imputation of having acted dishonorably. In his letter to Dr. Mantell, read before the London Geological Society, he stated that when he first found the tracks he was "aware that footsteps of animals upon rocks were unknown, or at least controverted occurrences," (I impute the discrepancy between this statement and his acknowledged ignorance of the subject in his first letter to me, to a slip of memory,) and that the scepticism of Professor

Silliman and myself was overcome by his efforts. That distinguished society understood of course, that Dr. Deane was as much of a geologist in 1835 as in 1842, and the result was just what might be expected; for while those high-minded men, Dr. Mantell and Mr. Murchison, whose opinion is law in the geological world, awarded me some compliments, they represented me as having acted only a subordinate part in respect to the footmarks, and made the impression unavoidable, that I had withheld from others the full measure of justice. With the data before them, their impressions were perfectly correct; and they have gone forth over the whole geological world. To remove them, at least within a limited sphere, has been the grand motive for presenting the statements now made, and especially the letter under consideration. I must leave the future historian of science to complete the work, if he judges I have made out my case.

As to Dr. Deane's efforts to bring discredit upon my published labors concerning the footmarks, I can only say, that none can feel their imperfection more deeply than myself; and it does not become me to doubt, that had he undertaken it, the work would have been more satisfactorily done; and if he now obtains the chief honor of it, I could wish he had had the labor, and thus several years of my life have been saved for other purposes. I will add however, that as to most of his criticisms, I am confident he never would have made them, had he ever carefully examined my large collection of specimens, or even other quarries, besides the four or five within six or eight miles of his residence.

This then is a brief summary of my positions. I declare most emphatically, that I have never received any assistance from Dr. Deane in investigating the footmarks, previous to the publication of my Final Geological Report, except specimens; and that his early opinion as to their origin had no effect whatever in removing my doubts, or in leading me to my final conclusions, because that opinion was not the result of scientific examination, but of the occasional inspection of a single cloven specimen as it lay for a few weeks in the highway—and because I found that the same opinion had been entertained by others many years earlier, and was indeed forced upon every intelligent man, by the first inspection of good specimens. I further maintain, and have endeavored to show chiefly from his own letters, that in 1835, when that opinion was given, he was not familiar enough with geology to appreciate the necessity of those researches in relation to segregated and clay veins, mud furrows, ripple marks, disintegration, concretions, organic remains, slides, faults, &c., which in the first instance cost me months and finally years of labor, and without which, no geologist would ever admit these markings to be bird tracks, although, as the results were chiefly negative, they were scarcely noticed in my papers. I maintain that I first, and for several years alone, made these investigations in relation to the tracks of this country, and therefore may claim to be the *discoverer*, in a scientific sense, of fossil bird tracks; and to admit the claims of Dr. Deane to a priority to myself in all these respects, and thus make me a mere humble expounder of his views, does me great injustice, and affixes a most unmerited stigma of illiberality and unfairness upon my character. On the other hand, I acknowledge, and from the first have acknowledged—according to the strictest rules observed by naturalists in these matters—my great indebtedness to him for calling my attention to the subject, and for specimens. I admit him to have been in a popular sense, the original *discoverer* of the footmarks; and had it not been for his scientific discernment, probably they would still have remained undiscovered. I admit that since he has turned his attention to this branch of geology, he has shown unusual ability as an observer, and produced some highly creditable papers on the footmarks; and by saying that he was not familiar with the subject in 1835, I merely echo the sentiment of his own letter, and mean not the slightest disrespect to his character.

I have thus written plainly, but I would hope not unkindly. If so, I charge it to the language rather than my heart. It is the most unpleasant discussion, on several accounts, in which I was ever engaged; and I have tried every possible way to avoid it, consistently with a sense of duty to myself, my children, and others. Had I been alone concerned, I should have borne in silence what seems to me the cruel injustice of having the fruits of several years of hard labor taken from me and transferred to another, just as I seemed on the point of gaining the hard-fought battle. But the desire of leaving some legacy to one's children and friends is lawful; and if I cannot leave to my family, and the institution with which I am connected, a name free from dishonorable imputations, and a modicum of scientific reputation, I can leave them nothing. If the claims now set up are acknowledged, a taint of dishonorable suspicion will attach to me, and the credit be wrested from me, of the most original and laborious scientific efforts of my life—and that too by friends! For in spite of the needless severity of Dr. Deane's article, I will still believe him sincere and honest in maintaining his claims. And now that we have referred our cause to the scientific public, he will, if he chooses, find me ready to reciprocate the offices of private intercourse and friendship.

I hope the Editors of the Journal will not consider all the space devoted to this discussion as lost to their readers. For it seems to me that the principles examined, are important to the decision of many cases of a similar character which are frequently occurring.

Amherst College, Sept. 16, 1844.

Art. XVI.—*Answer to the "Rejoinder" of Prof. Hitchcock;* by James Deane, M. D.

The preceding rejoinder being in the main repetitions of former statements, I had deemed any farther refutation unnecessary, but I am compelled to think a few brief explanatory remarks are indispensable, relating chiefly to matters of fact.

In the first place, the date 1834 was not quoted by me from the Report of Mr. Hitchcock as he affirms, but from his Northampton letter.

2. I never declared that the impressions were the tracks of a *turkey*, but of some bird, probably of the turkey species. It was the expression of an opinion, not of a fact, and I have shown that this loose comparison has often been made by Mr. H.

3. I solemnly reaffirm that Mr. Wilson never gave me information concerning the original discovery, and the statement that this gentleman pointed out the specimens to me, is unjust.

4. The circumstance of not removing the slabs is at most a negative argument of little moment. I had secured them from injury; they were large and heavy, and distant but a few yards from my study. But the true reason of this apparent negligence was, not that the specimens were indifferent to me, but that I had determined to present them to Mr. H.

5. The assertion that Mr. H. was apprehensive I should anticipate him in revealing the discovery, is supported by fact. The denial of this statement renders it imperative upon me to state further, that without my knowledge or concurrence, Mr. H. solicited the delay of my annunciation, on the ground that he could perform this service in a more satisfactory manner than I could do. This will doubtless be considered a severe charge, but I have a right to sustain my cause with facts when urged by necessity. Mr. H. complains of the needless severity of my article, or of what he terms my criminations; but they have been alleged openly, and with open right of defence, and under a full conviction of the responsibility attached to them. But a covert interference with my personal concerns, and the attempt to frustrate the record of my discoveries, has awakened in me above all things else, the consciousness that injustice had been done me; for what advantage would be a hundred discoveries in science, if by influence and persuasion the channel of communication should be stopped?

6. The accusation of appropriating the honor of scientific investigation which belongs to Mr. H. demands a few words. I beg Mr. H. to examine candidly whether I have really made such an assumption. It cannot be true that I have ever attempted to wrest from him the great distinction he has acquired in developing the history of footmarks. No man would be more sensible of the presumption of such an act than myself. I only maintain that the first link in the chain of discovery was constructed by me, and that although Mr. H. has prosecuted the subject with brilliant success, still the primary step, most important of all, and absolutely indispensable to him, was first taken by me. His attention was aroused by my repeated letters, and I cannot reverse the opinion that his scepticism was overcome by my exertions.

7. I cannot perceive the necessity of ringing the changes upon my admission of being no geologist. The sincerity with which it was made should have saved me from the severity of criticism. I candidly admit that I am no geologist now. I make not now, nor have ever made any pretensions to a knowledge of this noble science, and I might challenge the proof of having ever made the slightest pretensions to the honor of the discovery in question. In my letter to Dr. Mantell I did not claim it. My language

was, that when the footmarks first attracted my attention I wrote certain letters, but failing to produce a confirmation of my unwavering belief, the real truth was only obvious when I had constructed accurate models of the impressions.

Mr. H. lays great stress upon the five years of labor bestowed upon the investigation of footmarks. This is true, but it was a labor that most men would willingly endure when backed by the patronage of Massachusetts. He was adequately compensated from her treasury, and his expensive work was published by her liberality. Mr. H. declares that I never made explorations until the publication of his Final Report. I will now take the like freedom, and assert the belief that he has not done so since, except in a limited degree, and although he has published several papers upon this subject, he is greatly indebted to other sources for his materials.

