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VII
AMERICAN GEOLOGY.

LETTER

ON SOME POINTS OF THE GEOLOGY OF TEXAS,
NEW MEXICO, KANSAS, AND NEBRASKA; ADRESSED TO
MESSRS. F. B. MEEK AND F. V. HAYDEN.

BY

JULÉS MARCOU.

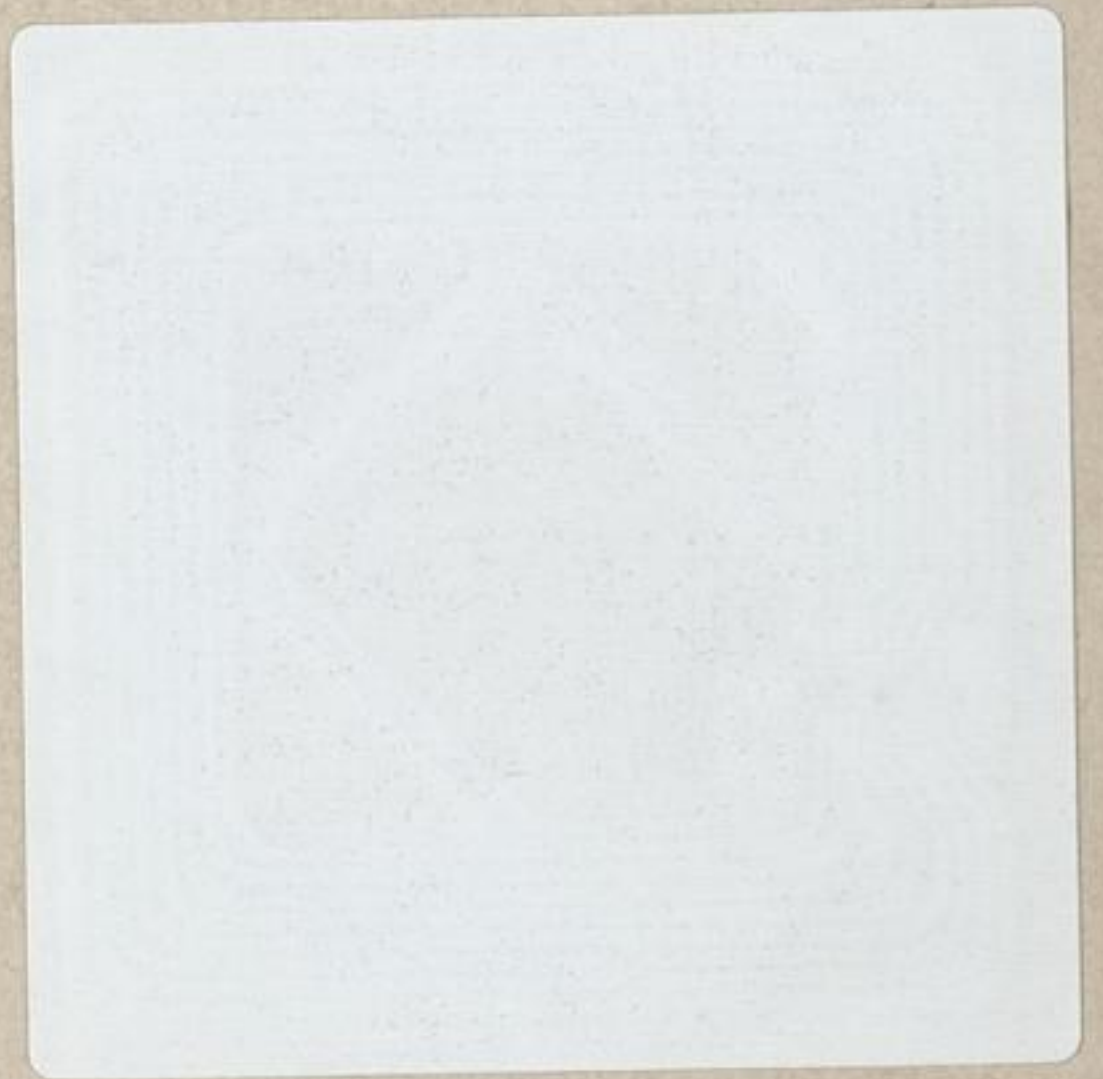
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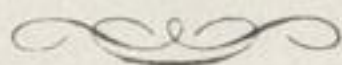
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AMERICAN GEOLOGY

LETTER

ON SOME POINTS OF THE GEOLOGY OF TEXAS
BY HENRY D. HARRIS AND FREDERICK A. HARRIS
MEMBER OF THE BERGAKADEMIE

BERGAKADEMIE
FREIBERG.

