Mr. Bradley

My dear Sir,

I owe you an apology for not sooner acknowledging yours of the 25th Dec. It arrived at a time when I was in the midst of confusion, in packing up and preparing to come down here; and since my arrival, I have had so many other things crowding upon me, that I have not felt fully at leisure to write you until this time.

I thank you for your kind attentions in regard to the matters respecting which you wrote. Another friend to whom I had written, gave me the information I desired from Dr. Hinman, and as I afterwards found good and satisfactory characters distinguishing our Silicite from D. lineatum, I did not think a more direct comparison necessary, and consequently did not write you on the subject. I feel quite as much obliged to you, however, as if I had borrowed specimens for comparison.

There are a few things yet, that I would like very much to have you look up for me, if the necessary books are accessible in New Haven. First, if...
you can find Millers Natural History of the Crinoids 1823. Will you be so kind as to make for me a hasty tracing of his Phorosaurus genus? (Table 2).

Will you also see if you can find Cumberlands "Reliques Conservatoeae, from the primitive materials of our present globe" with descriptions of Crinoids 1826. It seems he proposed three new names such as "Ollacrinus" for instance. What I am anxious to know is whether he described these genera, or how he proposed to figure them - whether he describes the species upon which they were founded, or refers by name, to figures or descriptions of the species in any work, so that we can regard them as having founded genera. If so, will you please make me a hasty tracing of his type of "Ollacrinus"?

"Will you like wise please to look at Lyons' Cape-dags description of their genus "Olosthenes", Amer. Journ. Sci. (3), Vol XXVIII, p. 24 (we have not this vol. here) and let me know what he says about the arms of this type. We have specimens of his typical species here, but none showing the arms, which seem to have furnished the characters upon which the genus was founded.

Please remit your to Mr. Dana of New York, and believe me very truly yours,

T.B. Meek
Prof. Dana.

My dear Sir,

Enclosed I send you a little notice of the first bulletin of the Chicago Academy of Sciences, for the annual. Dr. Stimpson is anxious to have it noticed, and something said about the Academy that will please the Chicago people interested in the Academy. If you have not already written a more appropriate notice, I hope you can insert this. I will also in a few days, prepare and send on a short review of Stimpson's paper on the Hydrobiaceae, unless you prefer to notice it yourself.

I also send you one dollar and fifty cents for a number of the Journal I want very much. I mean the number containing Lyon's Haeckel's description of Ectenocyrinus. The description commences on page 247, vol. XXVIII, 1859. I do not know what the price of a single number of the Journal is now, but suppose it may not be over $1.50. If this is not enough, however, please let me know, and I will send the remainder.
I wish very much to obtain some information from two books that I hope are in the College library. These are: "Miller's Natural History of the Terrestrial, and Cumberlands Reliquiae Conservatae." If these books are acceptable in New Haven, and you can find any competent person to send me the information we want, we will pay a liberal compensation for the trouble.

What we want is a tracing (I do not mean a nice drawing) of Miller's "Rhodovinum venes," with a copy of his description of that species; also of the Cumberlands' type of his genus "Allostomia." I enclose his diagnosis of the genus.

Some three or four weeks since, I sent on by express to you, a small package of Devonian Corals from the distant Arctic regions, in order to get some suggestions from you or Prof. Terrell respecting the generic relations of a few of them. I hope you received them, and that you will do me the kindness to look over them at your leisure. As I stated in my letter, I do not expect you to undertake to identify the species, but merely send them with the kind of getting suggestions respecting some of the genera which I suppose would require but little trouble, as they have made polished sections of them.

I wrote Mr. Bradley about the information from Miller and Cumberlands, but have just received a note from his father that he has sailed for Panama. Has he gone on a collecting expedition for the College Museum?

Very truly yours, etc.

S. B. Meek.
Washington Dec. 17th, 1866.

Prof. Dana
My dear Sir,

Yours of the 30th of Nov. was forwarded to me from the Smithsonian Inst. to Springfield, and reached me there when I was packing up to start on to this place. As I was very much occupied at the time, I concluded to wait until my arrival here before writing you.

I have been here three or four days, and this morning received yours of the 8th instant. which although plainly directed to me at this place, had for some unaccountable reason gone to Springfield, whence it was forwarded on here by the P. M.

I have been at the Squampha mark fist, in New Jersey, and know them, that is, those furnishing the best green sand mark, to the Cretaceous. They are in the 3d green sand bed of Prof. Cook, and afford the best mark found in the state.

I thank you for writing to Mr. Billings in regard to the notice of our Paleontology.
papaw, the Anticorte fossils has not yet arrived here. If he has sent one, he probably will it to Springfields. I will with pleasure write a notice of it for you when I receive it, but I am afraid that it will not be in time for the January number of the Journal.