8. It is strictly true, that when I wrote my first letter to Mr. H. I had no knowledge that fossil tracks were known, but I wrote in a general sense, that while the discovery was in my own hands I was aware of the fact. I did indeed quote from memory, but had the circumstance been subjected to reflection, most certainly I never should have committed what I now perceive to be, in the strictest sense, an error.

And now finally, in closing this unpleasant controversy, I assure Mr. H. of my willingness to meet him in the same spirit of conciliation he has offered to me. Whatever may be the merits of our discussion, whatever may be the conclusions to which an impartial public may come, still the unexplored field of discovery is ample for us both, and for all others that may choose to enter it. Even now I have before me a new development of these marvellous footsteps, on a scale so stupendous and sublime, as to be well calculated to extinguish the jealousies and selfishness which are among the great infirmities of humanity.

Greenfield, Sept. 24th, 1844.

Art. XVII.—*On the Unionidæ of the River of the Country of the Iguanodon;* by Gideon Algernon Mantell, M.D. F.R.S. &c.

Clapham Common, England, Aug. 24, 1844.

TO PROFESSOR SILLIMAN.

My dear Friend—Although but a few weeks have elapsed since the publication of "The Medals of Creation," in which it is remarked, that "in number, variety, and beauty, the Unionidæ which inhabit the rivers of North America, present a striking contrast with the few and homely British fresh-water muscles; and that in a fossil state there are no shells of this family at all comparable with those of the United States," I have great pleasure in acquainting you that the above observations may now be modified, for I have discovered in the Wealden strata of the Isle of Wight a species of *Unio* as large and massive as are the splendid shells of the Ohio and the Mississippi.

You will probably remember that when you so liberally supplied me many years since with a fine series of the recent species, that I expressed a hope of sooner or later proving that the lakes and rivers of the country of the Iguanodon were tenanted by Mollusca as gigantic as those of America; that wish is now realized. You are aware that several small species of Unio occur in considerable numbers in the sandstones and limestones of Tilgate Forest, and also enter into the composition of some of the varieties of Sussex marble; but the largest species hitherto described (the *Unio Mantellii* of Dr. Fitton) does not exceed two inches in length by one in altitude. During a brief sojourn in the Isle of Wight a few weeks since, I re-examined the Wealden deposits which emerge from beneath the lower arenaceous strata of the chalk, along the southern shore of the island, and extend from near Fresh-water Gate towards Atherfield; a line of cliffs peculiarly interesting to me, from the fine section exposed of the laminated sandstones and shales of the Wealden; and which in that locality abound in the fossil remains of reptiles, mollusca, and plants, peculiar to that formation. On my late visit innumerable fragments of fossil wood were exposed along the strand, having been washed out of the fallen masses of strata by the waves, which at high tides dash against and undermine the base of the cliffs. At low water numerous trunks of trees, from ten to twenty five feet in length, and from two to four or five feet in circumference, were lying prostrate half imbedded in the sand, and partially encrusted with fuci, and flustræ, and corallines. A microscopical examination demonstrates that these trees belong to the *Coniferæ;* and this inference is corroborated by the discovery of several small cones belonging to three or four species of *Pinus* or *Abies.* Here then we have the remains of a petrified pine forest, which once grew in the country of the Iguanodon; for the bones of that reptile, and of others equally gigantic, are associated in considerable numbers with the fossil trees. The trunks of these trees are converted into a compact calcareous stone of an ebony color, often permeated by pyrites, and having the bark in the state of friable lignite; but this investment is soon washed away, and the exposed stems are left bare, their surface displacing the ligneous structure with knots, and remains of branches. I observed one portion of a trunk which indicated a tree of considerable magnitude—not less than ten or eleven feet in circumference.

From among the remains above described, I obtained several examples of a large bivalve, having the external configuration of an *Unio;* but all the specimens were closed shells, filled and firmly cemented together by compact stone, and the structure of the hinge was altogether concealed. With considerable difficulty I have succeeded in separating the valves of one pair of shells, and have developed the characters of the hinge most satisfactorily, by which the nature of these bivalves is clearly demonstrated. These shells are in a remarkably fine state of preservation. The *ligament* remains entire; even the *color* of the original is not entirely obliterated; and traces of the thick internal pearly or nacreous coat of the shell remain. The following description embraces the essential character of these inhabitants of the lakes and rivers of the country of the Iguanodon; the specific name is intended to designate their geological habitat—the *Wealden formation.*

Unio Valdensis, (G. A. M.) The form of this species is ovate, the anterior extremity more rounded than the posterior, which is somewhat narrowed by the ligamental slope. Length (antero-posterior diameter) five inches; height (from summit to base) three inches; diameter (depth or greatest thickness of the united valves) two inches. Equivalves subequilateral; the posterior half being nearly one fifth longer than the anterior, and compressed above along the margin of the ligamental space. The substance of the shell is very thick and strong, and marked externally with concentric longitudinal striæ or grooves. The summit is rounded, rather antero-dorsal, slightly inclined forwards, and the beaks or umbones decorticated, as in most of the recent shells of the genus. The base or circumference is entire; and the margin or internal lip smooth and thick. The ligament is external and post-apicial; its horny character is still preserved, and its surface is marked with transverse rugæ, as in a recent state! The inner surface of the shells is smooth, with the exception of a few irregularities produced by the nacreous deposit being of unequal thickness. The structure of the hinge and the number and situation of the muscular impressions do not materially differ from those of several recent species; in the relative situation of the two cardinal teeth and the lateral tooth-plates, the fossil approaches nearer to the American *Unio purpuriatus*,* than any recent species with which I have been able to compare it.

UNIO VALDENSIS, (G. A. M.)

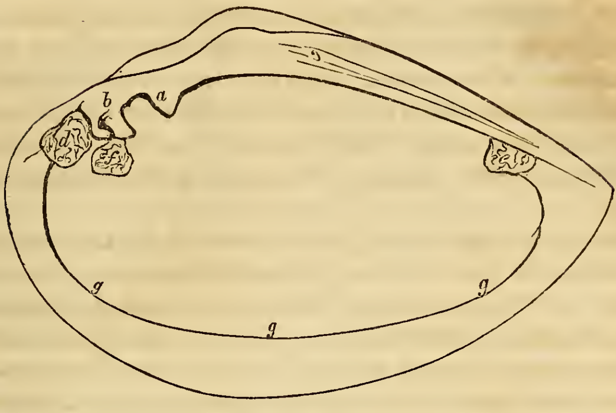
Wealden formation, Isle of Wight.

Fig. 1. View of the left valve, and part of the right seen above: closed.—(Reduced sketch; for dimensions, see bottom of p. 403.)

w, w. Decorticated umbones. *x*. Ligament.

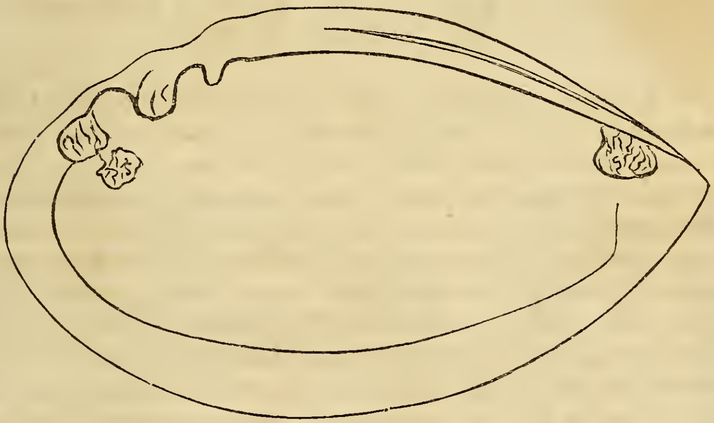
* So named in the British Museum collection.

Fig. 2. Right valve cleared.—(Reduced sketch.)



a. Single cardinal tooth. *b*. Double cardinal tooth. *c*. Lamellar tooth. *d*. Anterior muscular imprint. *e*. Posterior muscular imprint. *f*. Muscular imprint of foot. *g*, *g*, *g*. Palleal imprint.

