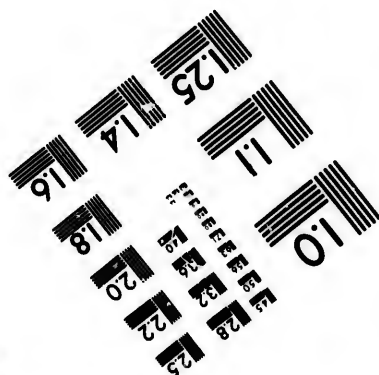
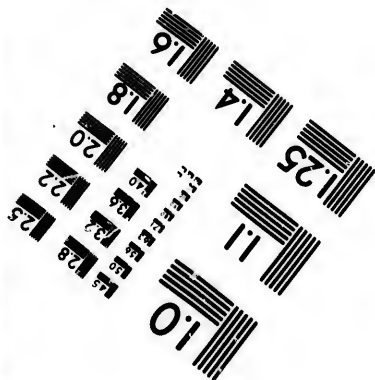
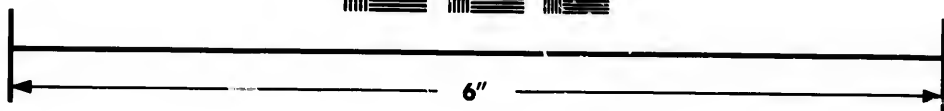
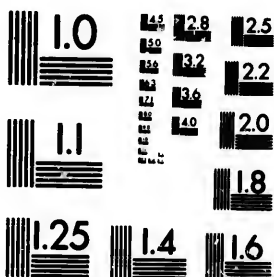


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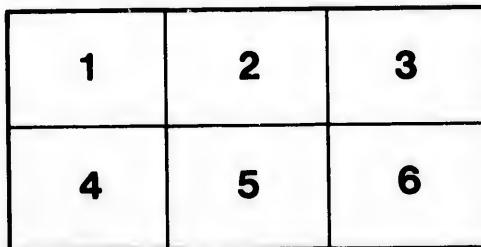
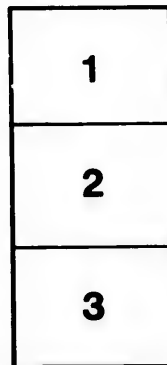
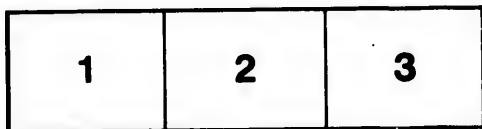
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JULES MARCOU.

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May, 1888.



AMERICAN  
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JULES MARCOU.

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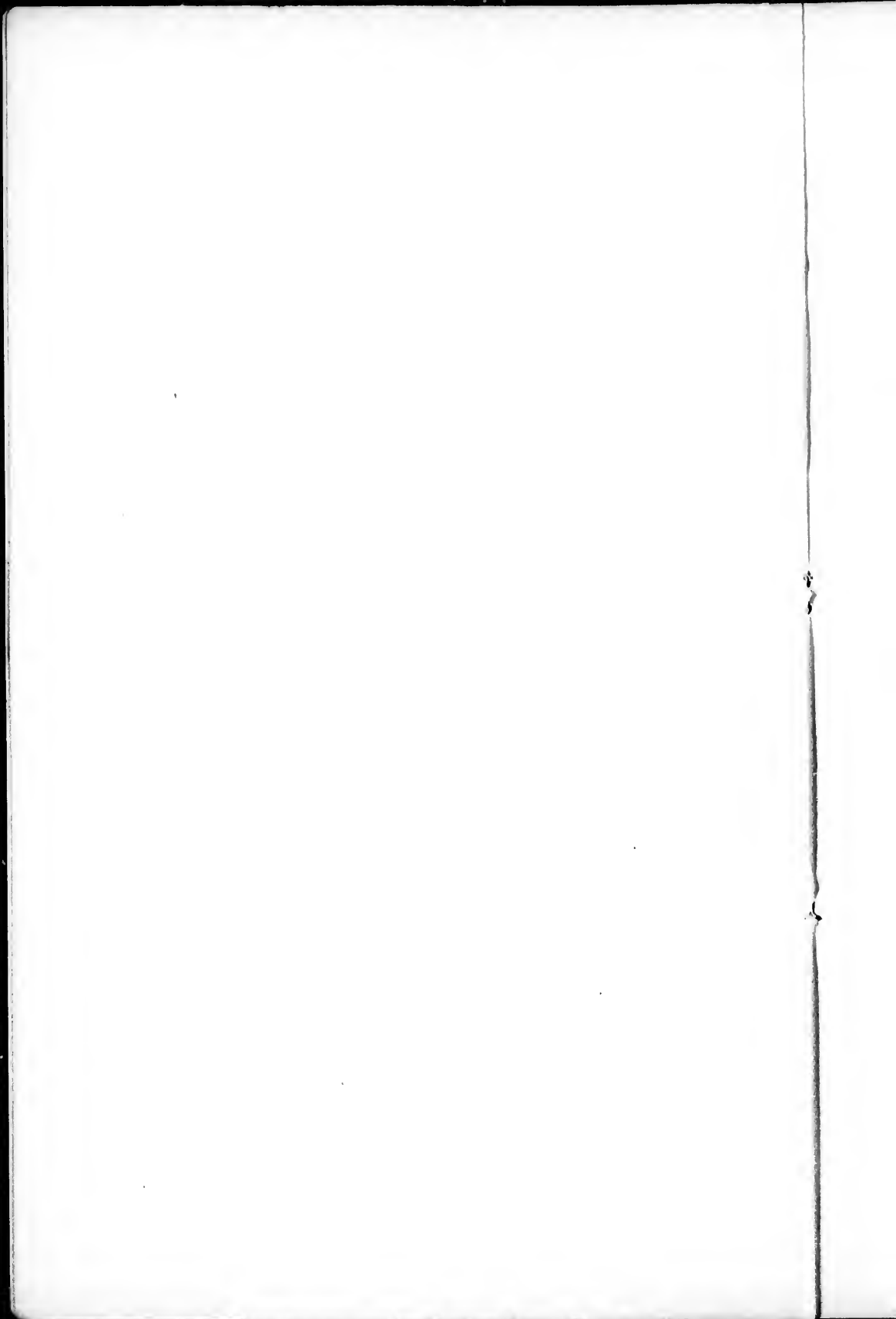
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## AMERICAN GEOLOGICAL CLASSIFICATION AND NOMENCLATURE.

### I. INTRODUCTION.

THE movement in favor of the uniformity of nomenclature, started by the International Geological Congress, although premature, calls for some remarks on the actual standing of American classifications. I shall confine myself to stratigraphy and the history of American nomenclature; the eruptive rocks being left apart.

Classification and nomenclature are necessities of the first order, and require of those attending to them knowledge and practical experience of rare and very difficult attainment. Mistakes are sure to result inevitably to all persons not well acquainted with all the different sides of the question, and errors are always attended with loss of time and loss of confidence; for, without an exact chronological order of all the strata, geology falls back into an inextricable labyrinth, a mass of incoherent and undigestible facts put together at haphazard. Nothing is so much wanted and so difficult to establish as a good classification, and the use of a cosmopolitan nomenclature acceptable, easily accessible and understood by all geologists.

In America the progress of nomenclature has been very steady although slow, being much embarrassed by interested persons, who have assumed to dictate authoritatively what they thought were the chronology and divisions of American stratigraphy; retarding for years, by all the means at their disposal, the acceptance of observations and classifications made by geologists better qualified and trained.

A summary of the discoveries and the opposition made to their acceptance is necessary.

### II. PRIMITIVE OR AZOIC SERIES.

The study of the crystalline rocks in Europe does not lead one to classify them into stratigraphical systems with geographical names, notwithstanding the attempt of Dr. Hicks for the British

islands (Lewisian, Dimetian, Arvonian and Pebidian); and in America attempts of this kind do not appear to have been attended by better results, although several efforts have been made to divide these rocks into ten or twelve systems only for Canada, Lake Superior and New England. The following names have been proposed and used to some extent, although no one has ever been able to see and give, with any degree of accuracy, the exact limits of each system, nor to be perfectly satisfied even as to their superpositions and successions: Laurentian, Huronian, Terranovan, Montalban, Norian or Labradorian, Taconian or Itacolunitic, Animiké (Animikie) series, Coatchiching, Ogishko, Vermillion series, Keewatin (Kewatin) and Keweenawan.<sup>1</sup> Not one of these systems, except the Keweenawan, contains fossils; notwithstanding the attempt made to record a lithological specimen as the remains of immense living sponges, called *Eozoon Canadense*.

Until now the geological survey of Canada, which seems to be the leader in classifying the primitive rocks into numerous systems, has failed to recognize and name with any degree of accuracy the different rocks. For instance, the chemist and mineralogist, Mr. T. Sterry Hunt, is responsible for such extraordinary confusion as to "all the *quartzites* of Montmorency Fall near Quebec, *gneiss*! The *erratic quartzites* and *quartz* of No. 1 on the road between Pointe Lévis and Notre Dame are named *limestone conglomerate*! and the *sandstone* lenticular mass marked 4" on the same "Plan of Pointe Lévis," published in 1862 by the Canadian survey, is recorded as a *magnesian limestone*!

With such lithological errors—which can be easily controlled by every geologist who visits Quebec—it is superfluous to discuss classifications. The lithology of Canada needs, not only a careful revision, but a complete recasting, before attempting anything in the way of nomenclature.

What is needed in America are minute lithological studies made by able observers conversant with Comparative Lithology, not only among American rocks, but also with European crystalline rocks; and also good and detailed surveys in the field.

The introduction of the name *Archaean* requires some explanation.

<sup>1</sup>The copper-bearing melaphyrs and conglomerates, constituting the main part of the Keweenawan, are not primitive rocks; and the whole system, as defined by Mr. R. D. Irving, is much younger. The melaphyrs contain *Orthoceras*, like the dinobae of Bohemia.

First used by Mr. J. D. Dana in 1863, *Manual of Geology*, p. 583, to designate the "Bronze or Archaic period" of the Prehistoric or "Age of man," it was not generally accepted. Mr. Dana, wanting to keep the name in geology, had the singular original idea of placing it at the bottom instead of the top of the column of classification,—a backward jump of the whole stratigraphic scale and index, changing only *Archaic period* into *Archæan system*.

*Archaios*, old, ancient, applies to the whole of geological science, and not to a particular epoch; and it can be used only in a general way. Otherwise, if employed for a group or system of rocks, it creates confusion in regard to fossils such as: *Archæopterix*, *Archæocidaris*, *Archæocyathus*, *Archæoniscus*, etc., which exist in strata and systems much younger than the so-called *Archæan system*.

For instance we have the Archaic period (Prehistoric), Archæan system (Pre-Taconic) on the one hand, and *Archæopterix*, *Archæocidaris*, *Archæocyathus*, *Archæoniscus*, etc., fossils, not one of which is to be found in either the Archaic or the Archæan. Archæan is one of those useless and cumbersome names which may well be dispensed with. The terms *Primitive*, *Crystalline* and *Azoic* series of rocks are far better and suffice amply for all demands.

### III. TACONIC SYSTEM.

The greatest error made during the last fifty years is the stubborn and inconceivable opposition to the existence of the Taconic system. Too many persons have been involved in the controversy and are, even now, interested in either suppressing it totally, or at least partially, not to expect all sorts of objections, oppositions and even trivial dissertations.

Billings in a paper, "Remarks on the Taconic controversy," *Canadian Naturalist*, April and July, 1872, has the courage to point out "the constant and utmost opposition of Messrs. James Hall and T. Sterry Hunt."<sup>1</sup> I shall add several other names: Messrs.

<sup>1</sup>At the meeting of the International Geological Congress at Berlin, in 1885, Mr. James Hall joined Messrs. A. Geikie and T. M. Hughes, in order to prevent the voting on the conclusions presented by Prof. G. Dewalque, Secretary of the Commission on the uniformity of nomenclature, which was entirely favorable to the Taconic system. The postponement until the meeting in London, in 1888, of the subject of divisions of the second order for the Lower Palæozoic series, on the ground that it "was mainly an English question," was a manœuvre on the part of those opposed to rendering justice to the just claim of American geology.

Those who succeeded in withdrawing Professor Dewalque's proposition have in view the interest of English geologists; hoping to have, at the London meeting, a ma-

W. E. Logan, James D. Dana, the two Professors Rogers, and C. H. Hitchcock. The part taken by these ~~seven~~ united adversaries of the Taconic system is inexcusable, and even odious. *eight.*

Incapacity in field stratigraphy and lack of practical knowledge of geology and palæontology on an unprecedented magnitude have never shown a bolder front.

From the beginning, the palæontologist of the state of New York, Mr. James Hall, has been at fault, ignoring the primordial fauna, its value, its true position in stratigraphy, even fifteen years after it had been stated in 1846 by Joachim Barrande; and rejecting the good observations and determinations of Dr. Emmons, when it was he, Hall, who was faulty and incorrect.

The ignorance displayed by all the opponents is startling, and can only be compared with their arrogance and their malicious acts. A few examples will suffice.

1. Disappearance of three thousand copies of the *Agricultural and Geological Map of the state of New York*, by Dr. Emmons, 1844, a large map, in four sheets, showing the extent of the Taconic system in New York, Massachusetts and Vermont.

2. The specimens, illustrating the Taconic system collected and arranged by Dr. Emmons in the State Museum of Natural History at Albany, all taken out, on an *ex parte* statement made by Mr. James Hall.

3. At the meeting of the American Association for the Advancement of Science, at Albany, in 1851, William B. Rogers said in the geological section: "that as for the Taconic system, it

is mainly composed of Englishmen, who will control the decisions and votes of the Congress, and accept the proposition arrived at—if any compromise can be made—between the partisans of Sedgwick and those of Murchison.

A preliminary meeting of the Commission of nomenclature was held lately—Aug. 30 to Sept. 6, 1887—at Manchester (England), in which a Canadian chemist, Mr. Sterry Hunt, representing "the united opposition of Dr. Emmons' contemporaries,"—just as Mr. J. Hall at the Berlin Congress—prevented once more the question of priority, and our just claim from being properly considered.

In the *Compte-rendu des séances à Manchester*, Bologne, 1887, we read at page 10: "M. G. Dewalque asks Mr. Sterry Hunt, if it is not right to consider the name *Taconic* which can be applied to one of the three systems in discussion, and which presents the advantage of giving a place to American Geology." Mr. Sterry Hunt answers "that the Lower Taconic is Archean, and the Upper Taconic is Cambrian. Historically, the last name has superiority over the first. Besides, he does not believe that the American geologists claim its maintenance."

With two such representatives before the International Commission of Nomenclature, the American interest has great danger of being jeopardized, and not properly acknowledged or defended. (See also: *On the use of the name Taconic* by Jules Marcou in *Proceed. Boston Soc. Nat. Hist.*, vol. XXIII, pp. 347, 348, March 2, 1887).

is dead! dead!! dead!!! with a significant pointing of his finger to Dr. Emmons."<sup>1</sup>

4. Mr. James D. Dana refused to publish Dr. Emmons' "remarks upon Logan's Report when he announced his Huronian system, though they were courteous in the extreme."<sup>2</sup> Emmons claimed that the Huronian was only a part of his Taconic system.

5. It was with the greatest difficulty that I was able to publish the letters of Barrande, being asked repeatedly by friends of Logan and Hall not to put them in print.

But even more. A very short résumé of my communication to the Boston Society of Natural History, October 17, 1860, made by the Secretary, was considered by the "Publishing Committee" as sufficient, although the letters are only mentioned without any extracts whatever; and it required the powerful intervention of L. Agassiz, in order to have my paper published in full (*Proceed. Boston Soc. Nat. Hist.*, vol. VII, p. 357 and p. 363, Boston, 1861).

As soon as published, the Barrande letters were taken possession of by the editors of the *American Journal of Science*, of the *Canadian Naturalist* and of the *Report on the Geology of Vermont*, under an altered and false title; and, because I signalized that unscrupulous and mean act, I stand as the only fellow of the Boston Society of Natural History who has ever been publicly censured.

It is at the end of vol. VIII of the *Proceedings* where, in the *Errata*, p. 310, the "Publishing Committee" point out my footnote on page 240, in severe and unjust terms, in order to please Mr. J. D. Dana, who, after altering and falsifying the title of my paper, was bold enough to ask an investigation against me. A "Special Committee" was appointed, and its report exculpated me from all blame and refused the censure called for by Dana, and nevertheless, passing over the report of the "Special Committee," I have been censured by the "Publishing Committee," for having maintained the integrity and exactness of the title used in the *Proceedings*, and for defending my property. After that most unjust blame against a fellow member, I did not withdraw from the society, but it prevented me for twenty-five years from continuing my reading of geological papers before it; and I have been obliged, in order to publish my observations on American geology, to do it either at

<sup>1</sup>In a letter of Dr. Emmons to J. Marcou, dated 20 Dec., 1860, partly published in *The Taconic system and its position*, etc., p. 188, Cambridge, 1885.

<sup>2</sup>The same letter and paper.



my own expense in Cambridge, or to have recourse to foreign periodicals in France and in Germany.

It is always disagreeable and even detestable to be obliged to speak of one's self; but it is unfortunately for me, the only way to maintain my discoveries and observations, which are constantly attacked or passed over silently by almost all those studying the same subjects. In other sciences, such as chemistry, physics, astronomy, zoology, physiology, botany, etc., it is always easy to control new facts and discoveries, by repeating carefully the observations in laboratories. It is not so in practical geology; for the laboratories are the whole surface of the earth, and you must go on each spot to see for yourself. But, besides, many geologists when in the field do not know how to observe, or are able only to observe a small portion of the phenomena spread before their eyes, neglecting most important points, and drawing false conclusions. A very easy and frequent way to impose and ventilate geological errors, is to say, "it appeared at the time to be the generally accepted opinion" (J. D. Dana's *American Journ. Sci.*, third series, vol. xxxiii, p. 416, 1887), when too often those who say so have done all in their power to impose and maintain the errors they are lamenting, trying as best they could to extricate themselves from their false position.

It is much more easy to prevent the truth being accepted in geology, by the very nature of practical observations in the field, than in any other science, and the facility of maintaining errors is constantly made use of by all interested parties.

But, even in geology, errors must come to an end. Forty or fifty years, if great for the life of an observer, are little in the history of progress. Truth is sure to have the upper hand; and there is nothing else to do for the original discoverers and honest and exact observers, but to maintain sternly and without flagging their views, opinions and discoveries, against all opposition, obstruction, denial, or studied silence.

This is my excuse for speaking of myself so often in this memoir, having no choice, and being unwilling to lose observations made during forty years of my life, and often under extremely difficult, even perilous, circumstances. I am the first geologist who has made a geological section from the Mississippi river to the Pacific shores, determining and naming carefully all the different systems of rocks existing in half the continent of North

America (1853-1854); and I have defended the Taconic system, single-handed almost, during twenty-seven years (1860-1887); two practical geological facts, which it will be most unjust to blot out of my record as an American geologist.

6. The geological survey of Canada not only did not help me in any way, when I went there in 1861, '62 and '63, to investigate the Taconic rocks, on the pressing invitation of one member of the survey; but, on the contrary, an assistant of the survey was sent after me, to see and report my doings in the vicinity of Quebec, to the director Logan.

7. Mr. James D. Dana has given a "List of papers on the Taconic system" (*Amer. Journ. Sci.*, vol. xix, Feb., 1880, p. 153) partial and most incomplete, from which he has excluded almost all my papers on the subject, as well as all those of Barrande and Perry. Mr. Dana pretends to make a classification of those adverse to and of those in favor of the Pre-Silurian (Pre-Potsdam) age of the Taconic system; but he has associated with Dr. Emmons and Mr. Marcou, in the second division, three of the most constant and bitter opponents of the Taconic. And, finally, in accordance with his usual practice of giving credit to those to whom it does not belong, Mr. Dana pretends that the Lower Silurian is called Champlain division by Mather, when it is an unquestionable fact that Dr. Emmons is the originator of the Champlain group.