It will be some little time before I can have my books all unpacked, and everything arranged for work again here. I also took a very bad cold coming on, and it is now about leaving me with an attack of neuralgia headache, that makes me feel little inclined to do much just now. I hope however, to be all right again before many days, and will then begin to think seriously about that map. Please let me know what size you think it should be. I think you said it should extend west to the Rocky Mountains. I suppose you also want it to extend from the boundary—or rather from the northern boundary to the southern extremity of Florida.

I had a narrow escape as follows on.

The train ran off the track at a point about 60 miles west of Cumberland, and shattered and threw the cars about in a fearful manner. There were only three

or four other persons in the car in which I was seated at the time; and fortunately none of us were near the anterior end which was much injured. I believe I was the fastest forward, but escaped without injury if I had been a few feet in advance of my position, my dreaming over fossils would have been at an end forever. Although some of the cars were thrown entirely off the track, and turned over, it was dragged far from the track, not a single person was killed or seriously hurt. Of course I feel thankful to providence for any Wisdom, and yet I am wicked enough to think sometimes, that the world has no very great attractions for me, and will have

Very truly yours,

A. B. Mcew
Washington July 15th 1867

Prof Dana

My dear Sir,

I received your letter in regard to the Ill. Report, and have written to Mr. Worthen on the subject. I have no doubt but he will be able to provide you with any other copy of the report without expense to you. I had four extra copies but were sent three to Europe, and one to Prof Marsh, publisher. I wrote to the engraver to go on with the maps, and he writes that they have it in progress. I have not heard yet from Prof G wyb. I see Carpenter has published a note in the Annals and Mag. Nat. Hist. rather pitched into me about the shell structure of the S. capitatus forms. He says he has examined many species of S. capitatus and similar forms, and that he is positively sure they are not punctate, and that he thinks I have mistaken little black spots on the surface of the shell for punctures.
I have no doubt whatever, but he is right in regard to the shells he has examined, the only question with me is, whether they have not come exactly from another (formostate type) that he has not seen.

I have written to Worthing to send me the Irish specimen I examined for re-examination, I cannot believe I was mistaken in regard to it. I have re-examined specimens here of Halls & capax (or at any rate the same type originally examined by me, and believed to be the capax)

also the another type examined by me from the fine grained sandstone, and mentioned by me in my paper, also Prof. Swallowes, S. hemisphaerica, all distinctly distinguishable from S. capax, and find them all as first asserted by me, beyond the slightest shadow of a doubt clearly
formostate. I have likewise re-examined Halls & hemisphaerica, the type of our Syncretista, and find it as distinctly formostate as a formostate.

I confess that when I first saw Carpenter's note, I was afraid that possibly I might have been too hasty, though I could not imagine how I could have been mistaken. You cannot imagine what a relief it was to me, when I placed some of my original specimens under the Spencer Microscope, to see that I was safe beyond doubt. The only question now is, in regard to the Irish specimen. I cannot think I was mistaken in regard to it; though it is barely possible I may have been, as Worthing had but the one specimen (and a very fine one it is too) so that I did not like to cut into it much. Still what I saw left no doubt on my mind, at that time, that I saw something in it. The Microscope I used then, is a very poor one, and that was what made me trouble when I saw the formostate. Since publishing my paper, I have also ascertained that Halls's formostate (almost exact image of S. capax), is a formostate shell.

I firmly believe now, that all our American species of the type of S. capax are formostate. I have however, examined a number of the other types of Syncretica, without seeing traces of formostate. I have written to Carpenter, to remove
from his mind the impression under which he seemed to be when he wrote his note that I was trying to cast doubt upon the accuracy of his conclusions. He had not seen my paper, but only a notice of it in the Annals and Mag., or he would have seen that I did not question the accuracy of his observations, but merely supposed that there might be a rare, intermediate type in the British rocks, that he had not seen.

I will send him some specimens for examination as soon as I receive a package from Worthing, and in the meantime I do not wish to publish anything further on the subject. As soon as he has examined the types I investigate, I think I will make some drawings and prepare an illustrated paper on the subject.

P.S. Tell Prof. Everil Krause that I have just learned that he had described our Illinois genus Sphenotterium a few months in advance of what we did, as a true coral.

J. B. M.

Very truly yours,

J. B. Medie
Washington March 16th 1867.

Prof. Dana:

My dear Sir,

I was very sorry to hear through Prof. March of your accident, and hope you have recovered from the wound by this time. I have not yet received from the engraver the skeleton map, and consequently have not done anything on it yet. I suppose, however, that it be ready soon. I wrote Prof. Gayet that I would consider his proposition and possibly we might alter a time, color his large map, if some arrangement can be made satisfactory to the publishers.