Fig. 3. Right valve cleared.—(Reduced sketch.) A variety of the above, or perhaps a sexual difference. This shell is shallower than the above, and may have been a male and the others females.



The annexed outline of the right valve (fig. 2) will convey an idea of its character. The tooth-plate is very strong and thick; the cardinal teeth are well developed; there is a deep fosse or hollow beneath the lateral plate. The anterior muscular imprint is deep, and in front of the cardinal plate; having the lesser impression, produced by the attachment of the retractor muscles of the foot, on its inner aspect. The posterior imprint is placed at the extremity of the lamelliform dental plate. The *palleal* impression is distinctly marked, and extends parallel with the margin of the shell, from the anterior to the posterior muscular imprint. The prevailing tint of the shells is a reddish tawny color; when recent they must therefore have closely resembled some of the richly colored species of the Ohio. The weight of a pair of shells, cleared of the stone, is eleven ounces avoirdupois.

This interesting addition to the fauna of the Wealden, tends to confirm the opinion I first advanced in my Illustrations of the Geology of Sussex, namely, that a great proportion of the strata comprehended in that formation, was deposited in the bed of a vast river which flowed through a country inhabited by colossal reptiles, and clothed with forests of palms, ferns, and coniferous trees.

With great regard, my dear friend, yours most faithfully,

GIDEON ALGERNON MANTELL.

ART. XVIII.—*On a supposed New Species of Hippopotamus;* by S. G. MORTON, M. D.*

IT is about six months since I received from my friend Dr. Goheen an extensive series of skulls, of mammiferous and other animals, from Western Africa. They had been obtained by him during a residence of several years at Monrovia, where he had officiated as colonial physician; a situation which gave him great advantages for procuring the natural productions of that region. Among these crania were two of a Hippopotamus, of small size, from the river St. Paul's. Although nothing could be more manifest than the difference between the head of this animal and that of the common species, I have hesitated to publish it, from a fear that some one else may already have done so; for I could hardly convince myself that so remarkable a species was wholly unnoticed in the systems. Having, however, searched the latest European works on zoology without finding any account of this interesting animal, I venture to submit the following facts in relation to it.

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* From the Proceedings of the Acad. Nat. Sci. Phil. Feb. 1844.

HIPPOPOTAMUS MINOR.

Dental Formula: Incisors, $\frac{4}{2}$ or $\frac{2-2}{1-1}$; Canines, $\frac{1-1}{1-1}$

False Molars, $\frac{4-4}{4-4}$; Molars, $\frac{3-3}{3-3}$

	Inches.
Length of the skull, measured from the anterior extremity to the notch between the condyles of the occipital bone, - - - - - - - -	12·3
Zygomatic diameter, - - - - - - - -	8·
Parietal diameter, - - - - - - - -	3·5
Distance between the orbits over the surface of the skull, - - - - - - - - - - -	3·9
Vertical diameter of orbit, - - - - - - -	2·
Horizontal diameter of orbit, - - - - - -	1·8

These measurements have been taken from a very old individual, in which the sutures are entirely obsolete, and the teeth worn almost to the level of the jaw; and the contrast in size, between this and the large or common species, (familiar to every one as the H. *amphibius*, but recently divided into two species, the H. capensis and H. Senegalensis,) will be manifest to every one. The difference, however, is not only in size, but in all the proportions of the head, as the subjoined drawings will show.

Fig. 1.

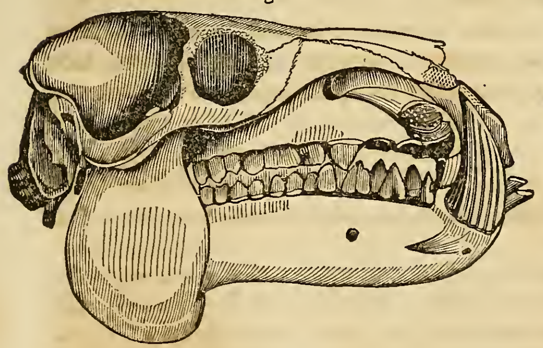
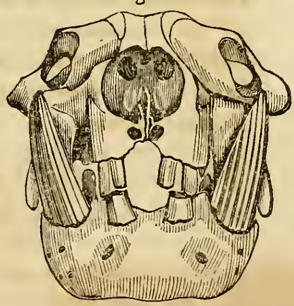


Fig. 2.



In the H. *minor* there is a uniform *convexity* of the upper surface of the cranium, from orbit to orbit, and between the occiput and ossa nasi; while in the common species the orbits are remarkably elevated, and the intermediate surface is *concave*. The orbit is placed about midway between the occiput and snout, and the latter is consequently short; while in the large species the orbits are placed about one third the distance between the occiput and snout. The H. *minor* has only two canines in the lower jaw; the false molars are proximate to the canines; and the base of the zygomæ is in the same plane with the upper maxilla.

The second skull of this species (which is of the same length as the other) is that of a younger animal; for the sutures are open, and the teeth in the process of changing from the deciduous to the permanent set. The posterior molars are only partially protruded, and rise obliquely from the jaws, like those of the Elephant and Mastodon.

Dr. Goheen, who assured me from the first that he could find no notice of this animal in the systematic works, has obligingly favored me with the following memorandum in relation to it. "This animal abounds in the river St. Paul's, and varies in weight from four hundred to seven hundred pounds. It is slow and heavy in its motions, yet will sometimes stray two or three miles from the river, in which situation it is killed by the natives. It is extremely tenacious of life, and almost invulnerable excepting when shot or otherwise wounded in the heart. When injured it becomes irritable and dangerous, but is said by the natives never to attack them when in their canoes. The negroes are very fond of the flesh, which seems to be intermediate in flavor between beef and veal."

My comparisons with the common Hippopotamus have been made on four specimens, (three of which are fully grown,) two from the vicinity of the Cape of Good Hope, and two from the Senegal River.

Art. XIX.—*Bibliographical Notices.*

1. *An Inquiry into the distinctive characteristics of the aboriginal race of America;* by Samuel George Morton, M. D.—Turning aside from the many systems that would derive the American race from the various tribes in the old world, Dr. Morton in his able essay gives us reasons for the belief that they are distinct from all other nations, and that in spite of some singular diversities among themselves, they are originally from one and the same stock. The physical characteristics and osteological conformation present a uniformity that is indeed aston-

ishing, inasmuch as it prevails through such an extent of country; and the few differences with which we meet must be regarded rather as exceptions than as reasons for doubting the general conclusion.

The comparison of "nearly four hundred crania, derived from tribes inhabiting almost every region of both Americas," affords in a greater or less degree the same characteristics in all; "the square or rounded head, the flattened or vertical occiput, the high cheek bones, the ponderous maxillæ, the large quadrangular orbits and the low receding forehead." "This remark is equally applicable to the ancient and modern nations of our continent; for the oldest skulls from the Peruvian cemeteries, the tombs of Mexico, and the mounds of our own country, are of the same type as the heads of the most savage existing tribes. Their physical organization proves the origin of one to have been equally the origin of all. The various civilized nations are to this day represented by their lineal descendants, who inhabit their ancestral seats, and differ in no exterior respect from the wild and uncultivated Indians; at the same time in evidence of their lineage, Clavigero and other historians inform us, that the Mexicans and Peruvians yet possess a latent superiority, which has not been subdued by three centuries of despotism." (p. 5.)

The remains of the ancient Peruvians around Lake Titicaca, from the peculiar form of the head, at first led Dr. Morton to doubt their identity with the other races of this continent; but the researches of M. D'Orbigny have satisfactorily proved that this difference arose only from a custom prevalent among them, of applying lateral compression to the skull. As the heads thus altered are universally those of men, and are found interred in the most elaborate tombs, it is regarded as a mark of distinction.