#### ORDER OF DISCOVERIES AND ORIGINAL RESEARCHES ON THE TACONIC SYSTEM.

1809. Maclure in his first geological map of the United States colors as "Transition rocks" the eastern side of the Hudson river from Hudson city towards Poughkeepsie. By transition rocks, he means limestone, greywacke, flinty slates and trap.

1817-18. Maclure in his second geological map colors as "Transition rocks" all the eastern band, extending from the Canada line down the eastern shore of Lake Champlain, through Vermont, eastern New York, western Massachusetts to Tappan sea in the Hudson river; a quite accurate geographical distribution of the Taconic system, and very similar to the band colored as Taconic in the *Agricultural and Geological map of New York*, by Dr. Emmons, 1844.

1819. C. Devey gives a sketch of the mineralogy of the Taconic range, near Williams College.

1824. C. Devey with the assistance of his pupil Dr. E. Emmons publishes *A Geological map of the county of Berkshire, Mass., and of a small part of the adjoining states*; with a sketch of the geology and mineralogy,—a first attempt to systematize and classify the rocks of the Taconic region.

1818-24-28-32. A. Eaton, in four different publications, gives sections, classifications and arrangements of the rocks of the vicinity of Williams College and the Taconic range, which are rather confused and certainly without any progress, on what was already known. Like Maclure and Devey, he refers a part of the rocks of the Taconic area to the Primitive and Transition rocks; and the discovery by Vanuxem in 1829, that all the New York strata belong to the Transition series, was not accepted by Eaton, except for the *Calcareous sandrock*, and the Trenton called "shell limerock at Trenton Falls" of the *Calcareous (sic) formation*; all the rest from the *Utica slates* included were referred by him to the secondary rocks. Whatever may have been Eaton's success as a teacher and a collector of specimens, he was certainly not a classifier, and his nomenclatures of 1818-24-28-32 are all of doubtful value and of little consequence in comparison with Devey's geological map of Berkshire of 1824.

1829. L. Vanuxem discovered, during 1827 and 1828, that all the secondary rocks of Maclure, Cleaveland and Eaton, in the states of New York, Ohio, Kentucky, Tennessee and Virginia, were Transition rocks, and of a "greater geological antiquity" than admitted until then (*Amer. Journ. Sci.*, vol. xvi, 254). It was the first great step, towards rational classification and a good nomenclature of the American palæozoic rocks.

1838-42. Dr. E. Emmons discovers, below the Potsdam sandstone, the great *Taconic system*.

1840-43. W. W. Mather originates the *Hudson river group* in which he places all the slates west and east of the valley of the Hudson, synchronizing it with the Frankfort group and Pulaski shales. He declares that, in the valley of the Hudson, the strata are "confusion worse confounded;" and that the breaks or fractures "have deranged all the rocks of the Champlain division and packed them together, helter-skelter, in the utmost confusion." He describes the Taconic system of Dr. Emmons, as confined to the rocks forming the Williamstown mountain range, and he thinks that they "blend into the Champlain division on the one hand, and into the primary rocks on the other." As a conclusion, Mather

considers "the Taconic rocks are the same in ages as those of the Champlain division, modified in character by metamorphic agency."

1844. Dr. Emmons publishes his *Taconic system*, with characteristic and special fossils. It contains the *first discovery and description of the Primordial fauna* all the world over, a discovery proved and advocated by Barrande, and which the adversaries of Emmons are trying—even now (March 1888 in *Amer. Journ. Sci.*)—to blot out from the record of American geology, and to replace by that part of the Cambrian system in which Sedgwick did not find a fauna, and about whose true sequence and exact stratigraphical divisions he knows almost nothing.

1846. Dr. Emmons reproduces his Taconic system in *Agriculture of New York*, with an appendix and a geological map showing for the first time, the band of Taconic rocks from Canada to New Jersey. The map—three thousand copies—was suppressed by interested parties, and was not issued until about 1877, when some mutilated copies were distributed by the state librarian at Albany. Finally, in 1887, the map, as dressed and colored by Emmons with the full title, has at last come out.

1847. Mr. James Hall, in *Paleontology of New York*, vol. 1, p. 319, does not recognize the Primordial fauna and ignores the position of the rocks of the Taconic system, which he thinks are clearly Hudson river group acted upon by gradual metamorphism.

He says that the fossils described by Dr. Emmons are "unequivocally" identical with well-known species in the Hudson river group (upper part of the second fauna); regarding the *Atops trilineatus* as "unquestionably" the *Calymene Beckii* of the Utica slates, one of the grossest errors ever made by a palæontologist; and that the *Elliptocephalus asaphoides* belongs to a Lower Silurian (second fauna) type, closely related to the genus *Oxigia* or *Asaphus*, another great palæontological mistake.

1855-59-60. Dr. Emmons continues to describe the Taconic system, adding new fossils to the primordial fauna of America, and synchronizing it, in 1860, with the Bohemian primordial zone of Joachim Barrande.

1859. In an article entitled: "Trilobites of the shales of the Hudson river group," Mr James Hall describes three trilobites of Georgia, as *Hudson river group* fossils, ignoring their primordial characteristics, and the meaning of palæontological laws, as revealed in the primordial zone of Bohemia and Scandinavia.

It is in this small paper, printed under four different titles, in less than three years, that Mr. Hall, in order to strengthen his opinion that the Georgia trilobitic beds should be placed in the upper part of the Champlain system, far above the Trenton groups, and also to cover his error as to the true position of the primordial fauna, wrote his celebrated authoritative phrase, now legendary among American geologists: "It would be quite superfluous for me to add one word in support of the opinion of the most able stratigraphical geologist (William E. Logan) of the American continent."

1860. Mr. Jules Marcou uses the name Taconic, in a communication before the Boston Society of Natural History, reading three letters of J. Barrande addressed to him, on the primordial characters of the Braintree, Georgia and Pointe-Lévis trilobites, and stating that the Taconic system, misunderstood until then by the adversaries of Emmons, must take its place and its right usurped by the Hudson river group and a sort of metamorphic Champlain division, extending even so far up, according to Messrs. Hall and Hitchcock, as to include the Upper Silurian, the Devonian and even the Carboniferous, a certainly very rich and grand solid series of metamorphosed strata. It was the first ray of light in favor of Dr. Emmons and his Taconic system. Unhappily, it was also the last; for that persecuted and ablest of all the American geologists was shortly after shut up in North Carolina by the civil war of 1861, where he died in 1863 without knowing the other efforts made by Barrande, Billings and Marcon.

My communication of the letters of Barrande, with my remarks on the Taconic rocks of the vicinity of Quebec, before the Boston Society of Natural History, Oct. 18, 1860, was like a thunderbolt in a clear sky. Its first result was the immediate stopping of the printing of three large works: first, the third volume of the "Palæontology of New York" by James Hall, the introduction of which was already heralded in the *Amer. Journ. Sci.*, Jan. 1861, p. 125, as handling, "with masterly skill the difficult subjects connected with the proper classification of the lower horizons of life in our planet;" second, the "Geology of Vermont;" and third, the "Geology of Canada;" besides the issue of the geological map of Vermont and the geological map of Canada; the publication of those five geological and palæontological works was at once interrupted, as soon as my paper had been issued at the end of Decem-

ber, 1860, and many changes, alterations and additions were made. The introduction of the third volume of the "Palæontology of New York" was entirely recast, with "the proper classification of the lower horizons of life on our planet" prudently left entirely out. However, all the changes and alterations in those five publications, all made in order to explain or cover, as far as practicable, the precedent mistakes, are all erroneous and it is merely a change from Charybdis to Scylla.

1861. M. Joachim Barrande, in a very remarkable memoir, published in Paris, recognizes, in the Taconic system, his own Primordial fauna and zone, and declares openly and positively that Dr. Emmons has the *priority* in the discovery of the Primordial fauna. He does not hesitate to regard the determination of the Taconic fossils by Mr. J. Hall as erroneous, and his conclusion in regard to stratigraphy as a great mistake.

1861-85. Mr. Jules Marcou, during a quarter of a century in ten memoirs published in America and in Europe, maintains the "Taconic system," adding to it the Potsdam sandstone as its uppermost division. He advocates the accuracy of the Taconic system not only in the main but in most of the details, as it was propounded by its founder, Dr. Emmons.

In his memoir of Dec. 10, 1884, "The *Taconic system* and its position in stratigraphic geology" (*Proceed. Amer. Acad. Arts and Sciences*, vol. XII, p. 174, Cambridge, 1885), Mr. Marcou gives, on p. 221, a vertical and general section of the Taconic system, with a tabular view, p. 224, showing, for the first time, the division of the Taconic series into three systems, each one characterized by a special fauna; first, the *Infra-Primordial fauna*, containing all the fossiliferous strata below the horizon of the *Paradoxides* or Lower Taconic; second, the true *Primordial fauna* as characterized by Barrande with its zone of *Paradoxides* and *Olenellus*, or Middle Taconic; and third, the Upper Taconic or *Supra-Primordial fauna* or zone of the *Dikelocephalus*, containing primordial types united with types whose great development takes place in the second fauna or Champlain system.

1863-82. In 1862, Mr. C. Fred Hartt, of New Brunswick, came as a student at the Museum of Comparative Zoology, and was placed by Agassiz under my direction. During 1863, Hartt showed me fossils collected round St. John, by Messrs. C. R. and G. F. Matthew. I referred them at once to the Primordial fauna, telling Hartt that they were analogous to the fossils of the Georgia

slates; and I put into his hands specimens of the Primordial fauna collected by me, in 1861 and 1862 at Georgia and Swanton, Vermont, and at Pointe Lévis, Canada; showing him also the identity of the forms of the American, Bohemian and Scandinavian primordial fossils.

Hartt published a "Preliminary notice of a fauna of the Primordial Period in the vicinity of St. John, N. B." in "*Observations on the Geology of Southern New Brunswick*, by L. W. Bailey, pp. 30 and 31, Fredericton, 1865, in which he says distinctly that the fauna of the vicinity of St. John is of the Primordial period, acknowledging the kindness of Professor Agassiz, and his suggestions and help. My name is not quoted, because it was the custom, by courtesy, to refer everything which passed in the Museum to its founder and director, Louis Agassiz.

Although well acquainted with the controversy I was then carrying on in favor of the Taconic system, Hartt and Bailey used only the names Silurian and Quebec group, according to the view of the Director of the Geological Survey of Canada, W. E. Logan.

Hartt did not publish the primordial fossils collected by Messrs. Matthew, Bailey and himself until 1868, when they appeared as a part of *Acadian Geology*, second edition, by J. W. Dawson, at pp. 643 to 657, London. They are referred to the Lower Silurian of Murchison, without any notice whatever of the Taconic system. But more, Mr. Dawson says: "These beds (meaning the series at St. John) are in the highest degree important in a geological point of view, as their fossils establish for the *first time on the American continent* a series of fossiliferous beds older than the Potsdam sandstone, hitherto supposed by American geologists to be our oldest Palæozoic group, etc." (*Acadian Geology*, p. 638). And further on he adds: "This formation has as yet (*sic*) been known as the *St. John group*; but I think this name unsuitable, . . . and would therefore propose . . . the name *Acadian Group*, by which I hope it will be known to geologists in whatever part of America it may be recognised."

In the third edition of his work, London, 1878, Mr. Dawson repeats the same inaccurate statement, changing only the name Lower Silurian into "Middle or Lower Cambrian, known in England as the Menevian;" and using the expression "Acadian series" of St. John, instead of "Acadian group."

It is impossible to excuse such omissions and statements on the part of Mr. Dawson, who, in 1868, and even in 1878, pretended to

ignore and pass over all the observations and publications of Dr. Emmons on fossiliferous beds older than the Potsdam sandstone, all the remarks and conclusions of Barrande, and all my researches on the Taconic and the Primordial fauna in America.

To Messrs. C. R. and G. F. Matthew is due the discovery of the Taconic fauna at Portland in the city of St. John (New Brunswick) in 1862, and to Mr. G. F. Matthew are due the excellent and numerous descriptions of all the fossils and sections, showing that at or near St. John the Taconic system is well developed and complete with its three faunas, the Primordial, the Infra-Primordial, and the Supra-Primordial.

The name "St. John group," applied to the division of the third order which contains only the Primordial fauna, was first given by Hartt in 1865; and ever since Mr. G. F. Matthew has always used it, with the same meaning (see *Illustrations of the fauna of the St. John group*, 1882, etc.). It had priority over "Acadian group" proposed only three years later, in 1868, by Mr. Dawson, who simply transferred the historical names of "Acadian" and "Acadia" into geology in imitation of Murchison's "Silurian" and "Siluria," without even giving any observations of his own on the stratigraphy or palæontology of the vicinity of St. John.

Mr. Matthew has not used yet the name Taconic, calling Cambrian all the strata round St. John. By Cambrian, he does not mean the true Cambrian of Sedgwick, containing a fauna, but only that part in which Sedgwick did not find a single fossil.

1880. Mr. S. W. Ford acknowledges Dr. Emmons' "great service," in opposing the Hudson river group doctrine, and admits his "signally good work;" but says, that his favorite system (the Taconic) is a failure—certainly a very strange and rather paradoxical way of recognizing his good work and great service.

1886-87. Mr. C. D. Walcott says: "Dr. Emmons deserves great credit for the work that he did." But misled by the erroneous notion constantly and perversely put forward and maintained by Mr. Dana, that the Taconic area, as originated by Emmons, is of Lower Silurian age, he regrets not to apply the name Taconic to the formation of the Georgia horizon, using instead the name Middle Cambrian. In doing this Mr. Walcott does not mean to synchronize the Georgia formation with the Middle Cambrian (Tremadoc slates and *Lingula* flags of Wales—Sedgwick), creating a new confusion almost unconsciously; but only meaning a Middle



Cambrian according to his view, which limits the Cambrian to only one system, the Primordial fauna of Burrande, an interpretation entirely different and opposite to Sedgwick's classification. Soon after, however, Mr. Walcott has the advantage to recognize that the original Taconic area is formed of rocks of the Primordial zone, as well as the upper Taconic of Washington county; and in two papers read in January and April 1887, at Washington, before scientific societies, he accepts the "Taconic system" in the main, and uses the name as the title of his two memoirs.<sup>1</sup>

1888. (March 31).—As predicted in my foot-note the adversaries of the Taconic system have just begun a new attack. Led or more properly misled by Mr. James D. Dana, Mr. Walcott is tracing back his steps, and appears now for the second and even third time as a strong opponent of Dr. Emmons, and against the just claim of American geology for the priority of the discovery of the Primordial fauna and the strata containing it, in the general classification and nomenclature of the world. I have expected it, and was sure that the erroneous opinions expressed during the last forty-five years, were to be defended and clung to with the greatest obstinacy; and that after obtaining with more than ordinary difficulty the acknowledgment of the existence and true geological position of the Primordial fauna, every effort would be made to reduce that ungracefully granted concession to its smallest compass in the typical Taconic area.

Mr. Walcott says: "Professor Marcou . . . has written at length upon the Taconic system, but I have been unable to discover that he has made any field observations in the typical Taconic area" ("The Taconic system of Emmons and the use of the name Taconic in geologic nomenclature," *Amer. Journ. Sci.*, vol. xxxv, p. 229, March, 1888). I have never claimed, that I made original researches in the Taconic range, nor intimated in any way that I was ever there. In January, 1861, Dr. Emmons wrote me, that in the spring or summer, he, Colonel Jewett, and myself, would go

<sup>1</sup>The adversaries of Dr. Emmons' Taconic system now admit two-thirds of it; but there still remain unaccepted the "Black slates," the Stockbridge marble and the Sparry limestone, besides the limestone and slates of Pointe Lévis, Phillipsburgh, Fort Cassin, Shoreham, and Wappinger Valley. It is to be expected that the strongest opposition will be made to placing the Upper Taconic, composed of the Lévis and Phillipsburgh group, and the Swanton and Citadelle Hill of Quebec group in the Primordial fauna as the supra-primordial or zone of the *Dikelocephalus* and *Bathyurus*, and that Messrs. Hall, Dana and their followers will contest that part of the Taconic with the tenacity of despair.

together over his original ground and study the Taconic area. But Emmons never returned; and I was begged repeatedly and most earnestly by Barrande, Billings and Jewett to go at once to Georgia and Pointe Lévis, as the two most important localities, on account of the discoveries of fossils belonging to the Primordial fauna. Yielding to this most urgent call I did go there in 1861, '62, '63, '73 and '74; and it took all the time I was able to spare to work out the geology of northwestern Vermont and the vicinity of Quebec.

I trusted to Dr. Emmons' observations and still continue to do so; while, on the contrary, I distrust all the numerous and continually changing opinions of his adversaries, for to my knowledge Dr. Emmons is the only observer with good stratigraphical, palæontological and lithological principles who has ever studied that region. For any impartial person, it is obvious that the errors of Mather, Hall, Hitchcock, Dana, Logan, Hunt, etc., who have persistently denied the existence of the Taconic system and the Primordial fauna, cannot be placed in opposition to the good and correct observations and conclusions of Dr. Emmons.

In order to impress favorably his readers, Mr. Walcott refers to his "principles," and says: "I have studied in the field most of the sections mentioned in this article, and know from which horizons the collections were obtained, and therefore with considerable confidence express conclusions that differ from those reached by geologists and palæontologists, who have arrived at their results through the accounts of the observations and collections of others or from stratigraphic or palæontologic data considered without giving due weight to the importance of combining them." And "hammer in hand I examined it (Prospect Peak, Nevada), and collected fossils at all places where they could be found" (see *Second Contribution to the studies on the Cambrian faunas, etc.*, pp. 12 and 33, Washington, 1886). He insists on "priority of definition" and "accuracy of original observations" and also says: "In the evolution of stratigraphic and historic geology, stratigraphic geology preceded palæontologic stratigraphy;" "different sections of strata in the same province may be compared with one another when the continuity is broken;" "that the unit of geologic nomenclature is the formation as lithologically determined;" "that the means of correlation of the formations of one province with those of another is by order of succession, as stratigraphically de-

terminated, of the contained organic remains of the respective formations." And finally, he concludes, "with the preceding statements in mind, I take up the question of the Taconic system in geology, as one that can only be intelligently understood and decided by the application of the principles contained in them" ("The Taconic system of Emmons, etc.," in *Amer. Journ. Sci.*, vol. xxxv, pp. 229, 230, March, 1888).

From these quotations it appears that Mr. Walcott supposes Emmons, Barrande and Marcon were lacking in practical knowledge of stratigraphy, palaeontology, lithology and of "How to observe," in the field. Evidently the *doctrine des colonies* is not considered with favor by Mr. Walcott, and the dozen of Trenton-Chazy-Calciferous species found by the late Rev. Wing, in the belt of limestone and marble "that outcrops both on the eastern and western side of the Taconic range," is a "hitch," which he cannot accept on account of his "principles." Besides he does not approve the "Precursory centre of creation," showing that *forerunners* and *prophetic types* of the second fauna have made their appearance in America sooner than in Europe; and that we have here a second example of the *doctrine des colonies*, only instead of being a part of the third fauna inclosed in the strata belonging to the second fauna as in Bohemia, we have a part of the second fauna inclosed in the strata of the Primordial fauna and consequently in the Taconic system.

It is certainly a sad spectacle to see how every opportunity to diminish the merit and the good original works of Dr. Emmons, is eagerly seized upon. But I will only repeat what I have said before: "the truth is always victorious, in spite of opposition and obscurity, and therefore the future of the Taconic system is fully assured" ("The Taconic system and its position in stratigraphic geology:" *Proceed. Amer. Acad. Arts and Sci.*, vol. xii, p. 175, Cambridge, 1885).

1887-88.—Mr. Marcon demonstrates in two papers read before the Boston Society of Natural History in March and May, the priority of the name Taconic over Cambrian, and continues to sustain the whole Taconic system, the "Black slates" of Emmons included. He also demonstrates the *priority* of Champlain over the Ordovician system offered lately by Professor Lapworth to designate the rocks containing the second fauna of Barrande.