1881

VERLAG VON G. SCHNEIDER, FREIBERG

1881

Letter on some points of the Geology of Texas, New Mexico, Kansas, and Nebraska; adressed to Messrs. F. B. Meek and F. V. Hayden.

Zurich (Switzerland), October 20th 1858.

Dear Sirs:

In April last, I received the following letter:

Smithsonian Institution.
Washington, D. C. March 24th 1858.

Dear Sir,

Inclosed I send you a copy of the Geological results arrived at from an Exploration of the Black Hills of Nebraska. I am quite certain it will interest you much and you will do Mr. Meek as well as myself a great favor by securing its insertion in some journal in your country, or in France.

Believe me very truly your obedient servant

M. Jules Marcou, Prof.
Zurich (Switzerland).

F. V. HAYDEN.

It was accompanied by the following note from my friend Prof. Spencer F. Baird, assistant-secretary, Smithsonian Institution:

March 24th 1858.

My dear Sir,

I send a communication from Dr. Hayden which he wishes commended to your favorable consideration.

Yours truly

Prof. J. Marcou.

S. F. BAIRD.

I accordingly translated your short memoir and procured its publication in the *Bibliothèque Universelle de Genève*; as it appeared in the *National Intelligencer* and in *Silliman's Journal*, I will not reproduce it here.

Not having the honor of a personal acquaintance with you, I considered this communication as a proof that, after having opposed my opinions and observations on the Geology of the West, you had changed your views and found that my observations, or *hazardous guesses*, as they are called in *Sil-*

liman's Journal, were not so absurd after all, and I gave to the article published in the *Bibliothèque Universelle de Genève*, Juin 1858 (*Notes pour servir à une description géologique des Montagnes Rocheuses*) a direction showing that change in your opinions, believing candidly that such was the meaning of your message. From subsequent letters it appears this was not the case, and now it is my duty to give a few words of explanation, that no mistake may arise from what has occurred.

In your letter dated Washington May 30th 1858 you say: «I hope you will permit me respectfully to suggest, that I cannot agree with you in your conclusions respecting the Pyramid Mount section (see: *Geology of North America*, p. 18). By this I mean to say, that I regard all the beds of that section down at least to A. as *Cretaceous*. The beds below may be Jurassic or Triassic, though I know of no evidence that they are so. Notwithstanding the high authorities you have for regarding the *Gryphæa* and *Ostrea* found at Pyramid Mount as *G. dilatata* and *O. Marshii*, I feel quite confident from collections shown to me from the South West, that these fossils are at some places associated with well marked Cretaceous forms; while numerous facts point to the conclusion that the bed from which you obtained these fossils at Pyramid Mount, is equivalent to N^o 2 of our Nebraska section, in which we find *Baculites* and other Cretaceous fossils. In addition to this, all the facts in our possession respecting the rocks in Kansas and south to the Arkansas river, point directly to the conclusion that the sandstones of our N^o 1 in Nebraska, in which the leaves already mentioned occur, are represented by the beds B., C., and D. of your Pyramid Mount section; while the beds from which Dr. Hayden obtained the Jurassic fossils near the Black Hills hold a position *far beneath these.*»

I respect your opinion, gentlemen, but I beg you to allow me to continue to regard my observations and conclusions as *exact*. I would also say that I do not understand the value of the following phrase in a note to your memoir entitled: *Descriptions of New organic remains*, collected in Nebraska Territory

in the year 1857; by Dr. F. V. Hayden, Geologist to the Exploring Expedition under the command of Lieut. G. K. Warren, Top. Engineer U. S. Army; *together with some remarks on the Geology of the Black Hills and portions of the surrounding country*; pag. 47: «*By these remarks we do not wish — to withhold from Mr. Marcou the credit justly due him for having first suggested the existence of Jurassic rocks in this region*»; — for you know perfectly well that my conclusions as to the existence of the Jurassic rocks in the Black Hills and the Yellow Stone river region, were based upon my observations at Pyramid Mount, and if I made a *mistake* there, I cannot claim any credit for arriving at the *truth*, by taking for my starting point an *error* so enormous as you think I have committed in the geology of the Tukumcari Mounts.