As I wrote you, I have reexamined the skull structure of the Brachio pods mentioned in my paper, and find all the American species mentioned by me, and two others not then examined (Sp. Hannibalensis and S. Probingnaeus Bell) and find them all clearly protozoal. On re-examining the Irish specimen mentioned by me, I have about satisfied myself that I was deceived in regard to its being
Illinois species) in the position of the eyes, and the
form of the area enclosed by the ocular ridge in B. 
(at the type of his new genus *Pestrichinia*) really exist, and
are not as we had supposed, due to the accidental
coinciding in of the central region of the cephalothorax.

Although our Illinois species agrees with his *Pestrichinia*
in having its body segments anguilliform as in *Limulus*
and hence differing in an important character from the
typical forms of *Belinus*, the differences it presents
from its type included in the position of the eyes, and the ocular ridge, are
I am strongly inclined to believe, of generic importance,
and hence make it typical of a third genus, intermediate
between *Belinus* and *Pestrichinia*. As I would be glad
to have your opinion on this point, I give here sketches of the
three types as to save you the trouble of hunting up
the books. From these you will see the ill. species is
intermediate in its characters, since it differs from
*Belinus* proper, in having its body segments anguilliform
and from *Pestrichinia* in the position of the eyes
and other characters mentioned. The figure given is a

geology represents the characters of the typical Pectastachia.

I will be obliged to you if you will look at the sketches I send, and if you desire, at the figures given by Woodward, and let me know what you think of the Illinois species being typical of a third genus. If you agree with me on that point, I will propose a name for it in the next journal. I see Woodward places it with doubt, from our description, in Belinas, proper, and I think it probable he will make a genus for it if we do not.

Hayden is here making a bold push for the Nebraska survey, and I have no doubt but he will get the appointment. Some of my friends urged me to apply, but Hayden was very anxious to get it, and I did not wish to enter upon a scramble with an old friend and refused to let my name be used. He has been sketching out a plan for the survey, at the request of the Commissioner of the Land Office, and has put in a clause for a Palaeontologist, and offers the place to me. I will accept it on conditions that the collections shall be sent here, and the results of my work shall be published in my own individual name. I am tired of partnership Palaeontology, in which I have to do all the work and others receive the share of the credit, or at least of the substantial benefits. Very truly yours.

F. B. Meek.
Prof. Dana

My dear Sir

Since writing you the other day that I sent to Worthington [name?] my late examinations of the Esiprie[sic] specimen had shaken my confidence in the opinion I had previously in regard to its structure (it is not in a good condition for examination). Worthington writes me that Davidson had written him for chippings of it sometime before I did for my late examinations, and that he had sent them. A few days back Worthington received another letter from Davidson, saying that he had sent the chippings to Carpenter, and that he found them to be distinctly psammodite! I suppose this will satisfy Mr. Davidson, and ought to make him a bit ashamed too, for his supercilious note in regard to my examinations, especially as this very identical specimen was sent by him to Worthington with the name S. Esiprie[sic] attached. As Carpenter now gives it up that it is psammodite, and will of course have to do so in regard to the others, in which the psammites are infinitely more distinct, I suppose they will begin to think over there that there are people here in the backwoods of America that can see as far into a Mill-stone as John Bull.
I do not doubt, however, but Carpenter is right in maintaining that the shells examined by him were without formating, and this very fact will establish my first suggestion, that they have two types, one, that they have confounded under the one name S. conduplicatus. So Mr. Davidson’s S. conduplicatus from Ireland, sent to Worthington, is not only not that species, but belongs to a different group, as distinct from Spirifer conduplicatus as Spirifer venosus is from Spirifer profusus. It will be interesting now to see how Davidson and Carpenter will treat the matter.

It is a clear case that Carpenter was either wrong in all his former examinations (which I do not believe to be the case), or, that Davidson has made a tremendous blunder in his identifications. That, however, they may still be as they please between themselves, but they cannot blind the world to the fact that my investigations have set them on the proper track. I now only want to determine that Windell’s type of Spirifer venosus is formate (and I will soon have an opportunity to examine for myself, as Windell has promised to send me his specimens), to establish not merely all my actual statements of fact, for these may now be regarded as granted, but also all my suggestions of probabilities. It will rather bring Davidson down a peg from his high horse, to have to acknowledge that I have detected one of his blunders in which he had confounded two generic, or at least subgeneric, types under one species.