Our author, in carrying out his argument, next proceeds to trace many points of resemblance in the moral and intellectual natures of the different tribes. The same keen love of war, untiring vigilance, and cruelty in their religious ceremonies, characterize them all. Brought for a long time into close contact with civilized nations, their social condition and manner of life remain almost unchanged. And yet among these rude tribes are found "a people whose attainments in the arts and sciences are a riddle in the history of the human mind. The Peruvians in the south, the Mexicans in the north, and the Messayas of Bogota, between the two, formed these temporary centres of civilization, each independent of the other, and equally skirted by wild and savage hordes." To these nations Dr. Morton gives the collective name of the Toltecan family, "for although the Mexican annals refer their civilization to a period long antecedent to the appearance of the Toltecas, yet the latter seem to have cultivated the arts and sciences to a degree unknown to their predecessors. And as the appearance of the Incas in Peru was nearly simultaneous with the dispersion of the Toltecas in the year 1050 of our era, there is reasonable ground for the conjecture that the Mexicans and Peruvians were branches of the same Toltecan stock." The strange diversity among the intellectual attainments of the various branches of this family, have led philosophers to account for it by supposing a plurality of races. But he inquires, whether we do not observe equally striking contrasts in the wild Arab of the desert, and the Saracen amid the luxury and refinement of Spain.

Dr. Morton dwells upon the general deficiency in maritime enterprise that marks all the American nations. They seem never to have advanced beyond the rudest style of canoe, and he quotes from De Azara a curious fact in illustration of this subject. On the discovery of the Rio de la Plata, its shores were found inhabited by two different nations, who from their inability to cross the river, had never in any way communicated. The manner of interment, which with few exceptions is universal among these tribes, is so different from that practiced by any of the inhabitants of the old world, as to identify them as a peculiar and simple race. The body is placed in a sitting position, with the knees drawn up against the chest, the arms bent, and the chin supported by the palms of the hands. The remains in the sepulchres around Lake Titicaca abundantly prove that this custom has existed from ancient times. Dr. Morton next inquires if these leading characteristics do not prove the race aboriginal to America. The Eskimaux have long been admitted to belong to the Mongolian family, but no similarity can be found to justify the idea that they are the connecting link between the polar inhabitants of Asia and the American tribes. Their physical, moral and intellectual characteristics are quite distinct. The common theory that this continent was peopled by immigration from Asia, is not only controverted by the total dissimilarity in appearance of the races, but it requires us to suppose "one continued chain of colonies during a long succession of ages" making their way over a tract of eight hundred miles, and among a great variety of languages; and, inquires Dr. Morton, "how does it happen that during the lapse of three hundred years, since the discovery of America, there has not been an authenticated immigration from Asia?"

Many of the same objections are urged against the theory of a Malay origin; and the comparison of languages only heightens the difficulties. "Once more," says our author, "I repeat my conviction that the study of physical conformation alone excludes every branch of the Caucasian race, from any obvious participation in the peopling of this continent. In fine, our own conclusion long ago derived from a patient examination of the facts thus briefly and inadequately stated, is, that the American race is essentially separate and peculiar, whether we regard its physical, moral, or intellectual relations. The evidence of history and the evidence of the Egyptian monuments, go to prove that these races were as distinctly stamped three thousand five hundred years ago as they are now; and in fact that they are coeval with the primitive dispersion of our species."

2. *The Journal of the Boston Society of Natural History*, Vol. IV, No. 4, pp. 377 to 512. Pl. 17 to 24.—The contents of this number are as usual chiefly zoological, and embrace a variety of interesting topics. The articles in this department are the following.

"On the external characters and habits, and on the organization of *Troglodytes niger*, *Geof.*, by THOMAS S. SAVAGE and JEFFRIES WYMAN, M. D." This is the conclusion of a paper commenced in a preceding number. The anatomical details by Dr. Wyman are characterized by his usual care and accuracy, and tend to confirm the observations of Prof. Owen on the same animal. Its distinctive characteristics are now well understood. The remarks of Dr. Savage on its habits possess a peculiar interest, from the opportunity which a residence of several years on the western coast of Africa, as a missionary, has afforded him for observing these animals in their natural condition. It appears that their habitation is chiefly in trees, where they construct a slight nest of broken twigs and branches, usually resting on a large limb or in a crotch, but sometimes suspended near the end of a leafy branch twenty or thirty feet from the ground. They are timid and inoffensive to man, and feed chiefly upon vegetables and fruit. The canine teeth are uncommonly developed and serve as a means of attack and defense, but do not indicate carnivorous propensities. They are very filthy. The degree of their intelligence appears to us to be overstated, and is probably derived in part from the imagination of observers.

"Descriptions and figures of the *Araneides* of the United States, by NICHOLAS MARCELLUS HENTZ." This elaborate article by Dr. Hentz is illustrated with excellent figures, and his descriptions are ample and in general accompanied with remarks on the habits of the respective species. The species described are the following: LYCOSA *lenta*, *ruricola*, *saltatrix*, *erratica*, *littoralis*, *maritima*, *aspersa*, *riparia*, *punctulata*, *scutulata*, *sagittata*, *ochreata*, *venustula*, *milvina*, *saxatilis*, *funerea*. CTENUS *hybernalis*, *punctulatus*. DOLOMEDES *tenax*, *hastulatus*, *tenebrosus*.

"Description of an African beetle allied to *Scarabæus polyphemus*, with remarks upon some other insects of the same group, by THADDEUS WILLIAM HARRIS." This paper contains an account of the group of magnificent insects known as the Goliath beetles, with copious remarks on their natural history, derived mostly from Dr. Savage, by whom they were brought from the western coast of Africa. Dr. Harris has here described the female of *Mecynorhina Polyphemus*, which was not known until brought to notice by Dr. S., and institutes a new species under the name of *Mecynorhina Savagii*, from male and female specimens derived from the same source.

"The importance of habit as a guide to accuracy in systematical arrangement, illustrated in the instance of the *Sylvia Petechia* of Wilson, &c., by T. McCulloch, Jr., of Halifax, Nova Scotia." The author of this paper takes a very just view of the insufficiency of external characters alone for the accurate discrimination of allied species, and insists upon an acquaintance with internal structure, stating also that in its absence, *habit* may sometimes serve as a guide in finding the true place of a species. Apart from the main object of the paper, which is well sustained, the observations of the author upon the Sylvia Petechia form a valuable supplement to the history of this species.

"On the anatomy of *Tebennophorus Carolinensis*," and "on the anatomical structure of *Glandina truncata*," two papers by Jeffries Wyman, M. D. The object of these papers is to show that the internal structure of these animals, justifies their separation from the genera in which they have hitherto been repeatedly placed, and their institution as the type of distinct genera. *Tebennophorus* was founded on the animal described by Bosc as *Limax Carolinensis*, and unless all the naked slugs are to be included under one genus, we cannot doubt the propriety of its adoption. The details of its structure vary considerably from those of any other genus. So too with *Glandina truncata*, both externally and internally it differs as much from every other genus, as any two do from each other; and the possession of a third pair of tentacles stamps it with a marked peculiarity.

"Description and habits of some of the birds of Yucatan," by Samuel Cabot, Jr., M. D. p. 460. Dr. Cabot accompanied Messrs. Stevens and Catherwood in an expedition to Yucatan, and the present remarks are among the results of his observations. He gives some particulars of the habits of *Ortyx nigrogularis* of Gould, and an extended description of the female, which was not seen by the latter. Dr. Cabot brings forward as new to science, four species of birds, of which he gives full descriptions, taken both from males and females, with notices of their habits, and an account of some of his own adventures when procuring them. The species proposed are *Falco Percontator*, *Corvus vociferus*, *Oriolus musicus*, *Momotus Yucatacensis.*

"Enumeration of the recent fresh-water mollusca which are common to North America and Europe; with observations on species and their distribution," by S. S. Haldeman. The species supposed to be common to the two continents are,—1. Paludina vivipara, Linn. 2. Paludina fasciata, Müll., *P. achatina*, Lam. 3. Physa hypnorum, Linn., *P. elongata*, Say. 4. Limnea palustris, Müll., *L. elodes*, Say, *L. stagnalis*, Linn.? *L. jugularis*, Say, *L. truncatula*, Müll. 5. Planorbis albus, Müll., *P. nitidus?* Müll. 6. Cyclas calyculata, Drap. 7. Pisidium appendiculatum, Leach, *P. amnicum*, Müll. 8. Alasmodon margaritiferum, Linn. The author names four causes affecting the dispersion of the same species into different regions, viz. transportation, former connection of the regions, distribution from several original centres, and transmutation. On the first three of these but few remarks are made, but on the fourth, which is not strictly a means of distribution of identical objects, various reflections are ventured. The author leans to the doctrine of transmutation, but we do not see the force of his reasoning, even to the limited extent to which he goes.