1887. The last issue of Mr. J. D. Dana's Taconic paper, "The

views of Professor Emmons on the Taconic system" (*Amer. Journ. Sci.*, 3d series, vol. xxxiii, May, 1887, pp. 412-419), is a curiosity in more than one sense, being a rather instructive illustration of the inside of the question, from its adversaries.

After Barrande and Marcou's paper of 1860, Messrs. Hall and Logan each tried to throw the blame on the other, one saying that it was "the most able stratigraphical geologist of the American continent!" who deceived him; the other, that it was the greatest American palæontologist "whose opinion is law in American geology!" that misled him. However, they soon rallied and, reassured by Dr. Emmons' death in 1863, and Mr. Marcou's temporary absence in Europe, 1864-71, they put their heads together once more, and in 1865, using as usual Mr. T. Sterry Hunt as their amanuensis or secretary, after new investigations and explorations made together of the Taconic area in New York, Massachusetts and Vermont, they published in the *Amer. Journ. Sci.*, vol. xxxix, p. 96, 1865, a restatement of their old opinions against the Taconic system.

We have now another rub between Messrs. Dana and Sterry Hunt reproaching each other for having persecuted Emmons and opposed the Taconic system. "Dr. Hunt's opinions were not always couched in courteous language," says Mr. Dana, which is too true; but it was simply an imitation of the language constantly used in all his controversies by Mr. Dana himself.

After marching hand in hand during more than forty years, using freely all their periodicals (*Amer. Journ. Sci.*, and *Canadian Nat.*), and their annual publications (*Amer. Association Adv. of Science* and *Geol. Reports of Canada*), to persecute and wage persistent war against both Emmons and Marcou, these two old associates, most intimate friends and comrades, have now come to exchange disagreeable remarks and violent attacks, a result long expected by those who know the two worthies.

Mr. Dana tries to explain why he refused to publish Emmons' remarks upon Logan's Report, when he announced his Huronian system, because Dr. Emmons claimed that the Huronian was only a part of the Taconic. "The refusal," he says, "was on the ground that the 'remarks' contained no facts sustaining the opinion, and that opinions on such a point without facts were of no value to the science. The Huronian region and the Taconic were remote from one another, and Logan's discoveries of fossils in

Canada seemed to be too decisive to be so set aside" (*Amer. Journ. Sci.*, vol. xxxiii, p. 418, 1887).

It is difficult to imagine more lamentably lame excuses. Mr. Dana has filled up his journal, since he is the geological editor, with papers of controversial nature without a single fact or observation made in the field or in museums, and such papers entirely valueless to science may be counted by the dozen and even by the hundred. In fact anything opposing Dr. Emmons or Mr. Marcou has always been accepted eagerly by him, and often published with commentaries of his own, not "always couched in courteous language."

As to "Logan's discoveries of fossils" in the Huronian, it is one of Mr. Dana's customary bold assertions, made against the printed opinion of the originator; for Logan takes special care in all his papers on the subject, from 1854 to 1863, to insist constantly that he did not find fossils. But this is not all; never has the character of a man shown itself so plainly and under such an unenviable light, as has Mr. Dana's in this article. He declares that he began to work on the Taconic question in 1843! "learning but publishing nothing." His "investigations in Berkshire were commenced in July of 1871, in order to get at the truth, without any feeling of opposition to Professor Emmons."

Forty-four years of investigations, observations, conclusions, controversies, and not a single fact worth recording, or which can be quoted as good in stratigraphy, or in palæontology, are certainly anything but creditable. Such negative results speak for themselves. For a man who, as a writer of *manuals*, and as an extraordinary, severe, unjust and very partial critic against all those who have worked on American geology, without his permission and special approbation, to be reduced to admit that "Prof. Emmons was right in his Berkshire stratigraphical observations," and not because he found it out himself, but simply because a geologist of the United States geological survey, in a single visit in Berkshire during 1886, has confirmed Dr. Emmons' observations, is a fact which does not require commentaries. It classifies Mr. Dana as a practical geologist and an original observer in his right place; showing the value of his persistent and blind resistance against progress, his opposition à *outrance*, and his *parti pris* to ignore a system of 25,000 feet of thickness, more important than the Cambrian (Champlain), Silurian and Devonian put together.

Messrs. Dana and Hall have not even excuses of distances to travel over, or want of facilities and opportunities to correct their colossal error; for both have passed their lives in full view and at the very door of the Taconic region, and both have maintained their systematic opposition in face of all the facts presented by Dr. Emmons, Barrande and Marcou.

It is comforting to see that Mr. Dana has never had any "feeling of opposition to Prof. Emmons," for no one would ever suppose that he was friendly or even strictly just to Emmons and the Taconic system. His efforts during forty-four years have been directed to "keeping life in wrong conclusions," and in the opposite direction of "truth." And at this late hour, to try to exculpate himself, Mather, Professors Rogers, Hitchcock, Logan and Mr. James Hall, throwing the blame upon an irresponsible chemist, Mr. T. Sterry Hunt, who has acted during twenty-four and even more years as amanuensis of Logan, Hall and Mr. Dana himself, is not generous.

In the Taconic controversy, "discourteous words" and "discourteous acts" have been constantly and systematically used by the adversaries of Emmons and Marcou; and the criticisms made, not only did not "give life and progress to science" as claimed by Mr. Dana, but were of such a nature, that both Emmons and Marcou were so pushed aside and almost silenced, that although neither has ever despaired, or has ever yielded one single *iota* of his observations, each has been obliged to stay outside of official and general relations with all American associations, committees, and special contrivances of the persecutors and recognized enemies of "truth." And now all the efforts of the united opponents against the progress of American geology, during more than forty years, to blot out their unscrupulous and unintelligent acts, will not succeed. Their records will stand, and be a perpetual subject of regret and a black spot in the history of American geology.

#### IV. CAMBRIAN OR CHAMPLAIN SYSTEM.

The *priority* of Professor Sedgwick's researches and classifications of the rocks containing the second fauna between 1830 and 1835, is unquestionable, and the name of Cambrian is excellent, notwithstanding its original meaning (*Cimbri*, robbers, and *Cambria*, country of the robbers), on condition of limiting its meaning to only one system and one fauna.

Dr. Emmons, in his early studies of the New York stratigraphy, recognized a great division above his Taconic system, to which he gave, in 1842, the name *Champlain group*, a good name for North America. But *Cambrian* precedes it, and ought to be used instead, even in America.

As to the names *Cumbrian* from Cumberland, proposed in 1847, by Elie de Beaumont; *Cambro-Silurian* of Sedgwick<sup>1</sup> 1853, and J. B. Jukes, 1857, and *Ordovician* of Professor C. Lapworth, 1879; they are all posterior, not only to Cambrian, but also to Champlain, and consequently they have to be dropped.

The confusion created in the first volume of the *Palæontology of New York*, by the mixing up of Taconic or Primordial fossils with the second fauna requires a complete recasting and a careful revision of the work.

#### V. SILURIAN SYSTEM.

In America, this system was first worked out by Vanuxem and Conrad, in the Geological Survey of New York, from 1836 to 1839. Mr. James Hall, in the *Second Annual Report of the fourth Geological District*, comprising all the western part of the State of New York, "considers the rocks of the fourth district as belonging to the Old Red sandstone and the Carboniferous groups and to be *above the Silurian system of Murchison*. The evidence for this conclusion rests, in part, upon the organic remains, and if we can rely on these characters, there appears little question regarding the age and position of our rocks" (*Second Ann. Report*, p. 291, Albany, 1838). He makes the Old Red sandstone end with the "Gypseous marl and slates" or Onondaga saliferous group; and he places the *Onondaga limestone* and all the strata above as *Carboniferous or Mountain limestone!*

We see that, from the beginning of his studies as palæontologist and geologist, Mr. James Hall made a singular and most unhappy use of the organic remains which, from that time until now, he has constantly misunderstood in their identification, comparison and value as a guide in geology. As a consequence he has almost always arrived at erroneous classification and nomenclature. Being only a second-rate palæontologist, wanting in zoölogical and geo-

<sup>1</sup>"I offered a compromise: viz., that of calling the Bala group, Cambro-Silurian. This compromise was rejected by Murchison and was afterwards withdrawn by myself 1854." [See Sedgwick's Preface of *A Catalogue of the collection of Cambrian and Silurian fossils*, by Salter, p. XXVIII in a foot-note. 4<sup>o</sup>, Cambridge, 1873.]

logical knowledge, his judgment is not authoritative, and cannot be compared with the opinions and views of Barrande, Agassiz, d'Orbigny, Deshayes, Ed. Forbes, Davidson, de Koninek, McCoy, Salter, Angelin, Linnarson, etc., etc. "Regarding the age and position of our rocks," Mr. James Hall has gone from the first hopelessly astray in dealing with all the American geological questions.

To Edouard de Verneuil is due the credit of having given the true parallelism or homotaxis of the palæozoic rocks of North America with those of Europe, so far as the second fauna (Cambrian), the third fauna (Silurian), the Devonian and Carboniferous are concerned; for he did not know in 1846, during his visit to North America, the Primordial fauna of Barrande published for the first time during his journey in the New World.

In his able paper, entitled *Note sur le parallélisme des roches des dépôts paléozoïques de l'Amérique septentrionale avec ceux de l'Europe, etc.* (*Bulletin Soc. Géol. France*, 2<sup>e</sup> série, tome iv, p. 646, avril 1847, Paris), de Verneuil limits the "Etage supérieur du système Silurien," which is the true Silurian system, from the Grey sandstone and Oneida conglomerate, to the Upper Pentamerus limestone of the Lower Helderberg in the State of New York.

The extension of the Silurian system to other parts of North America has always been easily recognized.

The name Silurian is good, and has been accepted with little opposition, except when its author Murchison and his friends and followers carried it too far, by extending its meaning and covering strata of other systems.

#### VI. DEVONIAN SYSTEM.

Established by Sedgwick and Murchison, in 1839, with the help of Lonsdale's study of the Devonshire fossils, which proved an important and complete fauna of a character intermediate between those of the Silurian and Carboniferous, the Devonian system was not clearly made out in America, until de Verneuil's journey in 1846. Having studied with d'Archiac the fossils of the Rhenish Provinces, in 1842, and afterward the Devonian of Russia, 1842-1844, de Verneuil was admirably prepared to synchronize the American strata with those of Europe.

The divisions of the upper part of the New York series, called Upper Helderberg and Erie groups, worked out mainly by Vanuxem and Conrad — J. Hall having proposed only the names of Marcel-



lus and Genesee slates, Portage and Chemung group—after being referred by Mr. J. Hall first as Carboniferous or Mountain limestone, were synchronized by him in 1840 and 1843 with the Silurian system and the equivalent of the Ludlow rocks and the upper part of the Wenlock limestone of Wales.

Conrad regarded the Devonian of New York as composed only of the Old Red sandstone (Catskill group) and the Chemung and Portage groups. Mr. Hall accepted that view, and even so as to admit "perhaps a part of the Hamilton group;" "if the Devonian is to be regarded as a distinct system," a fact which he was not disposed to accept, saying: "In New York, however, no subdivisions can be made which are entitled to the name of systems" (see *Geology of New York*, Part IV, p. 516, Albany, 1843), a rather sweeping opinion, which received shortly after, from de Verneuil, such a rebuke as to put it forever out of the way.

De Verneuil saw at once that the Devonian as considered by Conrad was too limited, and he successively placed in it the Tully limestone, the Hamilton group, the Marcellus shales, Corniferous limestone, Onondaga limestone and Schoharie grit. Nay, more, with some hesitation, he regarded the Oriskany sandstone as forming the inferior limit of the New York Devonian.

As in all great systems, the divisions at the limits are more or less subject to discussions in regard to their places in one or the other system.

Farther west and south, the Devonian is less developed, and sometimes it is reduced to very small proportions both as to thickness and geographical extent. However, it is always easily recognized.

#### VII. CARBONIFEROUS SYSTEM.

In general the Carboniferous series are more homogenous in America than in Europe with the exception of Russia; and their great thickness in some parts of America, and vast geographical extension, make them a very important geological horizon, from Newfoundland and Cape Breton Island to northern California, and from Michigan to Texas. It is a geological landmark of the first order.

Fresh-water or brackish-water deposits of the same age as marine deposits exist in America as well as in Europe, only they are much less extensive, being limited mainly to the eastern part of the New World, the Alleghanies, New Brunswick and Nova Scotia.

Difficulties have arisen in regard to limits, as was to be expected with such a great system. The synchronism and parallelism with European strata have more especially been the subjects of discussions and difference of opinions. The name *Mountain limestone* has been much controverted, because in the first difficult and necessarily hasty reconnaissance made in the far west, it was used for some outcrop of Carboniferous strata, some of which belong, perhaps, to the upper part of the Carboniferous system; although this is far from proved, even now.

An example will show the character of the controversy. I found, in 1853, that the Carboniferous limestone lying on the granite and crystalline rocks of Tigras, or San Antonio Pass, east of Albuquerque, New Mexico, and which also forms all the top of the Sandia range of mountains overlooking the Rio Grande valley from San Bernardillo to El Paso del Norte, are truly contemporaneous or homotaxial strata of the Mountain limestone of England, and I made out the synchronism by fossils, as well as stratigraphically and lithologically. Afterwards I found *Brachiopoda* at Pecos Village, in the Sierra Madre or de Zuni and in the western part of New Mexico, now Arizona, near the great extinct volcanoes of the San Francisco Mountains, south of the Great Cañon of the Colorado, under material difficulties of a nature entirely uncontrollable, which prevented me from making out the stratigraphy as complete as I did in the Sierra de Sandia; and naturally enough, I concluded that this Carboniferous limestone, containing the most important and common *Brachiopoda*, belonged also to the Mountain limestone. But it seems that some parts of the Carboniferous limestone of the Rocky Mountains region may perhaps belong to the Upper instead of the Lower Carboniferous, a fact of little consequence, which does not affect a geological map on a small scale. Besides more careful investigations ought to be made there before accepting conclusions arrived at and propounded by the different surveying parties which, many years after me, have gone over the western part of the continent. For there is no reasonable doubt that the Mountain limestone fauna of Derbyshire and Belgium exists all over the Coal Measures of the West; the strata carrying the *Brachiopoda* all through the Carboniferous. It must be also kept in mind, as Mr. F. Springer says, that "the attempt to identify the Brachiopods of New Mexico with specimens from the Lower and Upper Carboniferous rocks of the Mississippi valley, has not been in all respects followed by satisfactory results" (*Amer.*

*Journ. Sci.*, 3d series, vol. xxvii, p. 97, 1884). At all events, a geologist of considerable practice in the Carboniferous rocks of the Mississippi region, Mr. Springer, has proved that I was right in referring the Carboniferous limestone of the Sierra de Sandia to the *Mountain limestone* of Europe, being the equivalent of the Sub-Carboniferous of Burlington, Iowa, and other places in the West ("On the occurrence of the Lower Burlington limestone in New Mexico;" *Amer. Journ. Sci.*, vol. xxvii, 1884).

In the basins of the Ohio and Mississippi rivers the Carboniferous series are divided into two systems, the Lower or Sub-Carboniferous and the Upper or Coal Measures; each being subdivided, into divisions of the third and fourth orders, called Kinderhook, Burlington limestone, Keokuk, St. Louis and Chester for the Lower Carboniferous; and Lower, Middle and Upper Coal Measures for the Upper Carboniferous.

The names and divisions vary according to the states in these basins. In the state of Ohio, the Sub-Carboniferous is divided into the Erie shale, the Waverly sandstone and the Maxville limestone. As to the Upper Carboniferous, called also simply Carboniferous, it is divided into Carboniferous Conglomerate, Lower Coal Measures and Upper Coal Measures. Nor is this classification of the state of Ohio absolute, definitive and *ne varietur*; for in the first volume of the survey we have another nomenclature. The Erie shale being placed in the Devonian, then comes the Waverly group, subdivided into Cleveland shale, Bedford shale, Berea grit and Cuyahoga shale; then above comes a lower Carboniferous or Chester limestone. As to the Upper Carboniferous, it seems to be divided into four divisions of the third order; first the Conglomerate, then Lower Coal Measures, after Barron Measures, and finally the Upper Coal Measures.

In Indiana we have a Knobstone group below the Keokuk group, which seems to replace the Kinderhook beds and the Burlington limestone of the Mississippi basin.

In Utah and Nevada the divisions of the Carboniferous are the Wahsatch limestone or Sub-Carboniferous, then Weber quartzite and the Upper Coal Measures.

In California the Carboniferous limestone seems to present anomalies as well stratigraphic as palæontologic, being formed of large lenticular masses of limestone inclosed in slates, which promise to exercise the imagination of the western geologists accustomed to the easy Carboniferous classification of the Mississippi valley.

Much confusion exists, and it will require clear-minded observers to classify and put in good order the American Carboniferous. The comparison with European Carboniferous, on a basis of practical knowledge, in the field of the two great series in both continents, is also much needed.

#### VIII. DYASSIC SYSTEM.

The Dyas was discovered, in 1853, by Jules Marcou, during his exploration with the expedition of Lieutenant A. W. Whipple by the thirty-fifth parallel, for the Pacific railroad, west of the pueblo of Zuni, between the Rio Colorado Chiquito and the San Francisco extinct volcano (Arizona); and also on Topofki creek between Mount Delaware and Old Fort Arbuckle (Indian Territory).

It occupies an important position among the different stratigraphic systems of the United States and is the equal of the Cambrian (Champlain), Silurian or Devonian. It is divided in two parts, having many similarities and relations with the European Dyas.

The lower part has two types: one marine formation which must be taken as the normal type, and a fresh-water formation which is homotaxial or local contemporaneous.

The marine Lower Dyas exists at Nebraska city and vicinity, penetrating into Iowa, also in Kansas and even in Illinois. It was discovered by Jules Marcou in 1863, at Nebraska city, and described by him and Professor H. B. Geinitz.

Messrs. F. B. Meek and C. A. White have done all they could to suppress the Dyas in Nebraska and Iowa, preparatory to allowing Messrs. J. Hall and J. S. Newberry to suppress it in the whole of North America. In his *Report on the Paleontology of Eastern Nebraska* (see "Final Report of Nebraska" by F. V. Hayden, Washington, 1872), Mr. Meek has contested every species and even every genus determined and described with good figures by Professor Geinitz in his *Carbonformation und Dyas in Nebraska*, 4<sup>to</sup>, Dresden, 1866. He likewise has tried with even more force, and all the array of argument he was able to muster, to blot out all the classification and nomenclature of J. Marcou, published in his two papers: *Une Reconnaissance au Nebraska*, and *Le Dyas au Nebraska* (see *Bulletin Soc. géol. France*, 2<sup>e</sup> série, tome XXI, p. 132 and tome XXIV, p. 280, Paris, 1864 and 1867).

However, Mr. Meek did not go so far as to deny the exactness of Marcou's sections, descriptions and dips of the strata, and geolog-

ical positions of the fossils, which he reluctantly accepts, being even obliged to pass over details of the section of Nebraska city, which he was unable to follow and make out.

Dr. C. A. White says: "It was with surprise and great regret that I received a copy of a work by Dr. H. B. Geinitz, entitled *Carbonformation und Dyas in Nebraska*, and learned from it that that able palæontologist had referred certain fossils described in it, which were collected by Prof. Jules Marcou from the rocks of eastern Nebraska, to the Dyas; and others from other localities but in the same region, partly in Iowa, to the Kohlenkalk" (*Geology of Iowa*, vol. 1, p. 248, Des Moines, 1870).

I am neither surprised nor do I regret to see that those two palæontologists have given opinions and determinations different from those arrived at by Geinitz and myself. It is, on the contrary, gratifying that Messrs. Meek and White should have contrived to concentrate on that easy and clear question all their knowledge of comparative geology and palæontology, and their views on classification.