In your memoir entitled: *Remarks on the Tertiary and Cretaceous formations of Nebraska, and the Parallelism of the latter with those of other portions of the United States and Territories*; I find page 18, the following note: «*We think these (Marcou's G. dilatata and O. Marshii) identical with species found by Dr. George G. Shumard at Fort Washita, Texas, where they appear to hold the same geological position, and are associated with Ammonites vespertinus Morton. Dr. B. F. Shumard has correctly, as we believe, referred the first to Gryphæa Pitcheri Morton, and described the latter as a new species, under the name of Ostrea subovata (see: Capt. Marcy's Report Explorations Red river, page 205. — Appendix E; Paleontology).*

Supposing that your rejection of my conclusions, with regard to the Jurassic rocks of Pyramid Mount, was entirely based on my determination of these fossils, and thinking it possible you might change your mind in reading the chapter on *Paleontology* in my *Geology of North America*, which I have since sent to you; in my answer to your letter of 28th May I begged you to tell me if you still continued to hold the same views on that subject. You were kind enough to reply in the following manner: «*We also still think the shell you refer*

«to *Gryphæa dilatata* var. *Tucumcarii*, is the true *G. Pitcheri*
 «Morton, and the oyster you refer to *O. Marshii*, we think
 «the same as *O. subovata* of Shumard. Dr. Geo. G. Shumard
 «says: *G. Pitcheri* (meaning the true *Pitcheri* of Morton) ranges
 «through the whole series of the Cretaceous formations of Texas
 «and New Mexico (see: *Trans. St. Louis Acad. Sc.*, vol. I, N^o 2,
 «p. 289): all the other explorers of that region I have seen
 «tell me the same. You will bear in mind Dr. Shumard in
 «speaking of *G. Pitcheri*, refers to the peculiar form you re-
 «gard as *G. dilatata*, which you may see is exactly the type
 «of the original *G. Pitcheri* figured by Morton. The shell you
 «and Ferdinand Roemer figured as *G. Pitcheri* is now regarded
 «by all in this country who have given much attention to the sub-
 «ject, as a distinct, and unnamed species.»

I will answer your objections as briefly as possible and try to be clear and precise in my meaning.

First. — I would say that, before coming to a final conclusion with regard to the determination of the *Gryphæa dilatata* var. *Tucumcarii* and the *Ostrea Marshii*, I consulted all the books on the subject: Morton's *Synopsis of the Cretaceous* etc., Roemer's *Die Kreidebildungen von Texas*, and Shumard's *Paleontology of the Red river of Louisiana*. The figure in Morton's *Synopsis*, pl. 15, fig. 9, is not good, and the text is very unsatisfactory, so that I do not think it is possible to determine exactly what Morton means by his *G. Pitcheri*, with only his book in hand. Roemer gives a very good description of the *G. Pitcheri*, p. 73 and 74, and his figures are tolerably good, especially fig. 1 c; Roemer says that he has seen at Philadelphia the original specimens of Morton, but that they were very imperfect and he found it difficult to identify them with his own specimens; notwithstanding which, he does so and says they are the same species, which he calls *G. Pitcheri*. In Shumard's *Paleontology of the Exploration of the Red river of Louisiana*, the descriptions of fossils are excellent, but the figures are miserable and cannot be of any use for comparison. In saying this, I wish it to be understood that I do not intend

to blame Mr. Shumard, for it is not his fault; I wish merely to state the fact that the drawings are useless. Besides the study of books, I have myself seen *very extensive collections* of Cretaceous fossils made at Fort Washita, Preston, New Braunfelds, Trinity river, etc., and I have also *seen* the Cretaceous rocks on the banks of the False Washita, and Canadian rivers, and in the valley of the Rio del Norte near Albuquerque and Galisteo; so perhaps you will admit that the following view of the question was not adopted without *due consideration* of the subject. — A. The *Gryphæa dilatata* var. *Tucumcarii* differs from all the fossils found at Fort Washita. — B. The *Ostrea Marshii* is different from the *Ostrea subovata* Shumard, and further, it has never been found, to my knowledge, at Fort Washita, nor in any other cretaceous locality of Texas.