"Descriptions and notices of some of the land shells of Cuba," and "Descriptions of land shells, from the province of Tavoy, in British Burmah," two papers by Augustus A. Gould, M.D. The first named paper consists principally of descriptions of the *animals* of known species, with notices of their habitats, and other information relating to them, together with some corrections of existing errors; these being mostly derived from the memoranda of a gentleman long resident in the island. The following species are brought forward as new, and good figures are given of each of them. *Pupa* (*Siphonostoma*) *porrecta*, *S. lactaria*, *Planorbis dentatus*. The most interesting points of this paper are, that Dr. G. sees occasion to unite into one species the three following. *Helicina submarginata*, Gray, *H. Sagra*, D'Orbigny, and *H. pulcherrima*, Lea; and that the animal of *Glandina oleacea*, Fer., corresponds with that of *G. truncata*, Say. In the list of species there are three which are common to the United States as well as to Cuba, viz. *Helix septemvolva*, Say, *Pupa contracta*, Say, *Pupa rupicola*, Say, (P. servilis, Gould.)

The paper on the shells of Tavoy contains descriptions of the following species, Helix *procumbens*, *infrendens*, *gabata*, *anceps*, *retrorsa*, *Petitii*, Vitrina *præstans*, Bulimus *atricallosus*, Clausilia *insignis*, Cyclostoma *pernobilis*, *sectilabrum*. X.

3. *Actonian Prize Essay. Chemistry, as exemplifying the wisdom and beneficence of God;* by George Fownes, M. D., *Chemical Lecturer in the Middlesex Hospital Medical School.* New York, Wiley and Putnam, 1844. 12mo, pp. 158.—This ingenious and well written little essay is the first of a series on a plan similar to the Bridgewater treatises, and to be continued septennially, founded on the liberality of the late Samuel Acton Esq., of Euston Square, London, who invested one

thousand pounds in the three per cent. consol bank annuities, in the names of the trustees of the Royal Institution of Great Britain, the interest of which was to be devoted to the formation of a fund out of which the sum of One Hundred *Guineas* was to be paid septennially, as a reward or prize to the person who, in the judgment of the committee of managers, for the time being, of the Institution, should have been the author of the best essay illustrative of the wisdom and beneficence of the Almighty, in such department of science as the committee of managers should, in their discretion, have selected. The subject chosen for the prize of the first seven years, was the title of the present volume. The prize was awarded in April last to the present essay.

The subject was perhaps a novel one for an essay of this nature, but it has proved in the hands of Mr. Fownes a rich mine, from which he has drawn the most satisfactory and delightful evidence of that wise beneficence, which is seen in every department of the works of our Great Author.

We have not space to present an analysis of the argument, but having read the volume with great pleasure and profit, we cordially recommend it to the attention of all who are interested in works of this nature.

4. *A Manual of Chemistry; containing a condensed view of the science, with copious references to some extensive treatises, original papers, &c., intended as a Text-Book for Medical Schools, Colleges, and Academies.* By Lewis C. Beck, M. D., &c. 4th edition. New York, W. E. Dean, 1844. pp. 480, 12mo.

The issue of a fourth edition of Dr. Beck's Manual is presumptive evidence of its adaptedness to the objects of the author. Being printed in a small condensed type, the amount of matter contained in it is much greater than is usually found in similar works, and the full references to original authorities which accompany each section, renders it a truly valuable book to the teacher. Its arrangement is in the main similar to the former editions of Turner's Chemistry, which has been so popular as a text-book in this country. The present edition has been revised, to render it as complete a view as possible of the present state of the science.

5. *The Principles of Chemistry: prepared for the use of Schools, Academies, and Colleges.* By Daniel B. Smith. 2d edition. pp. 312, 12mo. Uriah Hunt, Philadelphia, 1842.

This unpretending little volume is well worthy the attention of teachers, as being a most concise and at the same time clear exposition of the main facts and reasonings of chemistry. It is not a condensation or abridgment of any larger work of an European author, (as most American text-books of this sort are,) but the offspring of a mind well imbued with the science, and capable of presenting it in a forcible and attractive style. No experimental details or figures of apparatus are given, and it is evidently intended by the author that these must be supplied by the oral instructions of the teacher, who has access for the purpose to more extended works. The separation of chemical philosophy from chemical manipulation seems to be the leading feature of Mr. Smith's creditable work.

6. *New Books received.*—The Medals of Creation; or, First Lessons in Geology, and in the study of Organic Remains. By Gideon Algernon Mantell, Esq., LL. D., F. R. S., author of the Wonders of Geology, etc. Two volumes, with colored plates, and several hundred figures of fossil remains. These beautiful volumes have reached us only at the last moment, and all notice of them must be postponed to our next.

On Dinornis,* an extinct genus of tridactyle Struthious Birds, with descriptions of portions of the skeleton of six species which formerly existed in New Zealand. By Prof. Owen, M. D., F. R. S., Z. S., &c. Part I.

Geological Observations on the Volcanic Islands visited during the voyage of H. M. S. Beagle, together with some brief notices on the Geology of Australia and the Cape of Good Hope; being the second part of the Geology of the voyage of the Beagle, under the command of Capt. Fitzroy, R. N., during the years 1832 to 1836. By Charles Darwin, M. A., F. R. S., Vice President of the Geological Society, and naturalist to the expedition. London: Smith, Elder & Co., 65 Cornhill. 1844. pp. 175, 8vo.

Transactions of the New York State Agricultural Society, together with an abstract of the Proceedings of the County Agricultural Societies. Vol. III. Albany, Carroll & Cook, 1843. pp. 671, 8vo. This volume contains, besides much valuable agricultural matter, a condensed review of the labors of the New York geological corps, from the pen of Mr. James Hall, which is an interesting paper, fully illustrated by figures of fossils.

Lectures on Polarized Light, delivered before the Pharmaceutical Society of Great Britain. pp. 110, 8vo. London, 1844.

Researches on Light; an examination of all the Phenomena connected with the chemical and molecular changes produced by the influence of the solar rays; embracing all the known photographic processes and new discoveries in the art. By Robert Hunt, Secretary to the Royal Cornwall Polytechnic Society. London, printed for Longman & Co., 1844. pp. 303, 8vo.

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* "Δεινὸς, *surprising*, and ὄρνις, *bird*."

MISCELLANIES.

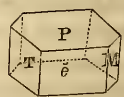
DOMESTIC AND FOREIGN.

1. *Extract of a letter from* Prof. Hitchcock, *respecting the Lincolnite.*—I wish to say a few words in arrest of judgment respecting the Lincolnite, which Mr. Alger has endeavored to show, in Vol. xlvi, p. 235, of this Journal, to be identical with Heulandite. His paper is also published in the last No. of the Boston Journal of Natural History.

This mineral was found by me, only in small quantity, more than thirty years ago; and I had revisited the spot only once in that interval. I then procured, within a few rods of the spot, some very minute crystals, which I judged by the eye, to be Lincolnite; and I found a like mineral on gneiss at Bellows' Falls. But I never attempted to measure these minute crystals. Those which I first obtained were much larger. Yet when Mr. Alger requested specimens, I found that I had lost or parted with nearly every one, except these small ones; and I believe that these were the variety placed in the State collection. Now it was probably these which were measured by Messrs Teschemacher and Alger, and they may very probably have been Heulandite. But I still feel quite confident that the original crystals which I measured, differed from Heulandite by about 10°. After reading Mr. Alger's remarks, I searched among my duplicates, and found two isolated crystals of those first obtained; and on measuring them, by marking their angles on a smooth hard surface, I found them to be nearly 60° and 120°, as stated in my Report. More recently I have discovered a still better specimen in the cabinet of Amherst College; and at my request, Professor Shepard has applied the reflecting goniometer to some of the crystals, and finds the angles of the base to be from 116° 45′ to 117° 15′, as taken by the reflection of a strong lamp-light.

I have never felt, or expressed, any great confidence that this species would maintain its ground; but it strikes me that as yet it has not been identified with any other species. E. H.