I have just received a copy of Geinitz’s Carboniferous Fauna and Dogos of Kansas. In looking over it, I must confess that I was never in all my life more astonished at anything than to see how completely he has been fooled by his authority. The whole work is full of the grossest blunders that ever were made by a man of his reputation. I also had a very poor opinion of Geinitz as a palaeontologist, but many of the errors in this work go far beyond any I had supposed him capable of, that if it came from any young man without an established reputation, I should think it beneath criticism. Nearly all his identifications with foreign species are absolutely wrong, and many of the species, as well as those of our own, are in the wrong generic references, are monstrous and glaring.
now time just now to enter into particulars, but I feel tempted to mention a few. If you will turn to p. III, fig. 1, you will see at once, that what he calls Rhynechonella angulata Lam. is the Spirifer hunicplicata, Hall, the type of our genus Spiriferidae of which one of our most common coal-marguerites speaks. The inhabitants of the Ill. Reports now in the first place R. angulata is a Mountain Limestone species, although he pretends (that is, the Nebraska of here) to find it in the Dyes. But this is not all; it is not a Rhynechonella at all, as Gruitz's own figures would at once show to any conchologist, for his fig. 3 shows clearly a well defined cardinal area in the umbonal valve, while his fig. 2 shows as distinctly the cardinal process of Ortho.

The fact is, however, that its closely approximated dental laminae, and mesial suture of the other valve, clearly establish the fact that it is not Ortho, as its distinctly metamorphic shell shows it is not a true Spirifer, nor a Rhynechonella and that it does not even belong to the Rhynechonellidae.

Then just think of a man referring any of these fossils to the recent genus Turbonilla, founded upon minute living shells with a revised apertire, or his referring the species of Melhurnya, Genus Livingstonea, of the Coal Measures, to Actinonics, and to the Dyes. In a word, I can safely say that I know over forty of the 90 species enumerated by him to be common to the Coal Measures and his Dyes. I intend after while to write searching oases of his work. Very truly yours,

J. B. Neilson
Washington March 22d 1867

Prof. Marsh,

Dear Sir,

I do not know whether or not I ever thanked you for the specimens of Orophyllus and Bellhuschina, you were so kind as to send me. If not I do so now. If you have in your collection several European fossils I will mention, and will be so kind as to loan me specimens of them I will be greatly obliged to you. I intend to review Grunty and Moreon, and I will greatly need some of these foreign fossils for comparison.

In the first place I would like to see a good speci

men of Aneal. I mean the real thing, not the genus was illustrated by Keyserling in his Oescherland, and not those Permian species of Anealina referred to it by Grunty, like P. squamosa. You will remember the type of the Jurassic genus are very inquivalae, and have a singular dumpyly projecting little ear connected with a byssal notch, just in front of the breaks, while they are without the furrowed cardinal area of Anealina. It is very remarkable that Grunty shou
have mistaken the cast of little depressions often seen on a section in the beaks of Mytilus for the little ears of Nassa. Rosinelli mentions this shelf and the depression in it, in his description of the genus, and you may see the casts of this depression in Kings fig. 5, pl. xiv. Permian fossils, between the beaks.

To satisfy yourself that these are really not ears, you have but to look at figures of the testiferous specimen such as fig. of the same plate.

I would also like to borrow good specimens of Siphonaria (Spirifer) planorbites, Minton, Eildophorh C. Pallasi, (not German, but authentic Persian species) Spirifer laminsor, by thecascos: carnoos, the so-called Asculina primaeformis, and indeed any of the foreign specimens to which Trinity refers the western fossils. I have here, however, German specimen of Brotulin borderis, the so-called Monotis specimen, with a few others.

The more I look at Trinity's work, the more I am astonished that a man could ever acquire the reputation of discrimination he has, without professing better powers of discrimination. I send you a specimen of Phoebus with it. Then look at his reform of Spirifer rudistratus, Hall, the type of our Synth.

work in his specific identifications. If he were a man of Marsden's stamp, I would want no better fun than to ridicule his blunders; but in consideration of his age, and standing as a scientific man, I must review him at least courteously. I shall speak plainly, however, and decidedly. If you want to see a few specimens of his discrimination, just compare the shell on Plate i, fig. 21, he refers to allocheta elegans, with Kings figure and description of that species. On that on pl. ii, fig. 3, he refers to Eildophorh, Pallas, with Mr. T.K.'s figure of that species in the Geology of this. The shell grinty figures is a true Phoebus, as por...