Second. — I have not yet seen any fossil from Fort Washita that could be identified with those of Pyramid Mount, neither have I found any at Pyramid Mount identical with those of Fort Washita.

Third. — Roemer may have been mistaken, though I think not, in identifying the *G. Pitcheri* of Morton with that which he found at New Braunfelds, also called by him *G. Pitcheri*, but, however this may be, I am convinced that the *G. Pitcheri* of Morton is not the *Gryphæa dilatata* var. *Tucumcarii*, and then it will be a *third species* different from the two others.

Fourth. — The description of Shumard (see: Paleontology, Appendix E, p. 205, in *Explorations of the Red river of Louisiana*) is exactly that of my *G. Pitcheri* and entirely different from the description of the *G. dilatata* var. *Tucumcarii*. I saw the collection of Dr. Geo. Shumard at Fort Smith in June and July 1853 and *then* he had not a single specimen of the *Gryphæa dilatata* var. *Tucumcarii*.

Fifth. — I am certain from my own observations, that the *G. Pitcheri* of Morton *does not* « range through the whole series « of the Cretaceous formations of Texas and New Mexico », and Dr. Shumard must have confounded together three or four different species of *Gryphæa* under the single name of *G. Pit-*

cheri, in order to arrive at such a conclusion. If true, this would be *unique* in the Cretaceous rocks, where a fossil never has been found to range from the base to the summit of the formation *in the same country*.

Sixth. — Morton says in his *Synopsis* that the original specimens of his *G. Pitcheri* were picked up on the plains of the Kiamesha, and at the falls of Verdegris river, by Dr. Pitcher; so far as I know, no other Geologist has ever visited these two localities, and I think a fair way of settling for ever the question of the identity of the Pyramid Mount fossils with species found at Fort Washita, the plains of the Kiamesha, and Verdegris river, would be for a Geologist to visit these different localities and collect carefully all the fossils he can find. My original specimens figured in the *Bulletin de la Soc. Géol. de France* and in my *Geology of North America*, are deposited in the Museum of the Geological Society of London and can be always easily consulted. For my part, I hope such a Geologist will be found before long, and I am too much a friend to the truth not to submit at once to the result of such a comparison.

Seventh. — It is almost needless to say that I consider the Jurassic rocks of Pyramid Mount as of the same age with those of the Black Hills, and *not far beneath* them as you think.

Eighth. — In your letter of the 22th August 1858 you say: «You were certainly mistaken in supposing we had identified «the Trias in Nebraska; we are not however disposed to take «offence at the form of your note in the *Bibliothèque Uni-«verselle*, as our remarks in the *Intelligencer* were too brief to «be fully understood by you. Dr. Hayden merely sent them «to you because he thought it would be gratifying to you to «know we had found reliable paleontological evidence of the «existence of Jurassic and Permian rocks in Nebraska and «Kansas.» Perhaps I am mistaken; but I think in the incomplete section given by Hayden in the second edition of his memoir entitled: *Explanations of a second edition of a Geolo-*

gical Map of Nebraska and Kansas, I see evidence of the existence of a portion of the Trias in his Permian. The Red Buttes on the road between Fort Laramie and Rock Independence are certainly not Jurassic, though colored as such on Hayden's map, but of the Triassic age as I said in my first Geological Map of 1853. I do not doubt that the Trias exists on both sides of the great Missouri valley, at the Black Hills, and on the border of the Plateau du Coteau des Prairies, continuing by the head waters of Red river to Lake Superior. As for the *reliable paleontological evidence of the existence of the Jurassic* in Nebraska, I do not think it in *any degree more reliable than mine* for New Mexico, and when you urged so earnestly in 1856 and 57 that there were no such formations as Jurassic and New Red in Nebraska, I maintained with no less certainty that they existed there, notwithstanding your repeated denials. Besides it will be just as easy for the learned Paleontologist James Hall to prove that your Jurassic fossils from the Black Hills are identical with Cretaceous fossils of Fort Washita, as it has been for him to prove the supposed identity of my Jurassic fossils from Pyramid Mount with Fort Washita Cretaceous forms.