The primary lateral angle of Heulandite (M : T) equals 130° 30′, and differs widely, as Mr. Hitchcock states, from the angle obtained by Prof. Shepard for Lincolnite. But the secondary prism formed by the planes T and ĕ, has for the lateral angle 115° 10′, to which Prof. Shepard's angles are as close, as could be expected from the mode of measurement. By lettering the second of Prof. Hitchcock's figures, (Rep. Geol. Mass. p. 663,) as annexed, the relation to Heulandite is apparent. The plane ĕ is that lettered *f* in most mineralogical works, and the figure is turned around, so that this plane is brought in front.—Eds.



2. *Singular crystals of Lead from Rossie, N. Y.*—These crystals, figured in Alger's and Dana's Mineralogy, have received the following explanation by Mr. J. E. Teschemacher, (p. 500, Vol. IV, Bost. Jour. Nat. Hist.) While crystallizing from the melted state, "the uncooled liquid central portion pressed by the contraction of the cooling exterior, oozes out from the middle of the plane and spreads in a thin liquid plate over part of the surface, taking nearly the form of the plane; contraction still continuing, a succession of thin plates ooze out, each of course spreading somewhat short of its predecessor, but retaining the same form." He illustrates it by remarking, that on examining with a microscope the cooling of a globule of phosphate of lead, "the angles of the planes appear to start out from the circumference, as the outer surface of the globule cools and the planes to flatten into their symmetrical shape."

But these crystals of galena are cleavable to the very centre like others of this mineral, showing that instead of this oozing from the interior, they were formed throughout by successively applied laminæ. The peculiar distorted form appears to be attributable to their confinement in the limestone, already partially hardened around them when they were crystallizing, and to the peculiar laws of crystallization by which planes are added to the surfaces of a primary—this intercepted mode of modification being by no means uncommon.

3. *Dipyre.*—(Extract from an article by M. Achille Delesse, Comptes Rendus, 1844, May 20, p. 944.)—This name was given by Laumont and Charpentier to a mineral from the vicinity of Mauléon in the Lower Pyrenees, but has since been referred to the species Scapolite. It occurs in square or regular 8-sided prisms, cleavable parallel with the lateral faces and the diagonal. The crystals are usually transparent and vitreous, though sometimes partially decomposed and opaque. It occurs with talc or chlorite, or an unctuous argillite. The mineral was imperfectly analyzed by Vauquelin. I obtained for the mean of four analyses, silica 55·5, alumina 24.8, lime 9·6, soda 9·4, potash 0·7. From the large proportion of alkali, the mineral appears to be allied to the feldspars. Von Kobell, some time since, referred it to Labradorite, but the system of crystallization is not the same; the specific gravity of Labradorite is 2·714, and that of dipyre 2·646; moreover the composition, although similar, is still essentially different, and appears to show that it is distinct also from Scapolite. The formula it affords is the following,—

$$2(\dot{Ca}, \dot{Na}, \dot{K})\dddot{Si} + 3\dddot{\mathbf{Al}}\,\dddot{Si}.$$

Before the blowpipe dipyre loses its transparency and fuses with a slight effervescence, yielding a white blebby glass. Melts easily with salt of phosphorus, and the pearl formed contains a floating skeleton of silica.

Forms easily a limpid glass with soda. The strongest acids attack it, when finely powdered, with difficulty.

4. *Blowpipe characters of the supposed Pyrrhite of the Azores;* by J. E. Teschemacher. (Bost. Jour. of Nat. Hist., Vol. IV, p. 499.)—The minutest transparent crystals change immediately in the reducing flame to a deep, dull indigo blue, perfectly distinct; the edges then rounded, and, after considerable exposure, fused without intumescence; on the application of borax, the fusion was immediate, and a small transparent light brown bead remained. The largest crystal was then exposed to the outer flame; it became opaque, of a light gray color; before the reducing flame it changed apparently to black; but the blue color is clearly seen, in a strong light on the solid angles. Of this crystal, the edges alone could be rounded on long exposure.

5. *Formula of the Pink Scapolite of Bolton.*—Dr. C. T. Jackson (Bost. Jour. Nat. Hist., IV, 504) deduces the following formula from his late analysis of that mineral, (the silica, alumina and alkalies, having the ratios of 4, 2, 1, respectively.) Formula, $2Al\,Si + (Ca, Na, Li)\,Si^2$; or in chemical symbols, $2\dddot{\mathbf{A}}l\,\dddot{Si} + (\dot{Ca}, \dot{Na}, \dot{Li})^3\,\dddot{Si}^2$.

6. *Head of Carpinchoe.*—Lieut. H. C. Flagg, U. S. N., of New Haven, has recently presented to the Natural History Society connected with Yale College, a fine skull of the Cabybara or Carpinchoe (*Hydrochærus cabiai*) from Guiana, the largest animal of the class Rodentia: also the well preserved skin of an albatross.

7. *Natural Polariscope.*—In the mica quarries at Grafton, New Hampshire, where this mineral is obtained in large quantities for stove fronts and other economical purposes, black tourmalines are frequently found compressed between the laminæ of mica. In looking over a large quantity of this mica, I found several specimens where the tourmalines were so thin as to be transparent and of a fine clove-brown color, although the crystals are ordinarily quite black. The thought at once suggested itself that we were here provided by nature with the means for polarization, and no time was lost in constructing of two thin tourmalines and a piece of the binaxial mica in which they are imbedded, a very good instrument. It will be observed that the compression of the tourmalines has taken place in a plane perfectly parallel to the vertical axis of the prism, and consequently in the right direction to ensure the maximum effect. It is feared that the color of the tourmalines will be found too dark to allow of their general use, as they must be made very thin in order to admit light enough to pass. But the fact is an interesting one, that nature should have anticipated the construction of one of the most refined of modern optical instruments. B. S. Jr.

8. *A new Comet.*—By the annexed extract from the Cleveland (Ohio) Daily Herald of Sept. 24, 1844, it appears that Mr. HAMILTON L. SMITH has had the good fortune to discover a comet, which, so far as we know, has not been detected by the European astronomers.

"Mr. HARRIS,—Below I give you the apparent places of a telescopic comet which I have been observing for nearly two weeks, and of which I have not seen any account published. The observations are as accurate as my means for determining them would allow. It was first seen on the 10th of September, at which time, with a night glass of 2½ inches aperture, it was in the same field with *Beta Ceti*, and about 12° above the horizon at 11 o'clock P. M. The tail was about a degree in length, and the nucleus very bright, and presented a fine appearance in a 44 inch achromatic.

		h.	*m.*		*deg.*	*m.*
Sept. 10,	A. R.	24	31·75,	Dec. S.	15	26
" 11,		"	34·25,		14	54
" 12,		"	36·75,		14	23
" 13,		"	39·25,		13	52
" 14,		"	41·50,		13	22
" 15,		"	43·75,		12	52
" 16,		"	46·00,		12	23
" 17,		"	48·25,		11	54
" 18,		"	50·25,		11	25
" 19,		"	52·25,		10	56
" 20,		"	54·25,		10	27
" 21,			cloudy,		cloudy.	
" 22,		"	58·25,		9	30

H. L. SMITH."

9. *Fluorine in Bones.*—Dr. Rees has examined recent human bones before and after calcination, without finding a trace of fluorine.

J. Middleton found fluoride of calcium in the bones of an ancient Greek, a mummy, and the bones of fossil vertebrata from the Sivalic hills, and ascertained that the proportions increased according to the age. He also found it in aqueous deposits of different kinds and ages. He refers its presence in bones to deposition from fluids, and hence accounts for its great abundance in fossil bones, which had been long exposed to aqueous infiltration.—*Phil. Mag.* 1844, xxv, 119, 222.

Berzelius and Morichini, previous to the above investigations, asserted the presence of fluorine in recent bones. Dr. Daubeny, in view of the conflicting statements, has made new investigations with the following results. By freeing the bones of animal matter and of carbonates, and obtaining the phosphates separate, and treating the latter with sulphuric acid, he succeeded in engraving upon glass, not only by means of fossil bones from Stonesfield, from Montmartre, from the cave of Kirkdale in Yorkshire, and from that of Gailenreuth in Franconia, but likewise with the bone of some quadruped that had been lying for a long but unknown time exposed to the weather in the soil of the Botanic Garden, with the vertebra of an ox recently killed, and with human teeth of recent date. The tibia of a human subject gave indications almost as distinct as any even of the fossil bones operated upon. —*Mem. and Proceedings of the Chemical Society*, *Part* 8, *p.* 97.