my sec by the impression in the cast of the long posterior lateral tooth, while the figure Mythina Pallasi, of Mr. K. shows that shell be lee entitled, sedentarius, as they also say it is in the description. It also differs, as their fig. 3 shows, in having the ridge bounding the ant. Muscular area directed obliquely backwards at an angle of 45° to the axis of the shell, instead of at right angles to the same. It is evidently a distinct genus, though Trinity confounds Germanization he has, without professing better powers of discrimination.
But the most astonishing thing is, that he should have supposed it a Physochonekla at all, especially when his artist has correctly shown it to possess a distinct cardinal area, divided by a wide triangular figure, in the ventral valve, and a distinct cardinal postceral (mesial tooth) like that of Oyster in the dorsal valve. No Physochonekla ever possesses these chinks while its punctate shell, and dental laminae and muscular septum of the ventral valve at once remove it from Physochonekla, as well as from the Physochonekla! Then just compare his figures of what he calls Spinifex lamarina, Mc Coy, pl. III, fig. 19, with Davison's excellent figures and descriptions of that species. The fact is the shell figured by Davison is very similar to I. Kentuckiensis of Shimer, which is a true Spinifexina, as its transition shell will I think he found to be. His Avicula[!] Spinifexina pl. III, fig. 13, is actually, figure is a new sp. of my genus Avicula spinifexina. What he calls Pectin alevi, C.ramosissi, Shimer is not that shell at all. The C.ramosissi, Shimer, came from the Saint Louis Limestone (Sil., Carb.), but an Avicula pectin discribed by Shimer, under the name P. accedente. He ignores the genus Barbarella entirely, when you know it differs not only in having anterior and posterior lateral teeth from Gasteroponds, but in usually having so large an anterior muscular scar that King removed it from the Monomyaria. But I cannot begin to touch upon his errors. I will prepare a brief review for the next journal, merely to secure the credit of correcting the worst of his errors and to name several new species, and after awhile I will take it up again in more detail. Very truly yours,

A.B. Meck.
Washington March 25th 1867

Prof Marsh

My dear Sir

I thank you for sending the specimens of Eustatius. I have been examining them, but they are in a bad condition for such an examination, being very opaque and brittle, so as to show the structure imperfectly. I have been able to see what I am inclined to think may be structures in the, but owing to the fact that the pieces I have examined all crack to pieces, and give way before I could proceed in grinding them to a sufficient degree of thinness, leaves me in doubt.

Enclosed I send a note proposing a new genus for our Illinois Bellimnus, which I would like to have inserted in the Journal. The confusion that has hitherto existed in regard to the limits of the genus Bellinmus being now all cleared up by Woodward’s late paper, leaves no doubt in my mind in regard to our fossil being the type of a new genus, in which opinion Prof. Dana and Mr. Belling agree with me.

If I could be permitted to distribute this note as soon as it is in type, either as a separate note, or as extracted from the Journal, with the date of distribution attached, I would like very much to do so, as I am afraid that Woodward, Hall or some one else, will name it, as soon as they see the figure in the Ill. Report, and have published by Woodward.
Could you ask Prof. Dana to loan me Pterichora loud and Simitz’s Dyas? I mean the part of the latter containing the figures and descriptions of the animal remains. I think Simitz ought to send this to me, as I sent him a copy of the Illinois report. I suppose he will do so, but I need it very much before I could expect to receive it from him.

I do not know whether or not I mentioned to you or Prof. Dana in any of my letters, that I recently had a long letter from Prof. Agassiz, offering me an appointment in the Cambridge Museum. He urged me to come on and see the collection at any rate, and that he would pay all my expenses.

I wrote him a very polite note, thanking him for his kind offer, but telling him I would not have time to do what he wants done in the Museum— that I had already accepted the palaeontology of the Nebraska and Colorado surveys ordered by the Government, and that I will also have to prepare the palaeontology for another Vol. of the Mo. survey, for which an appropriation has already been made; also that Whitney wants me to work up some Silurian fossils from Missouri now on the way, and that while wants me to do something with any Silurian fossils he may find in Western Iowa. Consequently, my whole time and attention, would have to be devoted to this western work.

Very truly yours,
C. B. Meek
Washington July 29th 1867

Prof. Dana

My dear sir,

I have merely time to write you a hasty note, in order to inform you of my return to this place again. My health is greatly improved so that I am able to work and walk, and look as if I have a pretty good supply of red blood in my veins again. I was led on from place to place, and delayed at Chicago, so returning, so as to prolong my absence much beyond the time I had expected to return. We examined the geology of the counties through from Iowa City, Iowa, to Nebraska City; then from Omaha, down to the souther boundary of Nebraska — also along the main Pacific R. R. out to its end (near 400 miles W.). I likewise spent nearly a week with an old friend near Atchison, Kansas examining that region, and went on with him sixty miles to the end of another branch of the Pacific R. R.
I see you could not get my review of Geikie in the Deucesnumber on account I suppose of the long delay in the proof. I do not regret the delay however, as I can now speak much more decidedly in regard to many of his errors, and some others I had not previously observed. I think I can now make much more errors in geology so evident that all others must see how the matter stands; and as for Grimes, I believe I could make him acknowledge his, if he had critical knowledge enough of palaeontology to appreciate the force of the evidence.

I suppose you received the corrected proof I sent back to you from Nebraska City, though too late to go into the Journal. Will you please send it to me now, so that I can look over it again, in order to make some additional corrections? I hope it can all go into the Journal with the closing remarks. I believe I can make it a "clincher" now.