Ninth. — I have not indicated the Permian between Fort Smith and Albuquerque, as I found unquestionable evidence of its existence only near the Rio Colorado Chiquito; but I have always strongly suspected that the New Red Sandstone between Delaware Mount and Beavertown was of Permian age. Having found no fossils, and being the first geologist to enter those regions, I was not able when in the field to declare exactly the age of those strata. All that I knew then was, that after having left the Carboniferous limestone of Delaware Mount, I entered upon strata belonging to another and younger formation; and it was only after having passed Beavertown that I saw clearly I was upon the New Red Sandstone. Since the discovery of Permian in Kansas I am still more inclined to the belief that the strata between Delaware Mount and Beavertown are Permian. Thus you see I include the *Permian* in

the *New Red Sandstone* formation. I know that good reasons, based exclusively on paleontological grounds, have been advanced by geologists desirous to place the Permian in the Paleozoic; but I think the old classification a better one, and more, I think the term *Permian*, at least as given by Murchison for the strata of the government of Perm (see: *The Geology of Russia in Europe and the Ural mountains*, vol. I, p. 137 etc.), a very improper one. There are strong suspicions that Murchison has put in his Permian of Russia, a *part* if not the *whole* of the *Trias*; and I am almost certain that, if geologists accept the Russian Permian as Murchison has defined it as the type, the *Trias* will disappear from classification in Asia, Africa, America, and Australia.

Tenth. — Allow me respectfully to suggest to you that I cannot see anything of cretaceous in what you call N^o 1 of your Nebraska section. It appears to me that you put in your N^o 1, or *Lower Cretaceous*, all sorts of strata of different ages, except true cretaceous rocks.

I can assure you that the strata on my route near the 35th parallel of latitude, which, in your memoir entitled: *Remarks on the Tertiary and Cretaceous formations of Nebraska, and the parallelism of the latter with those of other portions of the United States and Territories*, p. 17 and 18, you consider as equivalent to your N^o 1 and N^o 2 of your Nebraska section, are not *Cretaceous*, but *Keuper* and *Jurassic*. Not having visited Kansas or Nebraska, I have no decided opinion with regard to the Geology of those countries, for I profess the doctrine that geologists must see with their own eyes in order to decide the difficult questions of the science; and all that I have said about it is *approximative* and based upon my knowledge of the geology near the 35th parallel. With this *reservation* I will submit to you the following remarks upon your *Lower Cretaceous* N^o 1. You admit in one of your letters that the rocks near the mouth of the Judith river on the Upper Missouri are not the type of your formation N^o 1, and you say it is highly probable Jurassic rocks will be found there, so I

will say nothing as to the Judith river strata; another survey, more careful than the first reconnaissance of Dr. Hayden, is evidently required in order to arrive at the true geological structure of the vicinity of Fort Benton. The strata which constitute the type of your *Cretaceous* N^o 1, are brown sandstones and various colored clays seen along the Missouri near the mouth of *Big Sioux river*. You saw there those strata holding a position immediately beneath N^o 2 of your *Cretaceous* section; and in them Dr. Hayden made a large collection of leaves, unquestionably dicotyledonous and having quite a modern aspect. Rough sketches of some of these leaves were sent by you to my friend Prof. Heer, who has examined them carefully with the following result. — « *Liriodendron Meekii* n. sp. « Heer; species very analogous to the *L. Procaccini* Unger. The « latter is a very characteristic fossil of the Miocene of Switzerland and Italy. — *Populus leuce* Rossmassler? The identity of « your leaf with this species is not certain, owing to the imper- « fections of the drawing, but at all events yours is very closely « allied to the *P. leuce*, a characteristic fossil of the *Ligurian* of « Bagnasco, Piedmont, or the *Lower Miocene* of Lyell, or the *Oli- « gocene* of Germany. — *Populus cyclophylla* n. sp. Heer. — *Laurus « primigenia* Unger? Your leaf is very probably identical with this « species, which is a very characteristic fossil of the Lower Mio- « cene of the Isle of Wight. — *Sapotacites Haydenii* n. sp. Heer. « The genus *Sapotacites* is Tertiary. — *Phyllites* n. sp. The genus « is special to the Lower Miocene of Bohemia. — *Leguminosites « Marcouanus* n. sp. Heer. This genus is allied with the genus « *Cæsalpina* still living now. — In conclusion Prof. Oswald Heer « says that he sees nothing of the *Cretaceous* age in these leaves, « and he thinks the flora indicates the *Lower Miocene*. » — If this conclusion is correct, the beds containing them near the mouth of *Big Sioux river*, are of the same age as the Miocene of *White river* or *Mauvaises Terres* and must belong to a sort of outlier of the great Tertiary basin of *Nebraska*. Of course in such a case these strata cannot hold a position beneath your *Cretaceous* rocks N^o 2, but that question must be elucidated with the greatest care in the locality itself.