The fresh-water type, of the Lower Dyas has been described by Messrs. Wm. M. Fontaine and I. C. White (*The Permian or Upper Carboniferous flora of West Virginia and S. W. Pennsylvania* Harrisburg, 1880; in *Second Geol. Survey Pennsylvania*, Report of Progress PP), as composed of the Upper Barren Measures of the Appalachian coal field, without marine fossils, but containing a very characteristic Dyassic flora. Very likely the greater part of the division called Upper Coal Measures in Ohio, Indiana, Illinois, Iowa, Kansas and Missouri, belongs to the Lower Dyas.

The upper part of the American Dyas has been recognized on the Big Blue river in Nebraska by Mr. W. C. Knight, in the Kanab valley of the Grand Cañon of the Colorado in Arizona, by Mr. C. D. Walcott, and in the Guadalupe Mountains in New Mexico by Shumard. Equivalent fresh-water deposits exist in North Carolina, where Dr. Emmons has discovered at the base of the Trias a series of conglomerate and sandstone, called *Chatham series*, below the Triassic coal of Dan river and of the vicinity of Richmond (Virginia), which evidently are remains of lacustrine and brackish-water deposits existing more or less developed all along the eastern bases of the Appalachian chains. In it, Dr. Emmons has found the oldest mammal, *Dromaterium sylvestre*.

In Lake Superior the celebrated conglomerate and Melaphyrs copper-bearing rocks of Keweenaw Point and Isle Royale and the

red sandstone of Montreal river, belong also to the upper part of the American Dyas, and are homotaxial and contemporaneous with the *Kupferschiefer* of Saxony in Germany, and of Perm in Russia.

The Dyas calls for systematic investigations by competent and practical geologists as well in the field, as in the cabinet and museums. Several monographs of the marine and fresh-water American Dyas will clear up all the obscurities and hesitations in regard to the age and equivalents of a system of strata which is as well represented in America, as it is in western Europe, Saxony and Russia.

Messrs. James Hall and J. S. Newberry have declared with a certain solemnity and emphasis at the Berlin International Congress of Geologists, that the Dyas does not exist in America (*The Work of the Intern. Cong. Geologists*, by Persifer Frazer, pp. 29 and 30, 1886). The only answer is that these two palæontologists are most unfortunate in all their dealing with geological questions. The Dyassic strata are there distributed from Carolinas to Lake Superior, Nebraska, New Mexico and Arizona, and to deny their existence does not suppress them.

#### IX. TRIASSIC SYSTEM.

The New Red sandstone was first discovered in North America, by Ed. Hitchcock, and Edwin James,<sup>1</sup> from 1819 to 1824, in the Connecticut valley and along the Canadian river (*Amer. Journ. Sci.*, vol. II, p. 146, vol. IV, p. 39; and *Major Long's Expedition to the Rocky Mountains during the years 1819-1820*, vol. II, p. 399); it extends from the Gulf of St. Lawrence and Lake Superior to the Carolinas, Texas, New Mexico, all over the Rocky Mountains region, Arizona, Utah, Nevada and even in California, and is one of the most remarkable and important systems in American geology.

Its legendary division into three great "Etages," as proposed by d'Alberty, has not yet received the proper consideration required, and a thorough study of the system in America is now one of the great *desiderata*. J. Marcou is the only one who has

<sup>1</sup> James committed the singular mistake of putting the "Red sandstone" and "argillaceous or grey sandstone," which he compares to the "New Red sandstone of English geologists," below the Carboniferous system. It was not until Marcou's exploration by the 35th parallel, in 1853, that the Triassic system was truly found, described and placed in its right place, in the Prairies of the Canadian river and in the Rocky Mountains region.

attempted to recognize the three divisions in the region of the Canadian river, from old fort Arbuckle to the Tucumcari and the Rio Pecos, in his exploration of 1853 (*Geology of North America*, pp. 10-16, 4<sup>to</sup>, Zurich, 1858). It was done only on stratigraphical and lithological grounds.

As in Europe and other parts of the world, generally the fossils are rare, limited to a few privileged localities and small area; and often of a nature (fossil wood and vertebrata) which will require years of careful researches and descriptions before they can be used as characteristic and leading species.

The fauna of the Muschelkalk has been discovered east of Fort Hall, in the Blackfoot basin and mountains, near John Gray lake and Snake river in southeastern Idaho and western Wyoming, by Dr. A. C. Peale in 1877. It is represented by three species of ammonites very similar to the *Ceratites nodosus* of the European Muschelkalk, which have been called by Professor A. Hyatt, genus *Meekoceras*, two or three *Arcestes?* with *Pecten* (*Aviculopecten*), *Terebratula*, *Monotis?* *Modiolina?* recalling the Muschelkalk fauna of Lorraine and Würtemberg.

The thickness of the American Muschelkalk is 3,000 feet according to Dr. Peale (*Jura-Trias section of southeastern Idaho and western Wyoming*, in *Bulletin U. S. Geol. Surv. Territories*, vol. v, number 1, p. 119, Washington, 1879).

Above the Muschelkalk there is a great division of red strata, called *Red beds*, by Dr. Peale, 1,000 feet thick, representing very likely the *Marnes Irisées* or *Keuper* of Europe.

The St. Cassian and Hallstadt fauna or Lower Keuper (Kohlenkeuper) of the Alpine Trias was discovered in 1860 by Messrs. Homfray and G. Blake in the Humboldt mining region of the territory of Nevada. Afterward Mr. C. King extended the discovery to the Pah-Ute range, Havallah range, and Desatoya Mountains. In the West Humboldt range, according to Mr. King, the Trias is composed of two great divisions: the lower or *Koipato group*, about 5,000 feet thick, containing only fragments of a *Nautilus*; and the *Star Peak group*, 10,000 feet thick, containing the St. Cassian and Hallstadt fauna, such as *Ammonites*, *Arcestes*, *Gymnoceras*, *Trachyceras*, *Endiscoceras*, *Halobia*, *Monotis*, *Modiomorpha*, *Posidonomya*, *Avicula*, *Terebratula*, *Spirifera*, etc. The Koipato group seems to represent the Muschelkalk of the Blackfoot (Idaho) and the Star Peak group the whole Keuper.

Farther east in Utah, Wyoming, Colorado and Arizona, the Trias seems to have the same type as on the Canadian river, and further researches will establish the relations of the different beds in that vast region.

In Plumas county, California, a spot of Alpine Trias has been signalized. Also some rather extensive areas at Houston Stewart Channel in Queen Charlotte Islands and on Vancouver Island, in British Columbia, are covered by Alpine triassic rocks, with the fauna of the Tyrol and Salz Kammergut.

In Sonora, Rémond de Corblincau has discovered a Triassic flora at Los Bronces on the Rio Yaqui. Finally, we have the beautiful Triassic flora of North Carolina and Virginia, so well worked out by Dr. Emmons, Heer, W. B. Rogers and Mr. W. M. Fontaine. The last named author thinks that it "is most probably Rhetic in age, and certainly not older" (*Older Mesozoic flora of Virginia*, Monographs of the U. S. Geol. Surv. vol. VI, p. 128, Washington, 1883); a rather narrow interpretation of geographical distribution of the plants of the American and European Triassic system; for the whole Trias is there, well developed, with a thickness of two or three thousand feet; and the fossil plants are distributed all over that great formation, and more especially in the lower part. If the upper part of the Virginia and North Carolina Trias is contemporaneous with the Rhetic, it will be proved by the remains of fishes or invertebrata, and not by the flora.

The flora seems to be the most important element of fossil remains existing in the Triassic system of North America. Fossil wood, more especially, is quite common everywhere, and in some places like the Lithodendron creek,<sup>1</sup> an affluent of the Colorado Chiquito, west of Zuni, it forms a petrified forest.

Reptiles have been found in New England, the Prince Edward Island, New Mexico and Texas; besides the celebrated numerous footprints of the Connecticut valley.

The Triassic deposits are mainly brackish, with a great deal of fresh water, and some entirely marine. The synchronism of the

<sup>1</sup> "The geologist of Whipple's expedition, Mr. Jules Marcon, finding scattered in the valley of this creek, and even in the bed of it, many trees petrified and changed into hard and beautifully colored jasper, some of them of considerable size and length, called the creek *Lithodendron*, from *Lithos* (stone) and *dendros* (tree). Lieutenant Whipple accepted the name and put it on his maps and reports, December, 1853." See "*Origin of some geographical names*" in the *United States Army and Navy Journal*, April 22 and 29, 1882, p. 884, 4to, New York.



strata is not easy to establish, and the homotaxy of the different divisions and groups will require more than ordinary prudence, learning and careful studies to be established on a good basis; but we can say now that several most important links exist already between the American and European Trias, as well palæontologically as lithologically.

The Lake Superior horizontal sandstone of La Pointe, Apostle Islands, Bois Brulé river, etc., may be pointed out as an example of extraordinary confusion, brought about by authoritative dictation. Until 1850, every geologist who has explored Lake Superior from Drs. D. Houghton, Charles T. Jackson, J. G. Norwood and D. D. Owen<sup>1</sup> to Ed. de Verneuil and myself has referred those sandstones to the Trias. But an agreement was made then to impose the age of the Potsdam sandstone (proofs or no proofs, it was no matter) and to rule out of American geologists any one bold enough to dissent. The leader in that disgraceful piece of dictation was as usual Mr. James Hall, backed by Logan, Dana, Whitney, Foster and Sterry Hunt. One would expect that such a strong coalition would have only one opinion, instead of which, we have a most astonishing variety of views, every member of the coalition except J. Hall and Whitney changing twice or even three times the age of this so-much-discussed formation. However, I must say in defence of the adversaries of the Trias, that they have remained true to their triassic opposition, varying only between the lowest crystalline rocks to the Champlain system included, a range of forty thousand feet at least, giving them a sufficient margin to discuss and disagree.

It is almost superfluous to say that I have never varied on the Triassic age of the Lake Superior sandstone, which possesses all the lithological characters of a littoral and arenaceous formation of the *Bunter sandstein*. As to palæontology no fossil has yet been

<sup>1</sup> From 1839 to 1849, D. D. Owen, in all his explorations and reports on the Chippewa land and Minnesota territory, regarded the Red Sandstone marls and conglomerates of Lake Superior as contemporary with the New Red sandstone of Great Britain. But in 1850, he was notified that he was to accept and use the Potsdam sandstone age, on the penalty of having his final report taken from him; and thus losing his many years of researches in the upper Mississippi region. Owen, having fresh in his mind the unjust and high-handed removal of Dr. Charles T. Jackson, in 1849, from the direction of the survey of the land district in the state of Michigan, preferred to submit to the dictation of the ruling association of authoritative geologists, and changed his views. This is the explanation of the "mystery" signalized in *Geology of America* by J. Marcou, p. 12, Zurich, 1858. I did not give it then because the time had not come to disclose it.

found, except several *Orthoceras*, embedded in the melaphyr and conglomerate of the Kupferschiefer of Pointe Keweenaw; rocks, accepted by everybody as older than the horizontal Lake Superior sandstone of La Pointe, the Apostle Islands and Bols Brulé river.

#### X. JURASSIC SYSTEM.

The old Oölitic series, or more appropriately the Jurassic system, was discovered in 1853 by Jules Marcou at the northwest corner of the Llano Estacado, in the area called Big and Little Tucumcari, New Mexico, during his exploration with Lieutenant A. W. Whipple for the Pacific Railroad by the 35th parallel. The two fossils found are both very characteristic of the Lower Oölitic and Oxfordian fauna of England and the Jura Mountains. One very common, and very well preserved, is a large *Gryphæa* of the *dilatata* group, which Marcou has called *Gryphæa Tucumcarii*. It represents the type so abundant in the Oxfordian of the whole of Europe, from near Portland (England) to the vicinity of Moscow, (Russia). The other fossil, of which I have collected one single specimen, with a few fragments, at the only section (Pyramid Mount) which I was able to explore—on account of the rapid march of the expedition in the Indian territories inhabited then by the Apaches—is a large *Ostrea* of the *Ostrea Marshii* group, and very likely a true *O. Marshii* identical with the species of the Lower Oölitic of England, the Jura Mountains and Würtemberg.

Farther west first at Laguna, and after near Zuni, I recognized also the Jurassic system, containing near Zuni a thin bed of coal in which, five years later, at the Moquis pueblo, Dr. J. S. Newberry found a "florula jurassic" (*Colorado Explor. Expedition* by Lieut. J. C. Ives, Part III, Geological Report, pp. 83 and 129, 4<sup>o</sup>, Washington, 1861).

Several years after my discovery of the Jurassic system in North America, it was signalized in other localities of the far west, by different observers and explorers.

Mr. Henry Newton says: "The first determination of the Jura in the far west was made by Professor Meek from fossils collected in the Black Hills by Dr. Hayden in 1857" (*Geology of the Black Hills of Dakota*, 4<sup>o</sup>, Washington, 1880). This intentional mistake is a part of the scheme of my combined adversaries to de-

prive me of the priority of the discovery of the Jura in North America.

At the Tucumcari, the Jurassic has been much eroded and submitted to great denudation, and its thickness is only about two hundred feet. In the Uinta Range the thickness is 250 feet, with a small Liassic fauna. In the Black Hills, the Jura has a thickness varying from two hundred to almost six hundred feet; it contains a rather limited fauna of forty-five species of the Lower Oölite and Oxfordian types.

In southern Idaho and Wyoming, according to Dr. Peale, the Jurassic has a thickness of 1,500 feet, with a Lower Oölite fauna.

In Nevada, West Humboldt Range, Augusta Mountains, the Jura is 5,500 feet thick, with a few Liassic fossils. In Queen Charlotte islands, the Oxfordian and Upper Jurassic faunæ are well represented by Ammonites of the *Macrocephali*, *Coronati* and *Planulati* groups, with some forms related to the Tithonic species.

Some Upper Jurassic strata of Colorado and Wyoming, referred at first by Dr. Hayden and his survey to the Cretaceous, and afterward by Mr. O. C. Marsh to the Wealden, contains a quantity of fragments of vertebra belonging to reptiles and mammalia. A huge *Dinosaurus*, described by Marsh under the generic name of *Atlantosaurus*, is used to characterize that upper part of the American Jurassic; and since 1877 Mr. Marsh has used the name "Atlantosaurus beds," to designate the Upper Jurassic of Colorado and Wyoming. Already twenty-five species of mammalia have been described. All belong to very low forms without any distinctive marsupial characters, but not far from the marsupial, for at first Mr. Marsh thought they belonged to that order. Lately he has created a new order for them, under the name of *Pantotheria*.

During the civil war (November, 1863), when visiting some friends in camp round Washington, I was shown a fossil "pine-apple" found on the farm of Dr. Jenkins, one mile south of the Baltimore and Washington railroad, sixteen miles from Washington, Prince George County, Maryland. I recognized at once a well preserved Purbeck's *Cycadææ* and referred the red and grey marls, in which it was found in company with pieces of petrified wood and broken pieces of indeterminable bones to the Purbeck formation of England. The little of what I saw there reminds me of the Purbeck group as I saw it at Portland Island and Durlstone Bay

near Weymouth, England, where so many specimens of mammalia (marsupial), reptiles, turtles, fishes and *Cycadecæ* have been found in its celebrated "dirt bed."

Lately the United States Geological Survey have called those white, red, and bluish grey clays and sands "Potomac formation." It is a fresh-water deposit contemporaneous with the Purbeck strata of Swanage and vicinity, Dorsetshire, England, which represent in North America, that most important upper part of the Jurassic system, called now on the continent of Europe the "Purbeckian."

In California, the Inferior Lias or Sinemurian exists in Plumas county and also near Lake Walker (western Nevada). As to the narrow band of slates between the rive. Stanislas and Merced, referred by the Geological Survey of California (1860-76) to the Jurassic system, it represents the Rhetic or Upper Trias.<sup>1</sup> Mr. J. D. Whitney, and afterward Messrs. G. F. Becker, C. A. White and J. S. Diller, have referred the apparition of gold in the Sierra Nevada to the Jurassic time, because gold exists in the Triassic slates of the Mariposa estate, and that gold quartz veins occur "between those slates and not simply near them." I have never said that those Triassic slates were not auriferous, but that the age of the apparition of gold in the Sierra Nevada was not Jurassic, being much older, ante-Taconic, or Lower Palæozoic at most.

Being deposited among golden rocks, which formed the beds and sides of the fiord, the Rhetic marl got as a part of its material, gold dust and even some small nuggets entombed in them. Long afterwards, during the great break and very strong pressure, which have given the Sierra Nevada its actual shape, those Triassic marls were laminated into slates, more or less metamorphosed like the other Palæozoic slates among which they were forced; and it is not strange that they partake of all the lithological and mineralogical characters of the older slates. But it does not follow because they "form an integral portion of the auriferous series" (*Notes on the Stratigraphy of California*, by G. F. Becker, p. 19, Washington, 1885) that the age of the apparition of gold in the Sierra Nevada is to be put so late as the Jura. The extrication of the gold from the quartz matrix being due to pressure, naturally

<sup>1</sup>Note sur la géologie de la Californie par J. Marcon (*Bull. Soc. géol. France*, tome XI, p. 407, Paris, 1883).

gold dust entombed in the Triassic marl of the Mariposa may have been united into small nuggets during the process of lamination and crushing. An extremely limited incident in a small portion of the *gisement* of gold in the Sierra Nevada has been taken as an indication of the true age of the apparition of that precious metal—another of the numerous errors of the Geological Survey of California.<sup>1</sup>

As it was to be expected, my discovery of the Jurassic system was contested at once and denied by Messrs. James Hall,<sup>2</sup> J. D. Dana, W. P. Blake, J. S. Newberry, the two Drs. Shumard, W. M. Gabb, F. B. Meek, Dr. C. A. White, H. Newton, etc.

The palæontologist, Mr. James Hall, has declared most emphatically that the "series of sandstone and clays beneath limestones (of Pyramid Mount in the Tucumcari area) which are of *unquestionable* cretaceous age;" and also "Having examined the specimens in Mr. Marcou's collection from this locality, I have no hesitation in saying that the specimens labelled by him as *Gryphæa Tucumcarii* (*G. dilatata* var. *Tucumcarii*) are the *Gryphæa Pitcheri* of Morton, and present no features either in form, characters, condition of preservation, or otherwise, which can serve to distinguish them from *Gryphæa Pitcheri*" (*Report on the United States and Mexican Boundary*, by Major W. H. Emory, vol. 1, part II, Geology and Palæontology, pp. 135 and 136, 4<sup>to</sup>, Washington, 1857).

The two Drs. B. F. and G. G. Shumard have identified my *Ostrea Marshii* with their *Ostrea subovata* of Fort Washita; regarding at the same time the Jurassic system of Tucumcari as the equivalent of the Fort Washita limestone which they have

<sup>1</sup>Announced with great pomp and emphasis by Mr. J. D. Dana, in *Amer. Journ. Sci.*, vol. xxx, 2<sup>d</sup> series, Nov., 1860, p. 424, who says: "no similar enterprise in the United States has ever been set on foot on a more liberal and enlightened basis, or opened under more favorable auspices as respects either the importance of the work to be done or the ability of those charged with the duty;" the Geological Survey of California, Director J. D. Whitney, after an existence of fifteen years, is a choice example of a failure among the numerous State surveys. It had not even published the smallest sketch geological map of the State, or of any part of California, nor given a general classification and nomenclature worth discussing. And instead of being a progress over what was then already known of the geology of California, it is a very marked backward move—even as regards the physical geography of the Sierra Nevada and Mount Shasta, classified by Mr. Whitney and his assistants as being devoid of glaciers.

<sup>2</sup>The amount and tone of the criticisms and strictures against Mr. Marcou's discoveries during his exploration by the 35th parallel of latitude—the first crossing of the United States and North American continent by a geologist—is anything but creditable to all those involved in them (see *Reply to the Criticisms of James D. Dana*, by Jules Marcou, Zurich, 1859).

placed above the "Arenaceous group" and "Red river group," that is to say in the Upper Cretaceous system of Texas.