10. *New Project for Reforming the English Alphabet and Orthography.*—On a former occasion, (Vol. xxxix, p. 197,) we stated the legitimate objects of an improved English orthography, and noticed several proposals for attaining this end. We proceed now to state the plan of Rev. Ezekiel Rich, of Troy, New Hampshire, contained in a memorial to Congress, and published in the Documents of the House of Representatives, 28th Congress, 1st session, Doc. No. 126.

1. Mr. Rich finds in the English language sixteen vowel sounds and twenty four consonant sounds. Among the vowel sounds he reckons *u* in *duke*, and *i* in *pine;* and among the consonant sounds *ch* in *church*, and *j* in *just*. Of course his object is not to discover the proper simple sounds in the language.

2. Instead of adopting one simple character for each simple sound only, Mr. R. proposes simple characters also for the diphthongs *ou* and *oi*, and the double consonants *ks* and *gz*, and even for whole words. He thus mars the symmetry of his system.

3. He employs a temporary character, such as the types now in use would permit, to illustrate his principles. But his characters are ill chosen, and indicate no special familiarity of the author with the problem before him.

4. He says nothing of expressing analogous sounds by analogous forms. He seems not to have directed his mind to this point. Yet it is important.

5. He thinks that in the choice of a new character, great regard should be had to ease and convenience of writing; but he makes no suggestion as to their particular form.

6. He endeavors to simplify the names of the letters by using the sounds of the vowel for their names, and by using double names for the consonants, according as they occur before or after the vowel of the syllable; as *bē* for the initial sound in *bad*, and *ĕb* for the final sound in *rub*.

We ought to add that the reverend author is zealous in a good cause, and has exhibited in a strong light the defects of our present orthography, and the desirableness of a reformation.

INDEX TO VOLUME XLVII.

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.

Bulletin de l'Academie royale des sciences et belles lettres de Bruxelles, Année, 1842. Tom. ix, 1re partie, 2m partie, et tom. x, 1re partie, de la Academie. Bruxelles.

Nouveaux memoires de l'Academie Royale des Bruxelles. Tome xvi, 1843. From the same; received Jannary, 1844.

Bulletin de la Societé Geologique de France. Paris. Tome douzierne, 1840 á 1841, de la societé.

Annales des sciences Geologiques, publiées par M. A. Rivière premiere année. Paris. From the editor, with a proposal to exchange.

Revue des fossiles du government de Moscow, par C. Fischer de Waldheim. No. 2. Fossiles du terrain oolithique. Extrait du Bull. de la Soc. imper. des Natural. de Moscow. Tome xvi, 1843. From Chas. Cramer, Esq.

Geology; by D. Thomas Ansted, M. A., Prof. of Geology in King's Coll. London. Part. I. Feb. 1844. Rec'd Feb. 27.

Experimental researches on the compound nature of carbon, by Robert Riggs, F. R. S. Octavo, pp. 264. London, 1844. Smith, Elder & Co. From the publishers.

Sketch of the Analytical engine invented by Charles Babbage; by L. F. Menabrea of Turin, with notes by the translator. London, 1843. From Mr. Babbage.—Addition to the memoir of M. Menabrea, on the Analytical engine. Scientific Memoirs, vol. iii, part 11, pp. 566.

Thoughts on the causes of compass variation, by Peter Cunningham, surgeon R. N. London, 1843. From Dr. Mantell. Received Nov. 1843.

Manchester Geological Society. Fifth annual meeting, Oct. 26, 1843. From the society, Jan. 1844.

Report of a committee appointed to consider the rules by which the nomenclature of zoology may be established on a uniform and permanent basis. Feb. 1842. From the British Association.

Report of the mineralogical observations made on the isle of St. Helen, in the river St. Lawrence, by John S. M'Cord. Montreal, 1842.

Haudbuch einer Geschichte der natur. Von Profs. G. Bronn, Erster, and Zweiter Bauds. 8vo. pp. 448 and 836.

Verbreitung und Einflus des Mikroskopischen Lebens in Süd- und Nord-Amerika, Ein vortrag von C. G. Ehrenberg, (nebst 4 colorirten Kupfertafeln.) 4to. pp. 158. Berlin, 1843. From the author.

Magnetical Investigations, by the Rev. William Scoresby, B. D., F. R. S., and E. &c. &c. Part 1, 8vo. pp. 92. London, 1839. Longman & Co.

Do. Part 2, 8vo. pp. 364. From the author.

On the connexion of Geology with Terrestrial Magnetism, by Evan Hoppins, C. E., F. G. S. 24 plates. London. R. and J. E. Taylor. 1844. 8vo. pp. 129.

List of English fossils. 8vo. London, 1843. From Dr. Mantell.

Revista Ligure: Genova. Forwarded regularly by C. Edwards Lester, Esq., U. S. Consul at Genoa. 1843.

Memoires of William Smith, LL. D., author of the map of the strata of England and Wales, by John Phillips, F. R. S. London, 1844. From the author.

Memoires de la Societé Royales des Antiquaires du Nord. 1840–1843. Section Americaine. Copenhagne, 1843. From the society.

SCIENCE.—DOMESTIC.

Transactions of the American Philosophical Society, vol. ix, part 1, 1844. From the society.

Tables of organic remains from the geology of the fourth district of New York. From and by James Hall.

A System of Mineralogy, by James D. Dana, A. M. Second edition, octavo; pp. 633. New York and London. Wiley & Putnam, 1844. From the author.

Monograph of the fresh water univalve Mollusca of the United States, by S. Stehman Haldeman. No. 7, 1844. Two copies from the author.

Zoological contributions, by S. S. Haldeman. No. 2, 1843. Phil. From the author.

Le Sueur's philosophy of the Pantonomic system of the universe. Book first. 1843. Hartford. From the author.

Observations on vegetable and animal physiology, by W. L. Wright, M. D. Petersburg, 1843. From the author.

On the organic functions of animals, by Jas. Moultrie, M. D. Charleston, S. C. 1844. Two copies from the author.

Mr. Wheelwright's report on the steam navigation in the Pacific, with an account of the coal mines of Chili and Panama. 1843.

The Encyclopedia of Chemistry, by J. C. Booth. Philadelphia. Nos. 3, 4. 1844.

No. 13, of Conrad's Unionidæ. From J. R. Dobson. Two copies.

Meteorological register. From Roswell Marsh.

MISCELLANEOUS.—DOMESTIC.

Dr. Haddock's discourse before the Rhetorical Society in the Theological Seminary at Bangor, Me. Aug. 30, 1843. From the author.

An address delivered before the Railroad Convention at Lebanon, N. H., Oct. 10th 1843, by Charles H. Haddock, D. D. Hanover, 1843. From the author.

The key to nature's Laboratory, by Smith Oliver. New York.

An address before the Alumni Society of the University of Nashville, Oct. 3d, 1843, by the Hon. John Bell, A. M. Nashville, 1844.

Speech of Cassius M. Clay against the annexation of Texas to the United States of America, Dec. 30th, 1843. Lexington.

Annual report of the Commissioner of Patents for 1843. Washington. From the Hon. H. W. Ellsworth.

Report of Capt. G. W. Hughes of the Topographical Engineers, relative to the working of copper ore. April 9th, 1844. Washington, D. C.

Report of the late F. R. Hassler relative to the operations and condition of the coast survey. Jan. 31st, 1844.

Annual report of the Secretary of the treasury on the state of the finances. Dec. 6th, 1843. From Hon. J. H. Lumpkin.

Report of the committee on roads and canals, to the 28th Congress. April 3d, 1844.

Letter of Mr. Walker of Mississippi relative to the annexation of Texas. Washington, D. C.

Report of the committee on public charitable institutions to the House of Representatives of Massachusetts.

Address before the society of Natural History of the Auburn Theological Seminary, Aug. 15th, 1843, by James Hall, A. M. Auburn, 1844. From the author.