I am much inclined to adopt the opinion of Prof. Heer, and I suspect that your *Lower Cretaceous* N^o 1 of the mouth of Judith river is partly *Jurassic* and partly *Miocene*; the *Baculites* found with the leaves being probably a cretaceous *Baculites* of the neighboring strata that has been washed away from the cliff during the deposition of the Tertiary rocks. To continue, I also regard your Black Hill *Lower Cretaceous* N^o 1 with dicotyledonous leaves as *Miocene* as well as the similar formation indicated somewhere in Kansas by Major Hawn. Further, after a careful examination, Prof. Heer thinks that the deposit in the vicinity of Fort Bent, from which Lieut. Abert obtained leaves, is also *Lower Miocene* and equivalent to your formation of the Big Sioux river; and that the Muddy river coal discovered by Col. Frémont is of *Tertiary* age instead of *Jurassic* as it was pronounced by James Hall. The fossil leaves figured by Prof. Bailey in Abert's Report (see: *Report of Lieut. J. W. Abert, of his examination of New Mexico, in the year 1846 — 47*) belong to two species, the lanceolate leaf is identical with your *Laurus primigenia* of the Big Sioux, and the large cordate leaf is a species of *Ficus*; a Tertiary genus still living. But I will remark that Abert says p. 523 of his Report, that he did not find those leaves himself at the Raton mountain; he examined the coal, but was unable to find a single impression of leaves, and the specimens figured were given to him at Fort Bent by hunters, so the exact place of the leaves is doubtful; they may come from other strata of the vicinity of Fort Bent. I say this because I suspect the bituminous coal from the Raton to be the equivalent of the coal I found at Ojo Pescado near Zuni which is of *Jurassic* age. James Hall says: «*Glossopteris Phillipsii?* Brong. I have referred this species to the *G. Phillipsii*, «as being the only description and figure accessible to me, to «which this fossil bears any near resemblance. The geological position of that fossil is so well ascertained to be the «schists of the upper part of the oolitic period, that, relying «upon the evidence offered by a single species, we might regard it as a *strong* argument for referring all the other spe-

with the greatest care in the locality itself.

«cimens to the same geological period.» (See: Colonel J. C. Frémont's *Exploring Expedition to the Rocky Mountains, and to Oregon and North California, in the years 1842—43—44*, p. 305; Washington, 1845.) Now Prof. Heer thinks the supposed *Glossopteris Phillipsii* is nothing else than your *Laurus primigenia* of Big Sioux river; that the leaf, pl. II, fig. 4, is probably a *Quercus*; and as for the Ferns, no one of Hall's figures is characteristic of the Jurassic period or any other special formation, as the Ferns range from the Devonian until now, and when new are no evidence as to the age of a deposit.

The coal that I found at Ojo Pescado near Zuni is Jurassic, from its stratigraphical position above and beneath rocks that I consider to be undoubtedly of that age. When there I looked sharply for impressions of leaves, but I found none, and I am not aware that any have been found since in that region of Zuni, Fort Défiance and Cañon de Chaca.

I was unable to find a single impression of leaves during the whole of my exploration in the Triassic of the Prairies, so they must be scarce. Only in that part of the Trias that I look upon as equivalent to the *Bunter Sandstein* I found a *Pinites* allied to the *Pinites Fleurotii* of Europe, numerous specimens of *Araucarites* called by Gœppert *Araucarites Möllhausianus* (see: *Tagebuch einer Reise vom Mississippi nach den Küsten der Südsee*; von B. Möllhausen; p. 492; Leipzig, 1858), and several shells of the genus *Cardinia*. In America, as in Europe, the Trias is generally very poor in fossil remains, and I think they will never be found in great abundance in the Far West.

Let me call your attention to the following remark. Is it not strange that Prof. Heer and Dr. Leidy have arrived at the same conclusion in regard to the age of the lower part of the Tertiary formation of Nebraska, both of them calling it *Lower Miocene*, basing their opinion, the one upon *Plants* and the other upon *Mammalia* and *Chelonians*, and Heer having no knowledge whatever of Leidy's publications!