Seldom has such an accumulation of errors, palæontological, stratigraphical and lithological been committed in American geology. For the mistakes did not stop there, and Messrs. J. Hall<sup>1</sup> and the brothers Shumard,<sup>2</sup> followed by Dr. R. H. Loughridge<sup>3</sup> and others, classified, as Lower Cretaceous or Dakota group of Texas, all the Triassic system besides the Jurassic; at the same time they contrived to synchronize the *Neocomian* of Fort Washita, the False Washita and Canadian rivers with the Marly Chalk or *Turonian*.<sup>4</sup>

The only other great error with which comparison may be made is the Taconic system. The same men, Messrs. J. Hall and J. D. Dana, by erroneous palæontological determination and false classification, and without any regard to practical geology and stratigraphy, have misled those who followed their views. To be sure there is no excuse for any practical geologist accepting errors, trying to force them as true, and spreading such notions on American geology; and all followers and propagators of J. Hall and J. D. Dana's stupendous mistakes will have to take their shares in the blame, which one day is sure to reach them. It is only a question of time, which now cannot last many years longer; for it has gone already too long for the good reputation of American geology. It is unnecessary to say that no one of my opponents had visited the Tucumcari area, and that until 1887, no geologist had been there except myself, in 1853. All the government exploring geologists from 1858, until now, have carefully avoided coming at a distance of at least one hundred miles from the northern part of the Llano Estacado and Pyramid Mount. The main object of Dr. J. S. New-

<sup>1</sup> *Report U. S. and Mexican Boundary and Geological Map attached*, 1857.

<sup>2</sup> *A Partial Report on the Geology of Western Texas*, by Geo. G. Shumard, Austin, 1886.

<sup>3</sup> *Tenth Census of the U. S.*

<sup>4</sup> Dr. G. G. Shumard in *A partial report on the Geology of Western Texas*, Austin, 1886, describes what he calls: "the Lower Cretaceous or Marly clay group," with remarks on the "palæontology" of this formation, at pp. 24 to 27, which to say the least is a curious reading. Combined with the paper of his brother Dr. B. F. Shumard entitled *Observations upon the Cretaceous Strata of Texas*. St. Louis, 1860, we have an array of errors almost incredible. For they have not the excuse of being unacquainted with the country—at least a great part of Texas and New Mexico—and it is hard to conceive how two observers, one a pretty good palæontologist, could have erred to such an extent.

berry's two explorations in 1858 and 1859 was to control and if possible to deny all Mr. Marcon's observations and discoveries in New Mexico. Truly he did all he could to justify Messrs. Hall and Dana's confidence, using to his best his opportunity. However, he was rather shy of recording his observations on a map; and after his failure of a geological map for the Colorado, Grand Cañon and Moquis expedition (in two sheets, Nos. 1 and 2 of Ives' *Report of the Colorado river*), he did not venture to publish a geological map of his second expedition in New Mexico, notwithstanding his long stay and explorations around Santa Fé.

His results were heralded, first in a letter to Mr. Meek, published in the *Amer. Journ. Sci.*, vol. xxviii, second series, p. 298, 1859, under the attractive and sensational title: *Dr. Newberry's late Explorations in New Mexico. He shows Marcou's so-called Jurassic to be Cretaceous*; then Dr. Newberry published two voluminous reports, *Colorado Exploring Expedition, Geological Report*, 4<sup>o</sup>, Washington, 1861, and *Santa Fé to Green river Exploring Expedition, Geological Report*, 4<sup>o</sup>, Washington, 1876;<sup>1</sup> and afterward from fear that his results might not be accepted, he dispatched in the region of Santa Fé and Fort Union, his pupil, Mr. J. J. Stevenson, with the special purpose to maintain them.

I have never answered Dr. Newberry's assertions and observations, because he did not give any really serious material proofs against me. He chose to publish loose observations, wanting in details, exactness, and palæontological knowledge. To be sure he has emphatically declared that the "Jurassic rocks do not occur on any part of the route followed by Mr. Marcou, and where he claims to have discovered them"<sup>2</sup> (*Explor. Exped. Santa Fé to*

<sup>1</sup> Any impartial reader of these two works will be astonished at the constant attacks of Dr. Newberry against me. It seems as if he were eager to vie with the bitterness and injustice of the previous attacks by Messrs. J. Hall, J. D. Dana and W. P. Blake; and as though he wanted to give the impression that it was almost a crime on my part, to have the audacity to maintain my observations, saying that such a proceeding is "almost unendurable."

The boldness with which Dr. Newberry has tried to throw against me his rather childish objections and undigested views and opinions on New Mexican geology is almost incredible, and shows what a prejudiced man and an *amateur* geologist and palæontologist is able to accumulate in order to prevent the acceptance of "truth."

<sup>2</sup> More than twenty years after, two maps, marked sheets Nos. 75 and 76 of the *Geological Atlas, Wheeler's Explorations*, have given the geology of my route from Inscription rocks, at the western foot of the Sierra de Zunil to Cactus Pass and the Bill William fork. On sheet No. 75 my name was inscribed without my knowledge with those of Messrs. G. K. Gilbert and A. R. Marvine as geological assistants, notwithstanding

*Green River*, p. 142); and he has used the dicotyledonous leaves with great advantage to him, as well as what he was pleased to call the *Gryphæa Pitcheri* and *Gryphæa Tucumcarii*; but at the same time he has always been very careful not to localize on a geological map the exact points where he found them, and he did not publish figures or descriptions of any sort of the two *Gryphæa* and of the leaves, an easy way to escape control and to appear as an expert in the matter. Two quotations of his reports will be sufficient to show his ability and what degree of confidence may be placed in him as a practical geologist and palæontologist.

At p. 42 of his *Geological Report of the Colorado Expl. Exped.*, Dr. Newberry gives a "Section of the Cañon of the Colorado, etc.," absolutely fantastical when compared with the sections of the grand cañon of the Colorado published lately by Major J. W. Powell (*Geology of Uinta Mountains*, p. 61, 4<sup>th</sup>, Washington, 1876) and Mr. C. D. Walcott (*Pre-carboniferous strata in the Grand Cañon of the Colorado, Arizona*, in *Amer. Journ. Sci.*, vol. xxvi, Dec., 1883, and *Classifications of the Cambrian System of North America*, in *Amer. Journ. Sci.*, vol. xxxii, Aug., 1886, p. 144, fig. 4, Grand Cañon section). A more easy section is difficult to find, and Dr. Newberry's complete failure to make it out, calling Potsdam, Silurian, Devonian rocks which have absolutely nothing to do with those formations, is anything but creditable.

At p. 83 of the same *Report of the Colorado Expedition*, Dr.

the erroneous reference by Mr. Gilbert of the rocks between Cañon Diablo and the extinct San Francisco volcano, to the Carboniferous instead of the Dyas (Permian) as I have called them. But for the sheet No. 76, I protested against the further use of my name, except if my determination of Dyas, Trias and Jura, as I found them, should be recorded on the map, sending a corrected map according to my observations and views. My corrections were not accepted, and consequently my name was withdrawn from sheet No. 76. Mr. Gilbert failed to recognize the Dyas and Jura, maintaining Dr. Newberry's views.

Lately (August, 1887) Captain C. E. Dutton has given a *Geologic Map of Northwestern Mexico*, 1884 (Sixth Ann. Rep. U. S. Geol. Surv., plate XIV, p. 128, Washington, 1885 [1887]), covering my route from Laguna to Zuni (see the *Geological Map of New Mexico*, by Jules Marcou, 1857); and in which he gives the Jurassic rocks, with a point of interrogation, first at Laguna, then on each side of the Sierra de Zuni and all around Zuni. Besides, the Dyas exists on the road from Aquafria to Inscription rocks. So, according to this observer, my discoveries of 1853 are accepted and made use of. To be sure I did not recognize the Dyas at the same spot, because it was covered then by a thick forest at the very narrow strip where I crossed it; but farther west near Cañon Diablo, I did not hesitate to refer to the Dyas, the magnesian limestone of the region. It is a first step toward the recognition of my discoveries after more than thirty years of negation of the exactness of my observations.



Newberry says, that he found near the Moquis villages in a coal seam a *Jurassic flora* which he describes, on pp. 129 to 132, with figures. In his second *Exploring Expedition from Santa Fé to Green River*, on p. 142, he says: "It is, however, true at the present time that no Jurassic plants have been found on this continent."

Nothing can be more conclusive than these two examples; for we have there, in indubitable form, Dr. Newberry's way of observing as a stratigraphist and a palæontologist.

His pupil Professor J. J. Stevenson was no more successful, and his *Report upon Northern New Mexico during the years 1878 and 1879*, in Wheeler's U. S. Geographical Survey, vol. III, *Supplement; Geology*, 4<sup>o</sup>, Washington, 1881, contains an extremely meagre account, without a single fact to sustain it or even worth recording, of the Trias and Jurassic, which he calls "Jura-Trias." Although he explored at leisure the Upper Canadian and the vicinity of Fort Union, Pecos and Galisteo villages, he did not find the *Gryphæa Pitcheri*, nor the *Gryphæa dilatata* var. *Tucumcarii*, so often quoted by Dr. Newberry in that same area; and he hardly refers to the dicotyledonous leaves, except in one instance of a few *indistinct impressions* near the Galisteo creek. Notwithstanding these defects and absolute want of proofs, Mr. Stevenson has given a geological map of *North Central New Mexico*, No. 3, showing a most extraordinary geographical distribution of Dakota cretaceous! of Carboniferous! and a "linear outcrop of Jura-Trias!" which endorses entirely Dr. Newberry's erroneous views.

I have so often shown the grave errors made in identifying the *Gryphæa Tucumcarii* and *Ostrea Marshii* with the *G. Pitcheri* and *O. subovata*, that it is almost superfluous to insist again. However, I have lately received specimens from Fort Washita, which have led me to review the whole subject, and the result is that there is absolutely nothing in common between the *Gryphæa Pitcheri* of Morton and Roemer and the *Gryphæa Tucumcarii*. Their characters are different in every way, and the Jurassic form of the Tucumcari area cannot be confounded with any cretaceous *Gryphæa* or *Ostracæ*. It is a case even more clear and with more distinguishing characteristics, as between the *Atops trilineatus* Emmons of the Taconic identified by Mr. J. Hall with the *Calymene Beckii*, or the *Elliptocephala asaphoides* regarded by him as an *Asaphus*.

In both cases, the palæontologist of New York has given false determination of fossils, in order to suppress great systems of strata, the Taconic and the Jura, of which his knowledge is most deficient.

#### XI. CRETACEOUS SYSTEM.

L. Vanuxem was the first geologist who discovered the Cretaceous system in North America. It was in 1828. (*Geological observations on the Secondary, Tertiary and Alluvial formations of the Atlantic coast of the United States of America. Arranged from the notes of Lardner Vanuxem*, by S. G. Morton, Philadelphia.) He also made the true distinction between the Secondary, Tertiary and Quaternary rocks.

The American Cretaceous system, like the European Cretaceous, is divided into three great divisions.<sup>1</sup> First, the Neocomian founded at Fort Washita and on the False Washita and Canadian rivers by J. Marcou in 1853. Until now the Neocomian exists only in Texas and in the Indian Territory. When it was deposited and consequently at the end of the Jurassic epoch, a great upheaving of the North American continent took place and there was then more dry land or *terra firma* than ever before and probably than now. But during the Middle Cretaceous great changes took place, subsidences can be signalized on two-thirds of the continent and finally, at the end of the Cenomanian, the sea invaded the whole basin of the upper Missouri, the Rocky Mountains region north and south, the Colorado basin, etc. It was the last extensive invasion of the sea in America. After the deposition of the Neocomian, a great erosion with upheaval in Texas and subsidence in all other parts of the United States, took place, and we have had the deposits of the "Lower Cross Timber group and basal shales" of Mr. Hill of Texas, representing in part the Gault and Green sand of England, the Cenomanian of France. It was immediately followed, in the Great Missouri basin, the Colorado, etc., by the deposition of the true *Marly and White chalk* or *Turonian and Senonian* of d'Orbigny.

<sup>1</sup> If the Gault or Middle Cretaceous is to be united with the Neocomian and form the Lower Cretaceous—the Cretaceous system being divided into two great divisions of the third order, instead of three—then we have in Texas in the Lower Cretaceous, the "Comanche series" of Mr. Robert T. Hill, and his "Lower Cross Timber series." I have no objection against this grouping of the Cretaceous system, which seems to obtain the majority of the opinions of the International Geological Congress, for the proposed geological map of Europe.

This last great division is the most common, being well developed with variable groups called Dakota, Colorado and Fox Hills; and also subdivisions, such as Fort Benton, Niobrara and Fort Pierre groups or sub-stages.

The exact synchronism of the Cretaceous of the Great Missouri basin was made by Murcou in 1863, when he recognized the Dakota group of Nebraska and Sioux city (Iowa), as belonging already to the true *Chalk* instead of the Lower Cretaceous of Messrs. Hall and Meek and forming a part of the Turonian or *Marly chalk*; referring at the same time the cretaceous marl of Galisteo (New Mexico) to the same horizon. The Cenomanian does not exist there, and the geologists, who even now refer the Dakota group to it, are mistaken.

In California the Cretaceous is limited to the northwest corner of the state and occupies a small area west of Mount Shasta. The Geological Survey directed by Mr. J. D. Whitney has called Cretaceous all the Eocene of Fort Tejon and Chico creek. Lately, Dr. C. A. White (*On the Mesozoic and Cenozoic palæontology of California*, p. 17, in *Bulletin U. S. Geol. Surv.* No. 15, Washington, 1885) has put forward as a sort of compromise, the notion that the "Chico group" is "later than any formation that has yet been referred to the Cretaceous period either in Europe or in America;" and is more recent than any one known except perhaps in New Zealand.<sup>1</sup>

A great Cretaceous division of the third order, of 5,000 feet thick, and unique in the northern hemisphere is a needless impossibility; for the Chico formation represents in California the Lower or true Eocene, having a fauna contemporaneous and most characteristic of the Tertiary epoch. The existence of only two or three degenerate representatives of genera of the Cephalopod family in the Chico group has misled Messrs. Gabb, Newberry and White; palæontology being for them narrowly confined only to the Cephalopoda, and to an absolute rule of extinction all the world over

<sup>1</sup> The same author says: "The Laramie group represents in America a great and important period of [the Cretaceous system] which is yet unknown in any other part of the world." (*Eleventh Ann. Rep. U. S. Geol. and Geogr. Survey for 1877*, p. 264, Washington, 1879). It appears from these two quotations that Dr. White regards the two great groups of Laramie and Chico, as Cretaceous, but not contemporaneous and at the same time both more recent than any groups in the Atlantic States and in Europe; an opinion rather eccentric which it will be difficult to harmonize with any classification and nomenclature.

of the *Ammonites*, *Helicoceras* and *Baculites*,—certainly a very peculiar interpretation of fossil remains and their use in geology.

An important remark is that the Dakota group is always regarded as *Lower Cretaceous* by Mr. J. Hall and his followers, and one of them has gone so far as to say, that I have pronounced the Dakota group as being at "the base of the Cretaceous as that series is accepted in America" (U. S. Geographical Survey west of the 100th meridian, vol. III, *Supplement—Geology*, by J. J. Stevenson, p. 153, 4<sup>o</sup>, Washington, 1881).

I am obliged to repeat once more that, in 1863, during my explorations in Nebraska (*Une reconnaissance géologique au Nébraska* par Jules Marcou, in *Bulletin Soc. géol. de France*, tome XXI, p. 132, Paris, 1864), I referred the Dakota group to the *Upper Cretaceous* of America, and forming the first group of the true *Chalk formation* containing *White Chalk*<sup>1</sup> as in Europe.

In 1853 I recognized the Neocomian far below the Dakota group, and in 1861 I gave a tabular view of the Texas cretaceous showing the three great divisions as in Europe (*Notes on the Cretaceous and Carboniferous Rocks of Texas*, by J. Marcou, in *Proceed. Boston Soc. Nat. Hist.*, vol. VIII, p. 93, Boston).

Dr. C. A. White, following Messrs. J. Hall, Shumard and Meek says: "It is a well-known fact that we have in North America no strata, which are, according to European standards, equivalent with the Lower Cretaceous of Europe, but that all North American strata of the Cretaceous period are equivalent with those of the Upper Cretaceous of that part of the world" (*Eleventh Ann. Rep. U. S. Geol. and Geogr. Survey for 1877*, p. 264, Washington, 1879); passing over my discovery in 1853, of the Neocomian in Texas and the Indian Territory, and my tabular view of the Texas Cretaceous (*Notes on the Cretaceous rocks of Texas*, in *Proceed. Boston Soc. Nat. Hist.*, vol. VIII, pp. 89-93, 1861). Lately, however, Dr. White, better informed on the Texas Cretaceous, by the original researches of his assistant Mr. R. T. Hill, has admitted the existence of the Neocomian, and with some reluctance the exactness of my tabular view of 1861 (*On the Cretaceous formation of Texas, and their relation to those of other portions of North America* by

<sup>1</sup>It was the first discovery and announcement of the existence of true *chalk* which can be used as such in America. Dr. Hayden more than two years after referred to the existence of true *chalk* in America, neglecting to say who made the discovery, and seeming to appear as the discoverer (*Description of an extensive chalk deposit on the Missouri river*, by Dr. Hayden in *Proceed. Amer. Phil. Soc.*, November 16, 1886, Philadelphia).

C. A. White,<sup>1</sup> in *Proceed. Acad. Nat. Sci.*, Philadelphia, February, 1887).

Mr. R. T. Hill has given recently a detailed classification and nomenclature of the Cretaceous of Texas: *The Topography and Geology of the Cross Timbers and surrounding region in Northern Texas*, in *Amer. Journ. Sci.*, vol. xxxiii, p. 299, 1887, in which he corrects the errors of Messrs. Roemer and the two brothers Shumard, already signalized by me as far back as 1861.

In a second paper entitled "The Texas Section of the American Cretaceous" (*Amer. Journ. Sci.*, vol. xxxiv, Oct., 1887, p. 287), Mr. Hill places the "Dakota" as the equivalent of his "Lower Cross Timber division," regarding also the "Eagle ford shales" as well as the "Dakota" as Middle Cretaceous. The fauna of the Eagle ford shales is identical with the fauna of the Dakota of Galisteo (New Mexico) and Sioux City (Iowa), and belongs to the inferior part of the Upper Cretaceous, or true Chalk formation, or Turonian. The Lower Cross Timber division may perhaps be referred to the Middle Cretaceous or Cenomanian; but it will require additional researches before we can reach any definite conclusion.

As to the Washita and Fredericksburg divisions, called now by Mr. Hill "Comanche series," their two faunas represent the Neocomian, the Aptian and the Cenomanian of the Jura Mountains, to which I referred them in 1853 and 1861.

The upper portion of the "Washita division" is certainly younger than the Neocomian and ought to be referred to the Middle Cretaceous, having a fauna which has many affinities with the Green

<sup>1</sup> Having committed himself so strongly in 1879, in regard to the age and synchronism of the great divisions of the American and European Cretaceous system, Dr. Charles A. White tried his best to escape from the responsibility, taking great care at the same time to appear as an original investigator and discoverer.

His paper is a rather singular eulogy of very poor works, saying "the work so well begun by Dr. Shumard;" "the really valuable work of Dr. B. F. Shumard;" "the admirable work of Prof. Roemer." It would have been more just, if instead of praising the great mistakes and constant errors of Messrs. Ferdinand Roemer, James Hall, the two Drs. Shumard and their followers, he had simply given their tables of superposition of the strata and general sections, with their homotaxial opinions, and placed them in full view of my table of 1861 of the Cretaceous strata of Texas, and of the general section lately arrived at by Mr. R. T. Hill. The comparison of those tables would have permitted every reader to judge for himself of the real value of the work, done by each observer.