On my return here I found so many pamphlets, pamphlets, letters awaiting me that I feel as if I had been about a year or more
Amongst other letters, there is a very kind one from Mr. Davidson, to whom I had never written a line. He says Carpenter and himself have been investigating the Syringothyris question with great care, and that they have spared neither pains, trouble nor expense, and that they are highly gratified to find all of my conclusions confirm the says they have both prepared papers on the subject — Carpenter will appear in the July No. Ann. and Mag. No. 41, and Hillson in the Geol. Journ., and that they are highly complimentary to me. He thanks me all kindly for my investigations and hopes I may continue them — that there is much yet to be done before we can understand the Brachiopoda fully. They even obtain specimens from Melincourt Island, at the same locality from which the first examined by was obtained, and found them to be identical. So I think we can now doubt, in regard to the fact that they were all confounded under the one specific name, Spriifer cuspidatus, widely distinct British types.

There are also two letters here from Dr. [illegible] about the Map. There is not written them for some time because I supposed they had been corresponding with you and that you had informed them of my movements. They wish to know whether or not it would be well to announce the map as being in print. Do you think it would be a good plan? I write without much thought.
Washington July 30th 1867

Prof. Dana

My dear Sir,

I have just within the last 24 hours, used the proof of my paper on the cidrotic fossils collected by Kennicott, in which I have figured and described a number of Brachio poda. Before retyping it, I am very anxious to see Davidson's monograph of the British Silurian Brachiopoda. The Smithsonian Inst. had ordered it, and it was here just before I started West, but it was, with many other books, sent to the binder, before I had an opportunity to do more than give it a hasty glance. Since my return, I have with D. Gill spent a day or so looking for it, without finding any clue to it, either at the capital or here.

If you or Prof. Marsh have it and Davidson's Monogr. of the Brit. Silurian Brachiopoda, as far as published, will you be so kind as to loan them to me for a short time?

If you can, I will return them with the other books you were so kind as to loan me.
If you can do this soon, please send by express, directed on the inner wrapper to J. B. Meck, and on the outer simply to the Smithsonian Institution, Washington, D.C.

Very truly yours, [no name]

J. B. Meck
Washington Aug. 18th 1867

Prof. Dana

My dear sir,

So many things have claimed my immediate attention since my return from the west, that I have only not come to a point where I can again look that map in the face; and I confess that the more I think of it, the more extremely do I regret that I consented to undertake it. When I look over it, and see how many points of doubt and uncertainty will have to be met and decided one way or the other; and how much thought and study will have to be bestowed upon it, without the poor satisfaction of feeling any degree of confidence that I am right in the end; while I will be constantly annoyed with the fear of falling into so many bad blunders, that I have almost gone into a fit of the blues, even before I have commenced it.

I have therefore concluded to ask you again, whether you would not be willing to let Prof. Marsh undertake it, if he will. I am sure he can do it better than I can. It will of course have to be almost entirely a work of compilation, and he is better posted on general geology than I am. If you are willing, and he will undertake it, I will with pleasure, give him any
suggestions, I can, and I am sure he can have it ready
sooner than I can. If he could not undertake it, perhaps
Mr. Billings, or Prof. Winchell could.

I think I would be willing to pay for the expenses
already incurred in engraving the title-page, if it would not be
more than one hundred or one hundred and fifty dollars, and
mob the whole thing out if no one else would go on with it,
with whom you would be satisfied, and feel as if a mountain
had been lifted from my shoulders.

You know I am constitutionally slow at work of
any kind, and I now have a vast collection of fossils on
my hands (coming and already arrived) from Nebraska,
which I have to make some kind of report upon between this
and winter, and Martin will hold me to my engagement
to fix up the palaeontology of his third volume by the meeting
of the legislature - a no very easy small job in itself.

Do not understand me as meaning to be obstinate
about it, for if no other arrangement is satisfactory to you as
the publisher can be made of course I will go on and do
the best I can with it. But I am sure I would feel greatly
relieved if it could be taken off my hands, even at an
expense of one hundred dollars or so to me. I do not
think I could color it all over in less than three months.
and then I should feel exceedingly nervous about seeing it issued, knowing in how many points future explorations undoubtedly show me to have erred.

I know I should have thought of these things before (as I did indeed), but the thing has been delayed along, and it seems to me that the more I think about it, the more I can't bear the idea of doing it.

Please let me hear from you as soon as you can, whether or not there is any chance for it to be undertaken by Prof. Marsh. Of course I have had no communications with him on the subject.

Very truly yours,

J. B. Beck.
Washington Nov. 14th 1867

Prof. Dana

My dear Sir,

I am anxious to know whether or not Prof. Marsh is going on with that work, for I fear very much that you are thinking hard of me about it. I very much hope he is doing so. My sickness last summer, and recently, has placed me entirely out of my power to do anything with it, without causing a complete failure of all my plans in regard to the Illinois and Nebraska Palæontology.