Finally, I maintain *all the observations* contained in my *preliminary report* to Capt. Whipple as *exact*, notwithstanding

all the objections advanced against them. From your experience of the Indian Country, you will probably agree with me, that it is much easier to make objections from a comfortable room in a large town, than to observe in the wilderness of the Rocky Mountains; and you will permit me to suggest, that it would be better for the Science if my adversaries would go themselves on the field and follow my route near the 35th parallel, instead of making a show of their powers of argument in *Silliman's Journal*, or at the meetings of scientific associations. I have done, and I will say no more until my next exploration of the Rocky Mountains; for I hope soon to return to my home in Boston, and from there I will try again to reach some part of those beautiful mountains which you and I were the first to explore geologically. As an illustration of the difficulties I had to encounter when with Capt. Whipple's expedition, I will extract from my private note-book the following remarks. — «22th December 1853. — For the last ten
«days the cold has been very severe, the thermometer being
«at 7° Fahrenheit with frequent snow storms. We started at
«ten in the morning leaving the Rio Colorado Chiquito near the
«cascade, and directing our course due West, to the South of
«the San Francisco volcano. From Zuni to the cascade of the
«Rio Colorado Chiquito, our road passed over all the strata
«of the Triassic rocks, and our starting point of the 22d was
«on red sandstone, that I consider as the equivalent of the
«Bunter Sandstein. After having marched three miles, the red
«sandstone gave out, and was replaced by strata of Magne-
«sian Limestone cropping out from beneath the Bunter Sand-
«stein. Just at that moment a very severe snow storm came
«on, and I was obliged to turn my back to the wind and try,
«by scratching away the snow with my hammer, to collect some
«specimens and make a few observations on this new for-
«mation. The snow storm continued steadily until six o'clock
«in the evening, when we encamped on volcanic lava.» —
Thus in addition to the fear of Indians, for we were then in the middle of the Navajos country, which obliged us to keep

together and not to be more than half a mile in the rear of the party, I had to battle with a violent snow storm, and I think every one will admit the difficulty of making geological observations under such circumstances. Nevertheless I saw perfectly well that the stratigraphical position of those *Dolomitic* beds was beneath the *Bunter Sandstein*, in accordance of stratification with it; and the lithological character as well as the position showed me that it was the *Magnesian Limestone*, or *Permian* formation. Further, I found some very imperfect specimens of fossils differing in form from Carboniferous fossils, and I have no doubt that fossils are common in these strata from the fact that I found some under such disadvantageous circumstances. If the collections I made with Capt. Whipple had not been taken from me, perhaps I might have been able to determine some of them by carefully separating them from the enclosing rock. At all events, on *Stratigraphical* and *Lithological* grounds, I maintain that the *Magnesian Limestone* of the Cañon Diablo and on our road on the left side of that Cañon, are of *Permian* age.

We are far from agreeing, as you see, on many capital questions relating to the Geology of the Far West; but I join you heartily in the conclusion of your letter, when you say: «In conclusion I would state that we can differ in opinion «without entertaining hard feelings. We certainly desire to «arrive at truth, even if it should not accord with our published opinions.»

Very truly Yours

JULES MARCOU.

P. S. — In Dr. Hayden's paper, entitled: *Explanations of a Second Edition of a Geological Map of Nebraska and Kansas*, p. 8, he says: «In our remarks of the 2d of March, upon «the discovery of supposed Permian rocks in the West, both «Mr. Meek and myself wish to be understood as referring to «their existence in Kansas and Nebraska. Our object being «simply to announce our conclusions derived from the study «of fossils collected from these rocks in the West, we did «not refer to their *supposed* prior discovery in Pennsylvania and «on the Atlantic coast, nor were we able to *judge of the evi-* «*dence* of their existence in those localities, it being based, «for the most part, upon the remains of Vertebrata, which «*are out of our line of investigation.*» It appears to me to be very unphilosophical to ignore the discovery of Permian rocks by Professor Emmons in North Carolina, because it is based on remains of Vertebrata and the Vertebrata are out of your line of investigation. — What will you say if Geologists refuse to consider your observations on the Tertiary Basin of White and Niobrara rivers, because your evidence is based entirely on Vertebrata and Chelonians. Geology will never progress if each one of us reject the observations of others because they are *out of his line of investigation.*