But, instead, Dr. White, against all the rules of priority, passes over my discoveries of 1853 and my observations of 1861, and tries to make believe, that he is the discoverer of "the true relations of the different Cretaceous formations which have long been known to exist within the state of Texas," when he simply endorses and patronizes the investigations of his chief assistant Mr. Hill, and revives the discoveries and opinions given by me in 1853, 1861 and 1863.

sand and Gault fauna of England and the Jura (or the Aptlan, Albanian and Cenomanian of d'Orbigny).

Mr. Hill in his two papers identifies the *Gryphæa dilatata*, var. *Tucumcarii* and the *Ostrea Marshii* of the Pyramid Mount section, in the Tucumcari region, with species of his "Washita division" of central Texas, and refers the Jurassic system of the northwest corner of the Llano Estacado to the lower part of his Washita division, calling it "Jurassic and Neocomian of Mareou," a confusion which I did not make, having on the contrary insisted on their complete separation, and protested again and again against such erroneous views. As Mr. Hill has not yet explored the Tucumcari region, he has simply repeated the old mistake of Messrs. James Hall, Meek, the two brothers Shumard, Newberry and others. However he has separated, what he calls the *Gryphæa Pitcheri* var. *dilatata* Mareou (which I suppose is my *Gryphæa dilatata* var. *Tucumcarii*) not only from the true *Gryphæa Pitcheri* of Morton, but he goes so far even as to place it as a distinct variety from the *Gryphæa Pitcheri* var. *navia* Conrad, and the *Gryphæa Pitcheri* var. *forniculata* White, having then three varieties of the *G. Pitcheri*, besides the typical species. Farther on, Mr. Hill calls the *G. Pitcheri* an "anomalous form" which, according to his view, is "a Jurassic form, which has continued into the Cretaceous of this country;" adding that the *G. Pitcheri* var. *navia* of Conrad "is almost indistinguishable from the *G. arcuata* of the European Lias."

All these wavering and singular opinions, brought up and originated by the confusion of a false identification made by Mr. James Hall, show a desire to bring an excuse and pave the way for a change in the determination given with such certainty and authority in 1856 and 1857, by my adversaries. Messrs. Hill and White have just begun to realize that the *Gryphæa* contains less "confusing variations," than the "later *Ostreidæ*." A more careful study of the sub-genera *Gryphæa* will convince them that confusion has arisen only from a want of knowledge, and that the American *Gryphæa* are as well defined, and as good species, as the European ones; and more, they will see that the *G. Pitcheri* is easily distinguishable not only from the *G. arcuata* of the Lower Lias, but also from *G. obliqua* and *G. cymbium* of the Middle Lias, and from the *G. dilatata* of the Oxfordian. Close study and attentive comparative palæontology and stratigraphy will show them, how and why, errors on the part of my adversaries have been so persistently maintained.

## XII. LOWER TERTIARY SYSTEM.

The Tertiary series are divided into two systems, well marked by their stratigraphy, palæontology, lithology and geographical distribution. The marine and terrestrial faunæ although special to America, are similar and related by many a link, with those of the Tertiary of Europe, Asia and Africa.

The names Eocene and Oligocene can be retained and used with advantage on account of their diffusion and well-known meaning.

In North America, the inferior or Lower Tertiary is well developed in the states bordering the Atlantic and the Gulf of Mexico, Messrs. Conrad, Lyell, Tuomey, E. W. Hilgard, E. A. Smith and A. Heilprin have given good descriptions and classifications of the Lower Tertiary (*United States Tertiary Geology*, by Angelo Heilprin, 4<sup>o</sup>, Philadelphia, 1884). The Eocene is divided into four groups: (1) The Eo-lignitic at the base in Alabama and the Shark river deposits of New Jersey; (2) The Buhrstone or Chalk Hills of Alabama; (3) The celebrated Claiborne arenaceous formation of Alabama; and (4) The Jackson beds of Mississippi.

Then come the Oligocene represented by the "Wick limestone" and the Florida nummulitic beds.

In the interior part of the continent, upper Missouri basin, Rocky Mountains region, Colorado basin and the Columbia valley, we have immense and most important fresh-water deposits, contemporaneous and homotaxial strata with the European Lower Tertiary and with the marine deposits of California, the Gulf of Mexico and the Atlantic states. Four divisions into well-marked groups are easily recognized: first, the brackish-water formation called "Laramie group" representing the *Calcaire pisolithique de Paris*, *Lignite du Soissonais*, *Sables de Cuisse*, and the *Tufeau de Ciproly et Culcaire de Mons*, or *Lower Eocene* of Belgium. Then come the "Wasatch group," the "Fort Bridger group" and the "Uinta group," so well developed and so rich in fossil vertebrates south and southeast of Fort Bridger in Wyoming.

As to the "Chico creek group" of California, it represents the Lower Eocene of France (*Calcaire de Rilly*, *Lignite du Soissonais*, *Sables de Bracheux et de Cuisse*); the Lower Eocene of Belgium (*Landenien*, *Ypresien*, etc.), and the "Thanet sand" and "London clay" of England.

The "Tejon group," also of California, so well developed near Fort Tejon, is without a possible doubt the equivalent of the *Cal-*

*caire grossier* and the *Grès de Beauchamp* of the Paris basin. Its fauna is related and a continuation of the Chico group fauna. Dr. Charles A. White's description of *Chico-Tejon series* ("On the Mesozoic and Cenozoic palæontology of California," in *Bull. U. S. Geol. Surv.* No. 15, pp. 11 to 17, Washington, 1885) is neither correct nor complete; the palæontology of the Chico group being left out almost entirely.

I am the only geologist who has given yet a detailed section and description of the typical locality of Fort Tejon ("Report on the geology of a portion of Southern California," in *Ann. Report Geol. Surv. west 100th Meridian, for 1876*, pp. 158-172, Washington, 1876); and nevertheless Dr. White passes over it entirely unnoticed. This neglect is inexcusable, for I showed him in November, 1875, my specimens, pointing out the new species, with new names given by me, and which he promised to retain and use. Not only he did not publish my fossils, as it was his duty to do, as palæontologist-in-charge of Wheeler's survey, but he now carries his inaccuracy so far as to attribute to others the merit of having first sustained, by direct observations, the views of Conrad on the Tertiary-Eocene age of the Tejon group, when he knows well the priority of my observations made *in situ*, at Fort Tejon, in 1875.

But moreover, Dr. White asserts that it has been demonstrated: (1) that one or two Mesozoic types of Cephalopods pass up from the Chico group into the Tejon; (2) that the Chico-Tejon series is an *unbroken* series of strata; and finally (3) that the Miocene strata *everywhere* rest conformably upon the Tejon group. All these are erroneous suppositions against plain facts. No Cephalopods have ever been found at Fort Tejon, and the *Ammonites jugalis*, described by Gabb as a fossil from the Martinez group near Monte Diablo, belongs truly to the Chico group. The series at Chico creek and at Fort Tejon are both *broken*, and the complete series do not exist at either place of the two typical localities; Chico creek possessing only the Chico group, and Fort Tejon the Tejon group.

As to the Miocene, it does not rest conformably at the Arroyo de los Alisos near Fort Tejon, upon the Tejon group. A great *break* occurred there, and in several other localities of California, at the end of the deposits of the Tejon group (Upper Eocene or Oligocene).

The Laramie group, according to special and narrowly interpreted



palæontological studies, has been referred to the Cretaceous system, or even to a passage or transition group called *Post-cretaceous*, included in the Mesozoic (Secondary) and not Tertiary by any means.

The name ought to be spelled and written *Laramée*, from the French name *la ramée*, used often as a family name; like *Lafamme*, *Lapoire*, *Labrosse*, *Lasale*, *Laroche*, *Larochette*, *Larochelle*, *Larinière*, *Larive*, *Lapointe*, *Laplace*, *Lafèreté*, etc. The spelling *Laramie*, in which the *é* is replaced by an *i*, is a corruption, just like the spelling of *Aubry* as corrupted into *Aubrey*, for another group of American strata of the Carboniferous system in Arizona.

It is now a well proved and accepted fact that the Fox Hill group represents in the Missouri basin the extreme upper part of the White Chalk, or Danian and Craie de Maëstrich. It is a marine deposit, 500 feet thick, containing a quantity of characterized Upper Cretaceous fossils.

Above it and with a geographical distribution, quite distinct and much wider, we have an entirely different formation, a brackish-water deposit of more than 3,000 feet, sometimes 4,000 feet, thickness of strata, containing a fauna of vertebratæ, a special fauna of invertebratæ, and a special flora subdivided into three or even four florulæ.

The invertebratæ fauna is small — less than one hundred species — entirely of a brackish-water character and is closely related to a similar fauna of the Eocene of the Adriatic provinces of Austria. It has absolutely nothing of a Cretaceous character, while on the contrary it is connected by several common species with the Eocene and Miocene of America.

The vertebratæ fauna includes genera regarded as characteristic of the Cretaceous and also of the Tertiary.

The flora closely resembles the Senonian flora as it does both the Eocene and the Miocene flora. The apparition of many forms entirely Tertiary solve the question of synchronism, more especially when we consider the stratigraphy above the youngest Cretaceous group.

The Laramée group represents in America, the lower part of the Eocene or Paleocene (Calcaire pisolitique, calcaire de Rilly, Sables de Bracheux et d'Aix-la-Chapelle, etc.).

As to regarding such an important member of the Cretaceous system younger than any great division or even group existing

in Europe, as is the tendency among some geologists and palæontologists, it will be attended by serious objections without any advantages. The series in Europe are complete, and the Laramée deposits must be contemporaneous, and the homotaxis of some part of the European great divisions. To create outside of the general classification and nomenclature another index of geological times, invading and confusing the scale arrived at by long studies and plain facts, will be a step in the wrong direction, which will weigh heavily on the future progress of stratigraphy. It is not only a question of names, but above all a question of exactness and clearness in historical geology.

That a sub-group of one hundred or two hundred feet at most may embarrass classifiers; and that it will be prudent not to refer too hastily such a sub-group to one system or another is to be expected everywhere; and such cases occur as well in Europe, as in America, or any other part of the world. But it is very different with a great group or *étage*, forming an important factor in the table of strata. Geological age and time must prime all other questions; and the chronology must be made out and parallelized with the standard classification and nomenclature, without being stopped by imperfect knowledge of palæontological distribution of family and genus of animals and plants. The uniformity of palæontological rules and laws, as stated forty years ago at the dawn of the great researches on palæontology, cannot any longer be in the way and delay the advance of our studies; and decisions must be reached even when they interfere with the too easy dictations of those who think they possess in the drawers of their collections of fossils the keys of all the geological questions.

The affinities of a few forms of the Laramée group with Senonian fossils show only that some degenerate Cretaceous types continued to exist in America during a part of the Laramée time, but the appearance of a great number of new types which developed fully only after the Laramée, during the time of the deposition of the Middle and Upper Eocene and even Miocene, indicates that the Laramée group belongs to the Tertiary. On one side we have affinities with a small group of the fourth order only, the Senonian; and, on the other, most numerous and important affinities with two great systems and the whole series of the Tertiary, that is to say, with divisions of the second order and even of the first order. Palæontologically, it settles the question in the same direction and with the same conclusion that the great change between the marine

and brackish formations, and the different geographical distribution, all indicate that in America, as well as in Europe, the deposits at the end of the Cretaceous period have been replaced by well-marked and entirely distinct formations indicating a new order of phenomena in the distribution of terra firma, sea and land waters.

### XIII. UPPER TERTIARY OR HELVETIAN SYSTEM.

The Miocene is well represented in Maryland, Virginia (Yorktown) and in Carolina, but it is in California where the marine formation is most developed and may be taken as typical for North America. The great similarity of the California Miocene and Pliocene with the celebrated *Molasse* of Switzerland is startling and rare at such a great distance—half of the northern hemisphere.

Singular to say, both the Geological Survey of California conducted by J. D. Whitney and the United States Geological Survey conducted by G. F. Becker, have not only completely failed to see that beautiful lithological coincidence, but Mr. G. F. Becker even protests against it: "that any degree of similarity between the rocks of California and those of Switzerland should properly be considered as even tending to prove the age of either" (*Notes on the Stratigraphy of California in Bulletin U. S. Geol. Surv.*, No. 19, p. 11, Washington, 1885), putting aside completely the question of comparative lithology, which he "cannot conceive." As the palæontological evidence is, if possible, even more striking, we do not see on what principles those surveys have been or are conducted; until we remember that palæontology even more than lithology, is a sealed book to both Messrs. Whitney and Becker.

California is the best field in which to study the marine Tertiary series in North America. Until now, no observer, well posted upon the question, has taken the matter in hand; and it is a matter of regret for the progress of American geology that a competent person has not yet been chosen for the work.

The Pliocene exists round Los Angeles and other localities in California. It is only the upper group of the superior Tertiary and is mainly a part of the great Miocene formation, occupying a position relatively to it, somewhat analogous to the Oligocene in regard to the Eocene. Fresh-water Miocene and Pliocene formations exist east of the Rocky Mountains and in Oregon. Mr. O. C. Marsh has recognized five faunas and as many groups, and Mr. E. Cope three groups and six faunas. It was from the Miocene of Ne-

braska that Dr. Joseph Leidy obtained the materials for his first and justly celebrated great work on the vertebrata of North America, entitled *The ancient fauna of Nebraska*, 4<sup>to</sup>, Washington, 1853. Dr. John Evans visited and explored the Bad Lands of Nebraska during 1849 and brought with him the main part of the specimens used by Dr. Leidy.

#### XIV. QUATERNARY AND RECENT OR MODERN SERIES.

This series is divided into two systems or divisions of the second order. The inferior embraces all the old Quaternary or drift divisions, so well developed in America: (1) as alluvial drift along the Mississippi river basin, the Sacramento and San Joaquin valleys, the Colorado river, the Rio Grande del Norte; (2) as cave deposits and Loess; and (3) as glacial deposits in all the northern part of the eastern United States and Canada, in the whole area of the Rocky Mountains region, and in the Sierra Nevada and Cascade range.

The superior system, or actual deposits, is made by rivers, lakes, seas, delta, glaciers, landslides, sandy dunes, etc., etc.

#### GLACIAL EPOCH.

America is one of the most important parts of the world for the extension of old glaciers. Having been explored by Louis Agassiz, the father of the glacial epoch,<sup>1</sup> the glacial phenomenon has

<sup>1</sup> Dr. Otto Vogel, Mr. James D. Dana and a few others having lately called in question the right of priority of Agassiz, it is necessary and just to dispose of such erroneous notions.

Karl Friedrich Schimper has only the merit of first coining and using the word *Eiszeit* (glacial epoch), in a small bit of half-humorous and half-scientific poetry, printed and distributed at Neuchatel Feb. 15, 1837, when on a visit at the house of Agassiz, on the occasion of his (Schimper's) birthday. A few months later, Schimper wrote a letter to Agassiz, from the house of de Charpentier at Bex, which was printed by Agassiz under the title: *Ueber die Eiszeit*, in the "Actes de la Société helvétique des Sciences naturelles," pp. 38-51, after Agassiz's celebrated Discours de Neuchatel, le 24 Juillet, 1837. Beside, Agassiz in that discourse declares most frankly that the explanation given by him "est le résultat de la combinaison de mes idées et de celles de M. Schimper."

That is all Schimper's collaboration to the glacial epoch. He never studied carefully the glaciers, nor did he extend the glacial theory by direct observations in any part of the world except in the Swartzwald and Bavaria. His coming across a surface of Jurassic limestone polished and striated at Landeron near Neuchatel, Dec. 19, 1836, was nothing more than a new locality added to many others of the Jura borders, already well known to Agassiz, de Charpentier and de Montmolin.

As to Arnold Geoyot anticipating a number of Agassiz' most important conclusions on glaciers, as claimed by Mr. Dana, it is against all the well-known dates of Agassiz' explorations and publications; and I have sufficiently refuted, with all details and facts, such unfounded statements, in my letter *Glaciers and Glacialists*, published in "Science," July 23, vol. VIII, pp. 76-80, New York, 1886.

been compared in all its grand features and details with the classical ground and birthplace of the *Théorie glaciaire* in the basin of the Rhone. Nowhere in Europe, even in Scandinavia or the Alps, are there such a fine development and beautiful remains of glacial deposits and glacial works.

In Canada, New England, New York and the northern states comprising all the Great Lakes, parts of Pennsylvania, Ohio, Indiana, Illinois, Missouri, Nebraska, Dakota, an immense sheet of ice (*une calotte de glace*) covered all, giving almost one unbroken mass of ice similar to the one now covering Greenland, but on a much grander scale. That gigantic and enormous glacier has left its "Terminal Moraine" on a line, which follows more or less the 40th parallel, extending from the Atlantic coast to the upper Missouri (*Preliminary paper of the Terminal Moraine of the second glacial epoch*, by T. C. Chamberlain, in *Third Ann. Rep. U. S. Geol. Surv.*, p. 295 and Plate xxviii, Washington, 1883; and also *The Glacial boundary in Ohio, Indiana, and Kentucky*, by G. F. Wright, Cleveland, Ohio, 1884). In the Rocky Mountains, Wasatch Mountains, and the Humboldt Sierras great glaciers have existed and descended from all the great peaks into the valleys, leaving everywhere their marks of boulders, scratched rocks (*Roches striées et moutonnées*) and moraines. At Manitou and round Colorado City at the foot of Pike Peak, the traces of old glaciers are perfect and as beautiful as in Valais (Switzerland).

The Sierra Nevada is even more prolific in all the phenomena connected with the glacial epoch; the great mass of auriferous gravels, with few exceptions, being remains of old glacier deposits of the Quaternary period.

Singularly enough, the geologist, recommended by Agassiz to direct the Geological Survey of California, has failed completely to recognize not only the true age of the Quaternary glacial deposits of the Sierra Nevada, which he has assigned to the Tertiary (Eocene, Miocene and Pliocene), but has gone so far astray as to take the Sierra Nevada for a basis to deny the existence of the "Ice age," the greatest discovery of Agassiz!

To make the matter worse, Mr. J. D. Whitney has published his paradoxical and backward paper, in the quarto-serials founded by Louis Agassiz at his Museum; and in the same volume VII, which contains the last posthumous memoir of Agassiz. The title is: *The climatic changes of later geological times; a discussion based on observations made in the Cordilleras of North America*, by J. D.

Whitney—a controversial dissertation out of date and out of place. By out of date, I do not mean to say that the value of the paper would have been improved if it had been published fifty years ago; but only that it would have been then somewhat excusable, just as the anti-glacialist memoirs of Lecoq, Durocher, De Luc, Godefroy and Frapolli are.

As usual, Mr. James D. Dana, with his pen ever ready to sustain all the errors and prevent the progress of American geology, has taken up the same cause, attacking me most violently and erroneously, because I have quoted only very slightly the obnoxious memoir, in a carefully written and very exact paper on the *Glaciers and Glacialists*, published July 23, 1886, in "Science," vol. VIII, p. 76, New York. In a letter to "Science," vol. VIII, p. 162, August 20, 1886, Mr. J. D. Dana declares most emphatically that the *Memoirs of the Museum of Comparative Zoology* were not founded by Louis Agassiz, and that Mr. Whitney, although he "opposes Agassiz, has not a word of disparagement for Agassiz and gives no just cause of personal complaint,"—two assertions audaciously incorrect, which show that Mr. Dana is as low in the scale of geological critics as he is as an observer in historical geology by his incompetency during forty-four years to recognize the Taconic.<sup>1</sup>

Agassiz received a first grant of ten thousand dollars from the legislature of Massachusetts in 1863 for the publication of those memoirs, an act which must seem unparalleled and most extraordinary to any one who knows how parsimonious and extremely careful of the public purse the Massachusetts legislatures are. No one but Agassiz, and even no corporation, however powerful and influential, would have succeeded in getting money from the General Court and the Governor of Massachusetts for such special purpose as the publication of purely scientific memoirs, and nothing shows so well the great popularity and immense attractive power exercised by Agassiz, as the fact that, in the middle of the great civil war, when all the resources of Massachusetts were bent to support her armies in the field, he was able to obtain a considerable sum of money to print the transactions of his museum. As to the pro-

<sup>1</sup> Whatever may be Mr. Dana's talents and knowledge as a mineralogist and a zoologist, his intervention in historical geology has been most unfortunate, both as an observer and as a critic. His elementary books have disseminated false and erroneous notions on almost all questions of American stratigraphy; and under an appearance of competency, without any solid base to rest upon, he has contributed largely to prevent the acceptance of the truth.

priety, on Mr. Whitney's part, of publishing in memoirs, founded by Louis Agassiz,<sup>1</sup> a negation of the best and most important discovery ever made by that great naturalist, and to ignore him as being the discoverer of the existence of ancient glaciers in the British dominions, in New England and New York, in Brazil, in the straits of Magellan and in Chili, is, to say the least, most ungrateful and unjust. And the saying of Mr. Dana, that my remark "is essentially groundless," is another bold attempt to deprive a man of genius of the full share of his splendid discoveries.