I have for more than three weeks past been suffering with intermittent fever, complicated with a severe attack of bronchitis. By taking large doses of quinine, I have nearly recovered from the fever, but the bronchitis remains, though I am gradually recovering from it, and hope to be soon entirely relief of it. I can avoid taking cold. Curiously enough I have entirely recovered from my sickness of last summer, but it seems I only exchange one trouble for another. Although not enjoying an hour without...
exemption from some kind of sickness, for a long time, I have been gradually working along not without a task, with Hayden’s Latty, Nebraska projects. I have now about completed the drawings for some 5 or 6 good plates, including nearly all the species from the Latty, and Matlins, Drys and Mountain limestone. I have also made out a few of the figures of the species for which Latty erroneously refers so many of them to go on the same plates for comparison.

The descriptions, however, are all to be prepared to tell him I hope he will go on with the map. If not, and as Hayden has to present his report about the first of May, and I have to go to Illinois to prepare the paleontology for Mott’s 3rd Vol. by the first of next month, I am afraid I will fail to accomplish he sets, as I have not the time to finish Hayden’s work before starting to Illinois, and I fear I will not get through these in time to come back so as to finish Hayden’s work in time for Mott’s name for it at that time. Prof M., however, should look carefully through Trott’s little reports, and ten boys more, of specimens coming, and in fact scuttling papers, to see whether he may not have afterwards named it.

Mr. Davidson writes me that he has found the internal tube of Syringothyris in a number of specimens...
of what he believes to be the true Sp. eumyophila and that he thinks they will lie found to be wrong. Though De Carpenter had not at the time he wrote examined their structure. He also says King is "pitching into De Carpenter" and claiming that he can show that he is mistaken. They can fight this matter out to their hearts content, and all of my statements will still stand—that is, that all the shells examined by me are pomatolites and so far as their interior is known, belong to the character upon which Winchell proposed the genus Syringothyrus. All I have said in regard to the possibility of these shells being geographically or subgeographically distinct from the true Sp. eumyophila was of course based upon De Carpenter's negative and positive assertions that that shell is not pomatolite; and his recent statement that it is also without the internal tufts of Syringothyrus. It would be curious if he is mistaken on any of these points.

Very truly yours,

F. B. Meek
Washington Nov. 20th 1867

Prof. Marsh,

My dear Sir,

I am glad to learn you are willing to consider the map matter, and I hope you will undertake it. I would regard it as a special favor.

Unless my health improves more than seems probable, I will not be able to start west this winter. I am still in hopes, however, that I may be able to start by the middle of next month. I will be glad to see you if you conclude to come, and will give you any suggestions I can. I am afraid however that you will be disappointed as to the amount of assistance I can give, as I had not made any commencement with the map. The engraver had a skeleton of the map—that is, so far as the geographical features are concerned—in my hand all last winter, and I was waiting for it as a foundation to work upon. He at
Last sent a proof of it towards Spring, but he had gone on, contrary to instructions, and covered it over with large names of the states and smaller names of all the towns and villages. After he had ruled these out and made the corrections, the time had arrived for my journey to the West, which was a necessity on account of my health. When I returned in July (I believe it was) I found a proof of the skeleton map here, but it required fresh corrections, and I sent it back, and feeling quite well then, wrote Mr. Blije that I would soon be ready to go on with it. By the time it was ready, I was again under the weather in health, and felt as if I had no intellect or mental force left to do anything with it. Consequently wrote Prof. Dana, urging him to induce you to go on with it, and I have not, I believe, since written to the publisher, supposing Prof. Dana was communicating with him, and had let him know the condition of my health. To say how ever, I received from him the enclosed note, from which it is evident he is still under the impression that I am going on with it. None of his letters to me would be of any use to you as suggestions, as he had no plan for the map, but was leaving that entirely to Prof. Dana and me. If you would like to see them, however, I will hunt them up by the time you come, or send them to you. I will also, in a few days, send you some ten or twelve copies of the skeleton map. It does not come up to my taste as a specimen of map work, but may answer the purpose, after a few corrections.

I have no doubt but the Publisher will be willing to allow you a reasonable time. Its preparation will, of course, be work compilation, and I have no doubt but you will be able to prepare it better than I could even if well and with nothing else on my hands. The first step will be to lay down the principal divisions -- Devonian, Carboniferous, Silurian, &c. Then to work in the subdivisions.
in accordance with Prof. Dana's certificate, so far as our present knowledge of the biology of the country will permit.

No announcement of the map has been published so far as I know.

I have seen Woodward's new edition. It is but very slightly improved, and for being the present state of Cenozoology. I have been occasionally adding new facts to my copy of it. As to the plan of publishing a new edition—or more properly, a remodeling of the whole thing, with the large amount of additional matter I have collected from various sources, I can only say "man proposes, and God disposes."