Prairie Farmer, Chicago. March—April, 1844.

Army and Navy Chronicle. Washington, Thursday March 7th, and Feb. 8th.

Transactions of the historical and literary committee of the American Philosophical Society. Vol. iii, 1843. Phil. From J. R. Tyson, Esq.

Fifty seventh annual report of the regents of the university of the state of New York. Albany, 1844.

Southern Literary Messenger, from Nov. 1843 to May, 1844. Richmond, Va.

The Enquirer. Vol. i, No 3. Dec. 1843. Albany.

Compendium of the enumeration of the inhabitants and statistics of the United States. Washington. From the Secretary of State.

Proceedings of the American Antiquarian Society, at their thirty first annual meeting at Worcester, Mass., Oct. 1842, and at their fifty second semi-annual meeting at Boston, May, 1843.

Twenty third annual report of the board of directors of the Mercantile Library Association, Clinton Hall, New York. Jan. 1844. Two copies.

Twenty fifth annual report of the directors of the New York Institution for the instruction of the deaf and dumb. 1844. New York. From D. Bartlett.

Annual report of the trustees of the Perkins institution and Massachusetts asylum for the blind. 1844.

Eighteenth annual report of the board of managers of the Prison Discipline Society. Boston, 1843.

Report of the Pennsylvania Hospital for the insane, for 1843; by T. S. Kirkbridge, M. D. Phil. 1844.

Review of Dr. Caldweld's pamphlet, entitled Physiology vindicated, by Robert Peter, M. D. Cincinnati, 1843.

Lecture introductory to a course of chemistry in the University of Pennsylvania, Nov. 7th, 1843, by Robert Hare, Phil. 1843. Three copies from the author.

District School Journal. Feb.—April, 1844. Albany.

Lecture before the Boston Historical Society, by President Talmage of the Oglethorpe University.

Arts, sciences, and civilization anterior to Greece and Rome, by R. W. Haskins, A. M. Buffalo, 1844. From the author.

An examination into the charter of Trinity church.

Analytical report of a series of experiments in mesmeric somnilœquism, performed by an association of gentlemen, with speculations on its phenomena, by Dr. Drake. Louisville, 1844.

Two years experience in the employment of magnetic electricity as a remedial agent in disease, by Dr. N. Walkly. Tuscaloosa, Ala. 1844. From the author.

Medical almanac for 1844. New York.

Prince's catalogue of trees. New York, 1844.

Catalogue of Jefferson Medical Coll. of Philadelphia. Phil. 1844.

Catalogue of the University of the state of Alabama. 1844. From Prof. A. P. Barnard.

Catalogue of the members and library of the Gœthean Literary Society of Marshall Coll. Petersburg, Pa. 1844.

Catalogue of Western Reserve Coll. From Prof. St. John. 1844.

A decree in chancery.

Constitution, by-laws, &c. of the Maryland Historical Society. Baltimore, 1844.

American statistical association. Boston, 1844.

Address on insanity by Dr. White. Albany, N. Y.

A letter to a lady in France in answer to inquiries concerning late imputations of dishonor upon the United States, by T. G. Cary, Esq. From the author.

Prospectus of the Trinity coal and mining company. New York, 1844. From Dr. Page.

Love to the doctrines of the Bible an essential element of Christian character, by Rev. Ed. W. Hooker. Phil. 1844.

The dead are the living—a funeral sermon for Mrs. Ward Stafford, by Rev. Dr. Cox. New York, 1843.

The law of Christian rebuke—a sermon delivered at Middletown, Conn., by the Rev. J. Burt, before the anti-slavery convention of ministers and other Christians. Oct. 18th, 1843.

A discourse by Rev. S. Elliott, Jr., on occasion of the fifth anniversary of the Georgia Historical Society. Feb. 1844.

Supplement to the spirit of the nineteenth century, for 1843, by Robert J. Breckinridge.

Modern school geography with an atlas, by Wm. C. Woodbridge, and Woodbridge's and Willard's universal geography, with modern and ancient maps. Hartford, 1844. From the author.

Report of the commissioner of the general land office relative to the probable cost per mile of surveying township lines to the mineral lands on Lake Superior. May 4, 1844. From Hon. L. Lyon.

Memorial of the citizens of Cincinnati to the Congress of the United States, relative to the navigation of the Ohio and Mississippi Rivers. From Paul Audubon, Esq. 1844.

A letter to the people of the state of Illinois on the subject of public credit, by a citizen of Chicago.

Report of the committee on naval affairs to whom were referred certain communications from the war and navy department, on the subject of large iron wrought guns. May 15, 1844. From Hon. O. Baker, M. C.

Sermon preached on the day of the annual fast, April 5th, 1844, by Rev. Lyman H. Atwater of Fairfield. From the author.

Seventh annual catalogue of the Med. Institute of Louisville. 1844.

Annual report of the Connecticut State Colonization Society, adopted at their meeting held in New Haven, May 22, 1844. Hartford.

Speech of his Excellency, Roger S. Baldwin, governor of Connecticut, to the legislature of the state, May, 1844.

The Prairie Farmer, late the Union Agriculturalist and Western Prairie Farmer; edited by J. S. Wright and J. Ambrose Wright, June, 1844. From the editors, with a notice of this Journal. Chicago. Vol. iv, No. 6.

Analysis of cotton wool, cotton seed, indian corn, and the yam potatoe, made for the Black Oak Agricultural Society, by Prof. C. U. Shepard. Charleston, 1844. From the author.

Constitution and by-laws of the Mechanical Institute of St. Louis, with the names of the officers of the Institute for the year 1843. St. Louis.

Physiology vindicated in a critique on Liebig's animal chemistry, by Charles Caldwell, M. D. From the author. Louisville, Ky.

MISCELLANEOUS.—FOREIGN.

Report of the Medical Missionary Society. Macoa, China.

NEWSPAPERS.—DOMESTIC.

New York Daily Tribune, April, 1844—discovery of cobalt in Missouri. American Intelligencer, Phil., April, 1844. Baltimore Clipper, Nov. 1843. Boston Cultivator, April, 1844. Brooklyn Weekly Eagle, with a meteorological register for Feb. 1844, from W. O. Bourne. Onondaga Standard, March, 1844, with an account of the best way of making salt. Sentinel of Freedom, Newark, N. J. Boston Mercantile Journal, with a notice of this Journal. Albany Evening Journal. The Republic, Zanesville, Ohio. New York Daily Tribune. Louisville Journal, April, 1844, from H. C. Banks. Tri-weekly Cincinnati Gazette, Jan. 1844. New Orleans Commercial Bulletin, Dec. 1843. Bristol County Democrat, March, 1844. The Retina, from E. A. Thompson. Saratoga Republican, from Mr. Root. Baltimore Patriot, March 7th, 1844.

NEWSPAPERS.—FOREIGN.

Columbo Observer, Jan. 4th, 1844, with a notice of this Journal. The Morning Star, published at Jaffna, March, 1843. The Morning Chronicle, March 7th, 1844. London.

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New Experiments on the Solar Spectrum.—We are obliged to omit, for want of room, a paper by Prof. Olmsted, describing a series of very beautiful experiments on the colors of the solar spectrum, first performed by Mr. Forrest Shepherd. The communication will appear in our next number.

Papers, "on the analysis of the Coprolites of Birds," by Dr. S. L. Dana, and "Observations upon the Valley of the Jordan and the Dead Sea," by Mr. J. D. Sherwood, will have an insertion in the January No.; and also communications from Prof. Strong, Prof. Bailey, Mr. J. D. Dana, and Prof. Dewey.

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To the Friends of Science, and more particularly to the Subscribers and Patrons of the American Journal of Science and Arts, from the Editors.

We beg leave to present to your consideration the following statement. The American Journal has been sustained during twenty five years; a generation of men has almost passed away, while death and misfortune have nearly cancelled the original subscription, which has however, from time to time, been recruited by many additional names; but the severe pecuniary vicissitudes of the last six years, (affecting also, as we regret to learn, our literary cotemporaries,) have again crippled our subscription list, which is now barely sufficient to pay the expenses of the publication of the Journal; and any considerable additional diminution might place its existence in hazard.

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