Everybody knows well that Louis Agassiz founded the two publications of his Museum—*Bulletin and Transactions (Memoirs)*—as special contributions to the progress of natural history in the United States and not for its retardation. It is hard for his memory and such a noble example, that a very long and diffuse work has been published by the man who succeeded him in his chair of geology at Harvard University which, if accepted as true and sufficiently proved,<sup>2</sup> will not only hinder the advancement of a science so dear to Agassiz, but carry that great question of the "Ice age" more than fifty years backward, as it was before the justly celebrated *Discours prononcé à Neuchâtel* in 1837, by Agassiz before the Helvetic Naturalist Society as its president. What shall we say of the work of a man, who pretends to deny the "glacial epoch" and the "glacial doctrine," who not only did not give any credit to Agassiz for his superb work at the glacier of the Aar, and his discoveries of old glaciers in the United Kingdom of England and Ireland, and in North and South America, but passes over them as if Agassiz had done nothing on the subject? Is there anything more contemptible? Even the name of the discoverer of the "Ice age" is not given once in Whitney's large

<sup>1</sup> Agassiz did not give at first a general title to his 4to publications, using only as a subtitle "Illustrated Catalogue." But in his *Annual Report of Museum Compar. Zool. for 1867*, he has employed at p. 7, first the name *transactions* in a general way, and a few lines farther on as a special title with a great majuscule T, as the definite title: *Transactions of the Museum of Comparative Zoology*. Three years after his death the word *Transactions* was abandoned and replaced by *Memoirs*.

<sup>2</sup> Students, assistants of Museums and Surveys, and even naturalists of some reputation and renown have been deceived and led astray by Mr. Whitney. Happily all the practical geologists, more especially those interested in the glaciers and glacial theory, have regarded that singular paper as the most retrograde, incomplete and incompetent geological memoir published in America during the last forty years. The omission of more than two-thirds of the discoveries of old glaciers in the world, including those made by Louis Agassiz in Europe and in America, shows an almost total ignorance of the subject.

quarto volume. No excuse can be given for such an imposition on the geology of America, and on that breach of the most elementary courtesies and dignity due to Louis Agassiz.

#### LIVING GLACIERS.

Actual glaciers, although very small in comparison with what they were at the beginning of the Modern series during the Quaternary system, exist in the Rocky Mountains, the Great Basin, the Sierra Nevada, Mount Shasta, the Cascade Range, besides British Columbia and Alaska.

Mr. W. H. Holmes, U. S. Geological Survey, has found active glaciers in the Wind River and Tetons ranges of the Rocky Mountains. Others have been signalized since in the Flathead region, the Great Basin and northern Colorado. In the Sierra Nevada, Mr. J. Muir, as far back as 1872, described the "living glaciers of California." Mr. I. C. Russell showed "that nine glaciers now exist within the southern rim of the Mono Lake drainage basin;" and he adds that a larger number are to be found round Maclure, Lyell and Ritter peaks ("Existing glaciers of the United States" in *Fifth Ann. Rep. U. S. Geol. Surv.*, p. 315, Washington, 1885).

The late director of the Geological Survey of California, Mr. Whitney, not only did not find any glaciers in the Sierra Nevada,<sup>1</sup> but went so far as to deny their existence, even ten years after their descriptions by Messrs. Muir and Leconte. His former assistant, Mr. Clarence King, afterward geologist-in-charge of the fortieth-parallel explorations, has joined his protest against "the absurdity of applying the word *glacier* to a snow mass which appears and reappears from year to year" speaking also of "Mr. Muir's vagaries" (*Explor. fortieth Parallel*, vol. 1, p. 478, 4<sup>o</sup>, Washington, 1878).

<sup>1</sup> *Nevada* in Spanish, *Névé* in French, represent a fictitious form *nivatus*, from *nix*, *nievis*, snow, which is always applied by the Spaniards to mountains covered with perpetual snow or glaciers. In South America, from the Sierra Nevada de Santa Martha, in the northern part of Colombia, to Chill, we have numerous Sierra Nevada or only Nevada. The Sierra Nevada of Spain (Grenada), with its well known glaciers, is celebrated since the time of the Romans and the Moors. Consequently the name Sierra Nevada means a range of mountains with glaciers on their highest part. If Messrs. J. D. Whitney and C. King were conversant with Spanish physical geography, they would have reflected and probably paused before committing themselves to the flat denial of the existence of living glaciers in the Sierra Nevada of California, the very name of it meaning a range of mountains with glaciers.



Mr. G. Thompson, of the U. S. Geological Survey, made a topographical survey of the region about Mount Shasta in 1883. His map is published by Mr. Russell in the *Fifth Ann. Report*, opposite p. 330, plate XLIV. About a dozen glaciers exist; five of which are of good size, being several miles in length. Any one who has been in California and has previously seen either any portion of the Alps, or the Etna, knows well enough, even in looking at Mount Shasta from Marysville and all over the Sacramento valley, that it was covered with perpetual snow and consequently with glaciers. But the Geological Survey of California knows better. In September, 1862, the Director, Mr. J. D. Whitney, accompanied by his two assistants, Messrs. W. H. Brewer and Clarence King, all three claiming to be old travellers in the Alps of Switzerland and the Tyrol, and good experts on the glaciers, ascended Mount Shasta. The party had "considerable difficulty in crossing over a wide space on which the snow, almost icy in its texture was laid in sharp ridges" (*Geol. Surv. of California, Geology*, vol. 1, p. 340, 1865). Notwithstanding these snow "difficulties," Mr. Whitney and his companions did not discover any glacier! It was not until eight years later that one of that singular party of non-discoverers of glaciers, Mr. C. King, having become geologist-in-charge of the exploration of the 40th parallel, and accompanied by several members of his survey, one of whom was better qualified than himself in his knowledge of the Alpine glaciers, found at last three glaciers. As a matter of course, to excuse Messrs. Whitney and Brewer, as well as himself for their failure of 1862, Mr. King explains "why able scientific observers like Professor Whitney and his party should have scaled the mountain without discovering their existence" (*Amer. Journ. Sci.*, 3rd series, vol. 1, p. 157, 1871).

Such a feat was not to be left uncommemorated, and Mr. Thompson very wittily and most appropriately named the longest and first magnitude glacier of Mount Shasta, 3,800 yards in length, and covering an area of 1,900,000 square yards, at an elevation of 9,500 feet above the sea, *Whitney glacier*, "in honor of the former State Geologist of California" (*Fifth Ann. Rep. U. S. Geol. Surv.*, p. 333). A well deserved tribute due to the man who has declared that there were no actual glaciers on Mount Shasta, nor in the Sierra Nevada; and that the "glacial epoch" was only a myth, invented to explain everything in geology.

In the Cascade Mountains, glaciers exist at Mounts Rainier, Hood, Baker, Jefferson, the Three Sisters, etc. Also splendid and numerous Alpine glaciers cover a part of Alaska.

XV. EXPLANATION OF THE TABULAR VIEW OF AMERICAN CLASSIFICATION AND NOMENCLATURE.

The division in eight grand epochs, or series of the first order, is better balanced and gives a more just view of practical geology, than the old division into four classes: Primary, Transition, Secondary and Tertiary; or Azoiic, Palæozoic, Mesozoic and Cenozoic; or only into three great classes: Azoiic, Palæozoic and Neozoic. Such divisions are not well balanced, and their chronology was not established with a sufficient knowledge of the history of the earth. Their use has been confined to museums and theorists; but for practical purposes, when in the field, or at work at a general geological map of a moderate scale, they are too unequal, too great—except the Tertiary—to be of any help in surveying, mapping and classifying the rocks met with. They do not correspond any longer to the state of our knowledge. Up to forty years ago it was very well to use such great Vernerian epochs; but since the disentanglement of the older fossiliferous rocks, the extension all over Europe, America and a part of Asia, Africa and Australia, of the different systems of strata now well understood and sufficiently known, it is rather out of our time to maintain so incongruous and unbalanced a classification.

I have previously in the *Explication d'une seconde édition de la carte géologique de la Terre*, 4<sup>o</sup>, Zurich, 1875, used and explained the division in eight series, as better qualified to unite and condense the different great geological facts as they truly exist in nature.

The great "New York series" is due entirely to Messrs. Emmons, Vanuxem and Conrad. Their two other associates in the New York Survey, Messrs. Mather and Hall, not only did nothing to elucidate the stratigraphical classification; but, on the contrary, they have tried very hard, during forty years, to entangle and nullify the good observations made by the other members of the survey. And to refer, as is sometimes the custom among foreign and even some American geologists, the "New York series" to Mr. James Hall, as its author, is not only a gross error, but a great

injustice to Emmons, Vanuxem and Conrad, which ought to be checked by all means, and the sooner the better. Since 1858, in my *Geology of North America*, chapter ix, "a Synopsis of the History of the progress and discoveries of Geology in North America," p. 99, 4<sup>o</sup>, Zurich, I have striven to expose the truth, and Billings endorsed my efforts in a letter which I have published in my paper: "The Taconic system and its position in stratigraphic geology," p. 185 (*Proceed. Amer. Acad. Arts and Sciences*, vol. xii, Cambridge, 1885). Now it is to be hoped that the reference of the "New York series" to Mr. James Hall will cease and be replaced by its true discoverers, Emmons, Vanuxem and Conrad.

As subdivisions of the second order, I have given sixteen systems or *terrains*, all recognizable easily at first sight by any competent geologist all the world over. They exist in North America, beautifully developed, from Newfoundland to California.

In the third order, the divisions or *étages* are more numerous and consequently limited in their geographical extension. The Taconic series contains at least eight divisions, the New York series, nine divisions, etc., etc. It is a little difficult to find their homotaxial equivalents with the divisions of the same order in Europe, Asia and Australia. Some are, however, remarkably identical, palæontologically as well as lithologically and stratigraphically, on both sides of the Atlantic. The contemporaneousness is sometimes very striking and astonishing.

The divisions of the fourth order, called groups or *sub-étages* are all special and confined to one-quarter, or to one-third at most, of the United States and Canada and often even much less. Their equivalents outside of America are more or less doubtful and never to be entirely relied upon. Generally, a group or *sub-étage* is limited to a great physical geographical division such as the Missouri basin, the Great Basin, the Alleghanies, the Great Lakes, the Rocky Mountains, the Atlantic coast, the Gulf of Mexico, the Pacific coast, etc.

I have not put all the groups existing and already recognized in North America in the "Tabular View," because a great deal remains to be done in more than half of the country before constructing such a table with anything like permanency. Special monographs for each system are wanted in many cases before fixing the groups.

The divisions of the fifth order, called *beds* or *couches*, or *assises*,

or *strata*, or *band*, or *zone*, or *section*, always limited to a part only of a great geographical division and entirely local, vary according to places in regard to their importance and stratigraphical values. Some are confined to a few square miles and even one or two quarries or sections. In the "Tabular View" I have indicated very slightly in a special column that fifth order, because each part of the country and almost each state or province need such special subdivisions, often entirely limited to each one.

In order to show what they are, I have written on the "Tabular View" two or three examples only. In the very narrow St. John basin in New Brunswick, Mr. G. F. Matthew, who has worked out so well the St. John formation of the Middle Taconic, gives for the succession of members, first, five groups or *sub-étages*, numbered 1, 2, 3, 4 and 5; and each group is subdivided into belts called by him "bands" or "zones," or "sections." For instance, No. 1 is divided into bands *a*, *b*, *c* and *d*, each one characterized by special fossils and a special lithology.

In Texas Mr. R. T. Hill has lately shown that the Neocomian can be divided into two groups or *sub-étages*, called "Lower or Comanche Peak Division," also called afterward "Fredericksburg Division," and "Upper or Washita Division;" and then in each of those groups he gives subdivisions in beds, such as "Hippurites limestone," etc. In the tabular view, I have numbered the four beds of the Comanche Peak group. In New York, I have also numbered the five beds or the subdivisions of the Lower Helderberg.

I have not indicated in the tabular view any groups or division of the fourth order for the Lower Taconic, the Upper Carboniferous or Coal Measures, the Dyas, Trias, Jura and Upper Tertiary, because the study of these strata has not yet been carried out with sufficient details. However, the existence of the Rhetic, Sinemurian and Purbeckian indicated in the American Trias and Jura, shows already that important general groups may be recognized and created in those systems.

#### SYNCHRONISM AND HOMOTAXIS.

The synchronism and homotaxis of the divisions of the second order or systems, for the whole northern hemisphere, can be easily established, only the work should be done by practical geologists made well acquainted by studies in the field, not only with a more or less extensive country, but also with vast regions of the Old

and New World; an acquirement very seldom attained, owing to the difficulties to be overcome.

Often a geologist, after a tolerably good study of a state, or two or three states and territories of the United States, thinks that he can synchronize easily two or three systems of strata, not only all over North America, but also with Europe. Not knowing practically the geology of the greatest part of North America, and being totally ignorant of the geology of Europe, except what he can learn through a *Manual of Geology* or even special memoirs published on the question, he is inclined to generalize and give opinions which are always more or less erroneous and superficial. But even more: some geologists go to Europe, visit collections in the great museums, and even go a little in the field, and after three, four or twelve months of travel, believe that they know sufficiently the geology of Europe to make good synchronism between American and European formations. And, *vice versa*, for European geologists visiting America. Such observers have only a very imperfect knowledge, and are almost sure to make great mistakes; for it is not one or two months, or even one or two years, which are required for obtaining a pretty good practical acquaintance with European and American geology, but at least five, ten and even fifteen years passed in practical work on each continent. Even that is not enough to know well and be able to handle skilfully all the questions of homotaxis, but only some of them; for the geological systems are too numerous and too complicated to be well studied by a single geologist.

It is very easy and too common to speak at random either of the synchronism of the different systems between America and Europe, or of the impossibility of doing it, saying; "a system which is universal is artificial." Facts, practical facts well observed in the field, are what is wanted; and any one who has passed his life in practical work, will always say, that the repetition of almost identical, or at least very similar phenomena in every department of which geological science is composed, is not only of common occurrence, but the rule all the world over. The differences only strike the mind of the superficial geologists; similarities, on the contrary, are taken eagerly and with all their true meaning, usefulness and generalities by the specialists.

Mr. G. K. Gilbert in a paper on "The work of the International Congress of Geologists" (*Amer. Assoc. Adv. Sci.* at New York,

Aug. 10, 1887, Salem, 1887), which contains some good advice and is a commendable effort to put geologists on their guard against authoritative dictation and the tyranny of a too uniform taxonomy, speaks of "the fallacy of a world-wide unity of geologic systems;" and says also that, "there does not exist a world-wide system nor a world-wide group, but every system and every group is local."

As the author of the "Geological Map of the World" I have to say a few words. Not only the systems or divisions of the second order are easily distinguishable in every part of the northern hemisphere; but it is even not difficult to work them out in the southern hemisphere, although the similarities are a little less striking. I must say, that the obscurities and certain confusions are due more to the inability of the observers, than to the facts which exist in the field; facts, which require only to be properly made out by good practical geologists, as it has been proved again and again in South America, southern Africa, Australia and New Zealand.

That the *étages* or divisions of the third order are local is very true, as I have said before, but I cannot refrain from saying that there are a few even of them which are also almost world-wide; for instance the Keuper, the Lias, the Neocomian, and very likely others.

Mr. Gilbert "insists that a system which is universal is artificial." . . . "Take for example the Jurassic. It is a natural system in Europe." . . . "at the west (United States) the rocks called Jurassic *merge* with those called Triassic. In India, Medlicott tells us, a Jurassic fauna occurs at the summit of a great natural system containing a Permian fauna near its base. In New Zealand, according to Hutton, a continuous rock system, dissevered by great unconformities from the system, bears at top fossils resembling those of the lower Jurassic, and lower down fossils of Triassic facies. To establish a Jurassic system in either of these countries it is necessary to divide a natural system; and a Jurassic system thus established would be necessarily artificial."

All this argumentation is based upon the incorrect notion, that the Jurassic system in Europe is limited by "stratigraphic break" and "great unconformities." In the Jura Mountains, where the typical Jurassic system has been founded, and taken as a standard, the Triassic and the Jurassic systems are not separated by any break or unconformities of any sort, and according to Mr. Gilbert's phraseology *merge* into one another. The Neocomian is also in

concordance of stratification over the Jurassic in many parts of Europe. So we have exactly the same relations of rocks so far as breaks are concerned in Europe, in America, in Asia and in New Zealand.

A great system is marked not simply by "stratigraphic break" or "great unconformity" which are always local, but by differences in fauna and differences in lithology, which are a great deal more general and world-wide, at least for the fauna.

The examples chosen by Mr. Gilbert are unfortunate. In the western part of the United States, the Jurassic does not merge into the Triassic, but is as fully distinct palæontologically and lithologically as in the Jura mountains, at least all through the 35th parallel of latitude where I have discovered them in 1853. In India according to Medlicott, the Gondwana system is probably of "fluvial origin," consequently an exception like the Wealden; and in New Zealand the sequence of marine fauna is correct, and shows the generality of the palæontological rules extending even to the antipodes.

As to "the Chico-Tejon series as partly Eocene and partly Cretaceous," it is a repetition of a great mistake, the two formations belonging to the Lower Tertiary system and not to the Cretaceous, as I have shown repeatedly.

#### THE GEOLOGICAL MAP OF EUROPE.

In a previous paper "Notes à l'occasion du prochain congrès géologique international, etc." (*Bulletin Soc. Géol. de France*, tome XII, p. 517, Mai, 1884, Paris), I have shown some of the great objections and inconveniences against the publication of a "Geological Map of Europe" by the Congress. Mr. Gilbert opposes also that publication, giving excellent reasons. He says: "I also regard it as ill-advised that the Congress undertook the preparation of a map of Europe, for that—if more than a work of compilation—is a work of classification."

Time has already shown the exactness and importance of my objections and criticism. The international commission admit now, that it will be only an essay—as I said in 1884—instead of being a standard map. The number of years, first fixed at four or six years, for its completion and full issue, is now extended indefinitely; after seven years not a single one of the forty-nine sheets having yet been published.

A first difficulty, which appears to be insurmountable on account of its nature, is that the International Commission for the geological map cannot be complete, at any meeting or session, although composed of only eight members. Besides, the Congress is asked repeatedly to leave almost all the questions relating to the map to the *discretion* of the commission, and finally the commission itself is obliged to leave all the solutions of classification, coloring, etc., in the hands of the *Direction*, composed only of Messrs. Beyrich and Hauchcorne, at Berlin, or more exactly of Mr. Beyrich alone, Mr. Hauchcorne acting only as assistant for the material and manual part of the work.

In reality, the geological map of Europe by the International Congress will be the work of a single man, Mr. Beyrich, placed, very much to the detriment of the future progress of European geology, under the shield and responsibility of the Congress. It is the greatest act of authoritative dictation and tyrannical imposition, to which the science of geology has ever been submitted.<sup>1</sup> As the matter has become personal instead of international, it is just to say a few words of the man and his plans. Mr. Heinrich E. Beyrich is not well prepared for such direction; his works are only palæontological, biographical, bibliographical and on the stratigraphy of a special question very limited, the stratigraphy of the Tertiary series of Germany. He has absolutely no practice in dealing with geological maps of any kind, embracing great or even small area, nor with the classification and nomenclature of any geological system, except the Tertiary. As to Mr. Hauchcorne, Director of the School of Mines of Prussia, he is only an administrator, almost without a record in geology. When those two *savants* undertook the geological map of Europe for the Congress, they did not know what difficulties were in store for them, and Prussian-like, they have supplemented their deficiencies by a complete silence; never answering any inquiries of any member of the Congress, or

<sup>1</sup> The map has been conceded to the publishers, D. Reimer & Co., of Berlin, as a *perpetual property*; with the understanding that they may issue new editions every ten years — if asked for by the Congress, — also that they may publish a hypsometric map, and a reduction or *Tableau d'ensemble* of that Geological map, as a popular or school and college edition. All the governments of Europe are subscribers for nine hundred copies, to be distributed according to the importance of the different states. With such arrangements, it will be absolutely impossible for any geologist to publish a geological map of Europe; no editor, in the future, will ever think for a moment to compete with Reimer & Co. It is simply a monopoly of the geological map of Europe, an act which cannot be too much stigmatized as an attempt against the liberty of all original observers, and which will weigh heavily on the future progress of geology.



of the Commission nor even of the General Secretary of the International Commission, an unmistakable proof that the map is, and will remain, Beyrich's geological map of Europe.

Mr. Beyrich really cares only about three or four points. First, to have the Prussian division of the Rhenish Devonian used and accepted; second, to maintain the Jura divisions of von Buch in three great *étages*; and third, to see his own divisions and name of Oligocene accepted and placed on the general map of Europe. To take the Rhenish Devonian as the type for all Europe is a mistake; its great development is abnormal and an exception—just as the enormous Luxembourg Lias is another exception—and what is wanted for a whole continent as a type is a formation which is generally found in every part of it, with the same or nearly the same characteristics. Dumont, in his *Carte géologique de l'Europe*, 1855-57, has already attempted the extension all over Europe of the Rhenish Devonian divided in three great *étages*, and Mr. Beyrich's actual attempt will not be attended by better success. It is a move in the wrong direction, analogous to the publication of the "Geological map of Europe" by Murchison and Nichols in 1856, with the special purpose of annihilating the Cambrian of Sedgwick, coloring all the strata containing the Primordial fauna, the second fauna and the third fauna as Silurian.

The division of the Jura into three great *étages*, as proposed by von Buch, does not satisfy either the palæontology, or the lithology and orography of the Jurassic system all over Europe, and more especially in the Jura Mountains. If the scale of the map of Mr. Beyrich were at least 1 : 320,000, the Jura might be divided into four great *étages* (Lias, Lower Oolite, Oxfordian, Upper Oolite); but with a scale of 1 : 500,000, the Jura can only be divided into two great *étages* (1st, Lias—Lower Oolite; and, 2nd, Oxfordian—Upper Oolite).

As to the Oligocene, that special creation of Mr. Beyrich, it is good and may be used with advantage for the upper part of the Lower Tertiary system.

The tendency manifested at Berlin to suppress the Dyas as a system, and to join it as an *étage* or division of the third order only of the Carboniferous, is simply a move made by persons wanting in practical knowledge, and which has not the smallest chance to remain for any length of time in geology, because it is at variance with many plain facts in England, France, Germany,

Russia, the United States, etc. It is a momentary opposition prompted by personal rivalry and jealousy. The Dyas will take care of itself.

It is to be regretted that the geological map of Europe of the International Congress, after it had been voted and accepted, was not given to Austria instead of Prussia. Austria was the promoter of it, and in the hands of Messrs. Franz Ritter von Hauer, Edmund Mojsisovics and Melchior Neumayr, it would have been placed at least under the direction of geologists having great practice, as well in making general and special geological maps, as in handling classifications. The whole affair was arranged at the meeting of Bologna, and Italy wants too much to please Prussia to let the geological map of Europe go to its proper place at Vienna, where is the best geological school and centre now existing all the world over. As to poor France, the *vie victis* was rudely applied, even in geology.

#### XVI. CONCLUSION.

American geological classification and nomenclature not only have not been benefited or helped in any way by Messrs. J. Hall, J. D. Dana, W. E. Logan, J. D. Whitney, J. S. Newberry and their followers; but, on the contrary, they have been built up, little by little, against them and notwithstanding their most strenuous opposition and obstruction. If their opinions had been accepted American geology would be now fifty years behind our actual knowledge; and instead of having the "Tabular View" presented in this paper, we should have one without the Taconic, the Devonian, the Dyas, the Trias, the Jura, the Neocomian, the Eocene and Quaternary (California), the Ice age and actual glaciers! That is to say, American geology would have remained stationary with as few and insignificant changes and modifications as possible, where it was in 1837.

When I took in hand the Taconic question at the earnest request of Barrande and Emmons, it was under rather discouraging circumstances. Dr. Emmons had just left Albany for North Carolina, September, 1860, never to return. Barrande was too far away; he was also advanced in age and was too busy with his own work in Bohemia to pay any more attention to the Taconic,

after the publication of our joint paper of 1860 and of his *Documents anciens et nouveau sur la faune primordiale et le système Taconique en Amerique* in 1861.

Billings was not to be relied upon, on account of an incurable illness, joined to his peculiar position in the Canada geological survey, then in the hands of Messrs. Logan and Hunt; and as to Colonel Jewett, he refused all his life to publish anything of his notes and observations on geology.

So I was left alone against the united opposition of the old adversaries of the Taconic system. If the opposition to Emmons during eighteen years, 1842-1860, was of a nature verging on persecution, it was much more so with me. For I had to sustain the whole weight of the most unscrupulous opposition, not only on the Taconic, but also on the Mountain limestone of the Sierra de Sandia in New Mexico, the Dyas of the Colorado Chiquito and Nebraska City, the Trias of the Canadian river and of the Colorado Chiquito, the Jurassic system of the Tucumcari area, Cañon Blanco, Cuesta, Laguna, Inscription rocks, and of Zuni, the Neocomian of the false Washita and Canadian rivers, the Tertiary of Fort Tejon and Chico creek, the Quaternary auriferous gravels and the age of the apparition of gold in California, the copper-bearing rocks of Keweenaw Point and the Lake Superior sandstone, and finally on the Ice age.

The opposition to Dr. Emmons was mere child's play, in comparison with what has been done against me. My observations extending from Quebec to Los Angeles, and from Lake Superior to Nebraska, Texas, New Mexico and Arizona, have all been flatly and systematically contradicted and denied, never by direct observations made in the field, but simply by guesses, false determinations of fossils and erroneous notions on American geology.

My name has been ruled out of the list of authors on American geology and palæontology, by the successor of Agassiz as Professor of geology at the Lawrence Scientific school of Harvard University and who is at the same time Secretary of the Agassiz Museum, in which are preserved many of my collections, made in America and in Europe; and some of the rarest and best specimens of fossils I have met within my explorations (*List of American authors in Geology and Palæontology*, by J. D. Whitney. Library of Harvard University, Bibliographical contributions; edited

by Justin Winsor, No. 15, republished from the *Bulletin of Harvard University*, 4<sup>to</sup>, Cambridge, Mass., 1882).

To all the adversaries of Dr. Emmons a dozen at least of other opponents have been added, always under the lead of Messrs. James Hall, James D. Dana and J. S. Newberry. Never has such a united opposition been offered to the works of a single geologist, during so long a period, almost forty years. It is unparalleled and unique not only on account of its duration, but also for its character of exceptional bitterness and animosity unequalled in geology.

Undisturbed in their assertions, my adversaries have the field all to themselves, and with iron rod in hand they have assumed the whole control of American geology; denying plain facts, giving false determination and false identifications of fossils, incorrect sections, geological maps with false and incomplete classifications, and nomenclatures so imperfect as to be monstrously mutilated.

They have gone so far in their blind opposition, as to use as tools against me specimens collected by me and my field notes written during my explorations from the Mississippi river to the Pacific shores in 1853-54, and put honestly into their hands by my friends, Generals A. A. Humphreys and A. W. Whipple, in order to secure my discoveries.

Messrs. James Hall and W. P. Blake have erroneously denied the most careful, difficult and sagacious observations I have made, aggravating as much as it was in their power the wrong done me by the tyrannical and most unjustifiable action of the famous Jefferson Davis, who as Secretary of War deprived me of the right of making my final report — a process absolutely without precedent in the history of geology, and which places James Hall, W. P. Blake and Jefferson Davis in their true and unenviable light.

There is not a single question in American geology which has not been submitted to their deadly influence. Even the history of discoveries has been falsified with the greatest ignorance and partiality, and all that in order to please their own fancies, and to shield their errors and mistakes. They have treated all questions without any knowledge whatever of comparative geology, comparative palæontology or comparative lithology, not only in Europe and other parts of the world, but even within North America. Discourtesy marks the whole proceeding; and the blindness of jeal-

ously has seldom if ever—in science—shown such an array of reprehensible acts, and persistence in wrong-doing.

Two geologists alone have had to sustain during forty-five years the repeated assault of the most influential scientific periodical, *The American Journal of Science*, led by its principal editor Mr. J. D. Dana, with the support of Mr. J. Hall and a whole staff of contributors, Sir Roderick L. Murchison among them. What an attempt against the liberty of opinions and observations, and truly against the progress of geology!

Notwithstanding such a powerful obstacle, American geology has progressed, thanks to a few independent and honest observers, who after all have never been entirely extinguished, nor paralyzed by the autocratic dictation and manœuvring of a dominant party whose rules have been to oppose and even suppress all the observations not originated or nursed among its members.

I have done all that was in my power to call the attention of geologists, as well in America as in Europe, to many of the most important questions offered by the North American continent; and if I have not entirely succeeded in freeing American geology of the dictations of an aristocracy so baneful and demoralizing, which has opposed almost all the progress, and prevented as much as it could the expression of all original opinions and observations, I hope, however, that I have not suffered in vain, and that the time has now come when all geologists, on the American continent, will be able to observe and state their results without fear of being ostracized and treated as a *paria*, as was the common fate of Dr. E. Emmons and myself. I do not complain, however, for after all it is no ordinary compliment to have aroused the jealous rancor and ire of all the geologists who have contrived to monopolize and control in their personal interests the researches executed in both hemispheres, and to have passed my life, almost without remuneration of any sort, working all the time for truth, progress, honesty and justice.

I must add, as a great compensation, that I have enjoyed the confidence, the trust, and often the intimate friendship of all the best and most honest geologists and palæontologists. Quoting only the dead: Barrande, Lyell, Louis Agassiz, Ebenezer Emmons, Alcide d'Orbigny, Pictet, Deshayes, d'Omalius d'Halloy, André Dumont, de Koninck, Boué, de Verneuil, Delesse, Fournet,

Grüner, Edouard Lartet, Gervais, Jukes, John Phillips, Thomas Davidson, John Morris, Thomas Wright, Charles T. Jackson, S. Morton, T. Conrad, Thomas Oldham, Stoliczka, Auerbach, Grewingk, Opper, von Hochstetter, von Haast, Haidinger, Thurman, Merian, Escher von der Linth, Studer, Heer, Bartolomeo Gastaldi, and Quintino Sella. Besides, I have the privilege and the rare honor of having received approbation, advice, and even public quotations of my researches, at the beginning of my career as a geologist, from such great and original observers as Alexandre Brongniart, Constant Prévost, P. Louis A. Cordier, Alexandre von Humboldt, Léopold von Buch and Jean de Charpentier.

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TABULAR VIEW OF AMERICAN CLASSIFICATION AND NOMENCLATURE, BY JULES MARCOU, JULY, 1887.

SERIES or 1st order.	SYSTEMS OR TERRAINS, or 2nd order.	DIVISIONS OR ÉTAGES, or 3rd order.	GROUPS OR SUB-ÉTAGES, or 4th order.	BEDS or 5th order.
VIII. Modern.	1. Recent or actual.	Fresh water deposits..... Existing glaciers..... Marine and brackish water deposits .. Terrestrial changes..... ..... Glacial deposits or Ice period (Venetz, 1833, de Charpentier, 1834-40 and Louis Agassiz, 1837-40-73.)..... Old Diluvium.....	Rivers and lakes deposits. Nêvés, ice, moraines. Littoral, estuary and delta. Sand dunes, landslides, earthquakes. ..... { Erratic blocks, boulder clays, polished and scratched rocks, coarse gravels, contorted drift. ..... Diluvium and cave deposits.	
	3. Upper Tertiary or Heivetau.	Pliocene—Molasse of Los Angeles [California]. <i>Equus</i> beds. Miocene—Molasse of San Fernando [California]. <i>Oreodon</i> beds. ..... Oligocene—Florida nummulitic and Wickburg limestone—Fort Tejon formation—White river formation. ..... Eocene—Chico creek formation [California].....	Atlantic and Gulf. { Jacksonian. Claibornian. Bnhirstone. Eo-Lignitic. ..... Interior deposits. { Uinta group. Bridger " Wasatch " Laramée " .....	
VII. Tertiary.	4. Lower Tertiary.			

TABULAR VIEW OF AMERICAN CLASSIFICATION AND NOMENCLATURE. (Continued.)

SERIES or 1st or 2nd order.	SYSTEMS OR TERRAINS, or 2nd order.	DIVISIONS OR ÉTAGES, or 3rd order.	GROUPS OR SUB-ÉTAGES, or 4th order.	BEDS or 5th order.
VI. Secondary.	<p>5. Cretaceous. (Vanuxem, 1828.)</p> <p>6. Jurassic. (J. Marcon, 1853.)</p>	<p>Chalk—New Jersey (Morton, 1824, and I. yell, 1844)—West of the Mississippi (J. Marcon, 1853-63).....</p> <p>Gault—[The upper part of the Washita group belongs to the Middle Cretaceous]</p> <p>Neocomian—Texas and Indian Territory (J. Marcon, 1853)</p> <p>Upper Oolite—<i>Atlatosaurus</i> beds [Wyoming, Colorado]—Tithonic [Queen Charlotte Islands].....</p> <p>Oxfordian and Lower Oolite—[Tucumcari, Ilano Estacado—Black Hill and Queen Charlotte Islands].</p> <p>Lias—[Uinta, Idaho, Wyoming and Nevada].....</p>	<p>Missouri basin. Texas (Hill, 1827). Fox Hill group. Colorado " " { Ripley group. Rotten limestone. Eagle ford shales. Dakota " " } Lower Cross timber Washita group. Fredericksburg group.....</p> <p>1, 2, 3, 4.</p> <p>{ Purbeckian of Maryland or Potomac formation.</p> <p>Lower Lias or Sinemurian [Plumas county, California].</p>	<p>1, 2, 3, 4.</p>
V. New Red Sandstone Hitchcock and James (1819-24).	<p>7. Trias.</p> <p>8. Dyas. (J. Marcon, 1853.)</p>	<p>Keuper or Variegated Marls (J. Marcon, 1853)—Canadian river—Red beds [Idaho]—Star Peak Division [Nevada].</p> <p>Middle Trias—Michelbank (J. Marcon, 1853, and Peale, 1877)—Canadian river,—<i>Megaceras</i> beds [Idaho]—Kojinato [Nevada].</p> <p>Bunter Sandstein (J. Marcon, 1853)—Canadian river—Lake Superior sandstone.</p> <p>Upper Dyas or Zechstein and Kupferschiefer—Big Blue river [Nebraska]—Kanab valley [Arizona]—Cratharn series [North Carolina]—Keweenaw Division [Michigan].</p> <p>Lower Dyas—Nebraska City—Upper Barren Coal Measure above the Wernersburg sandstone [West Virginia and Pennsylvania].</p>	<p>{ Upper Keuper or Rhenic—Mariposa [California] and upper part of the coal states of Dan river [North Carolina] and James river [Virginia].</p>	



TABULAR VIEW OF AMERICAN CLASSIFICATION AND NOMENCLATURE. (Continued.)

SERIES OF 1st order.	SYSTEMS OR TERRAINS, or 2nd order.	DIVISIONS OR ÉTAGES, or 3rd order.	GROUPS OR SUB-ÉTAGES, or 4th order.	MEMB or 5th order.
IV. Carboniferous.	9. Upper or Coal Measures.	Upper Coal Measures (Dyas?)—Ohio, Indiana, Illinois, Iowa, etc. Middle Coal Measures.—Missouri, Iowa, Illinois, Indiana, etc. Lower Coal Measures and Conglomerate.—Ohio, Iowa, Missouri, etc. Lower Carboniferous or Chester.....	Ohio. ..... Illinois, Missouri. { Cuyahoga shales. Berea grit. Bedford shales. Cleveland shales. ..... { St. Louis group. Keokuk Burlington " Kinderhook "	
	10. Lower or Sub-Carboniferous.	Waverly formation (Lock, 1838).....	{ Catskill. Chemung. Portage. Hamilton. ..... { Upper Helderberg. Oriskany sandstone. ..... Lower Helderberg.....	1,2,3,4,5.
	11. Devonian. (Conrad and de Verneuil, 1839-46.)	Upper Devonian..... ..... Lower Devonian.....	{ Water lime. Onondaga salt group. Niagara. Clinton. { Oneida and Medina. ..... { Lorraine shales. Utica shales. ..... { Trenton limestone. Black river. Birdseye. ..... { Chazy limestone. Calceiferous sandstone.	
	12. Silurian, or Third Fauna. (de Verneuil, 1846.)	Lower Helderberg (Conrad, 1839) Salina..... ..... Niagara (Conrad, 1837).....	Upper Champlain .....	
III. New York. Vanuxem, Emmons and Conrad (1837-43).	13. Cambrian, or Second Fauna. (Champlain of Emmons, 1842.)	Middle Champlain or Trenton..... ..... Lower Champlain.....		

TABULAR VIEW OF AMERICAN CLASSIFICATION AND NOMENCLATURE. (Continued.)

SERIES or 1st order	SYSTEMS OR TERRAINS, or 2nd order.	DIVISIONS OR ÉTAGES, or 3rd order.	GROUPS OR SUB-ÉTAGES, or 4th order.	BEDS or 5th order.
14. Upper Taconic or Supra-Primeroidal Fauna or zone of the <i>Hyaloccephalus</i> and <i>Lithogyrus</i> . (J. Marcou, 1882-84.)	Potsdam sandstone (Emmons, 1888)—Tonto (Gilbert, 1874). Swanton slates (Marcou, 1881-82)—Pogonic (Walcott and Hague, 1888).	{ Upper Potsdam [Wisconsin and Sara- logyl]. { Lower Potsdam [Keeseville, N. Y.].		
	Phillipsburgh and Point Lévis formation (Marcou, 1882).	{ Hamburg shale and limestone. Secret cañon and upper part of the Pros- pect limestone [Nevada] (Walcott and Hague, 1888).		
15. Middle Taconic or Primeroidal Fauna or zone of the <i>Onchodonta</i> and <i>Parasidolites</i> . (Emmons and Bartraude, 1880.)	Georgia formation (Marcou, 1881)—Lower part of the Prospect mountain limestone [Nevada] (Walcott and Hague, 1888).	Saint John formation [New Brunswick] (Matthew and Hart, 1862-65)— <i>Parasidolites</i> zone of Braintree, Mass. and St. Mary's Bay [Newfoundland].	{ Group 1..... a, b, e, d. " 2..... " 3..... " 4..... " 5.....	
	16. Lower Taconic or Infra-Primeroidal Fauna. (J. Marcou, 1884.)	Chuar and Grand Cañon formation (Powell, 1876)—Sand- stone of Great Bell Island [Newfoundland] (Mur- ray, 1882) Slates of St. John's [Newfoundland] (Murray, 1868-82).		

I. Primitive or Crystalline or Azoic rocks.  
Eruptive rocks.

For explanations, see *American Geological Classification and Nomenclature*, 8vo, Cambridge, 1888.