I am glad to learn Prof. Dana is in good working order, and I hope he may long remain so. My fever returned day before yesterday, and yesterday evening. Today I took quinine, and only had a slight neuralgic pain over my left eye at the usual time for the fever. These returns of the fever make my cough worse than before.
Philadelphia, Nov 21, 1867

Dear Sir,

If able to give any clear idea of the purpose to issue of the Topographical Map we hope you will do so—

Mr. Sinclair calls quite often. They desire of concluding the work very kindly.

Yours truly,

Theodore Bliss & Co.

Mr. T.R. Buck
Washington D.C., Dec. 15th, 1867

Prof. Marsh,

My dear sir,

Some little time back, I sent on to Prof. Dana a note on the shell structure of Aviculopecten; for the January number of the Journal. Since then I have been looking at other shells in the same way, and now find myself puzzled. All of these shells are mainly cast, with a thin film of what I think must be the outer layer of the shell (only) remaining. It is always too fragile to be polished or ground, but so thin that a little light placed on a slip of glass, in water, or balsam, shows a distinctly prismatic structure. This... Seeing this, I was steady in these thin shells from several species of Aviculopecten, led me to the conclusion that these shells have an outer layer of prismatic structure, and hence must belong to the Aviculoidea, and not as I had previously thought, to the Pectinidae.

Further examinations, as stated above, puzzle me a little. For instance, in examining a thin piece taken in the same way from Lima retifera of Shumard, I found in...
the same bed at Nebraska City, I see in it the same structure, as clearly as in any of the Ariculodectens!

Carpenter says the shell of the genus Lima has an outer laminated layer, and then inner large layer perforated by minute tubuli, forming a complete network. This looks like a ski work on our shell, but has no appearance of being a result of tubuli, but of a prismatic structure, and besides I cannot think it is the inner layer, as it shows the fine lines of growth, and the prismatic structure seems to make up the entire layer.

The question that now puzzles me is, whether this is really a layer of the shell itself, or whether it may be the shell of Nebraska, Nebraska; while in the other species of Ariculodecten from the same class, as well as in the Lima, the prismatic structure is much finer, so as to be only well seen under a 40 inch lens. Other specimens of this same Ariculodecten from central Iowa, broken from hard calcareous matter are thicker and composed as I think, of both outer and inner layer. On placing one of these under a 40 inch power, with a strong transmitted light, through the whole shell, a prismatic structure of exactly the same composition is clearly seen, but the prisms here do not present the same trigonal appearance, which leads me to think it not real, but only a deceptive appearance in looking on the inside—see p. 10.)
the other, produced in some way by the refraction of light. I also send you one of these down specimens alluded to marked (3), which you can easily examine by taking the light under a 1/4-inch lens. I am sure it is the same species as the other in clay, from a comparison of good specimens from both localities, though this would not be satisfactory from the specimen alone, that I send.

In regard to the Lima, the question may suggest itself whether or not it is a Lima, as we have not seen its bivalve. It certainly has, however, exactly the form and other external characters of that genus, as you will see by the specimen I send marked No. 4. This specimen retains the thin layer of shell, as I understand it. By lifting a bit of this with the point of a knife and placing it in water under a 1/4-inch lens, with a good transmitted light, you can see the structure to which I allude. What do you think of it? Is it really shell structure, and is this really a part of the shell itself, or is it only something replacing it, with a minutely crystalline structure? If the latter, here and constant in the Trigonopecten (No. 2), it is hard to account for the difference in the size of the prisms or crystals in the two

Send a bit in balsam on a slip of glass marked (1 a), I think it can be better seen in water.
If my note on this matter is not yet in type, I will be obliged to you if you will send it to me, as I would prefer to modify it somewhat, so as to tell the whole story, doubt, and all. If it is in type, however, I can modify it in the proof without much change. I merely wish to speak with some more reserve.

Give my regards to Professor and tell him my health is improving, though I still have some remaining irritation about my bronchial tubes, and some remaining cough. Sometimes I nearly get rid of this, but the slightest cold causes a relapse, so that I have to confine myself mainly to my room. The Dr. says my lungs are sound, but that I may not entirely recover from the effects of my severe attack of acute Bronchitis, for some time.

The great trouble with me now is, that Worthing is getting out of patience about the Illinois Palaeontology and yet refuses to send the specimen or have me come out there to do the work—but of letting the 3d vol. go to press without the Palaeo...
getting Whitfield to prepare it, if I will not come
there. I think I will be well enough to go by
the end of the month, though I rather dread the
journey, as I nearly always take cold in
travelling by rail road in winter, even when in
my usual health.

I hope you are going on with the map.
Have you concluded to do so?

Although not well all the time, I have been
working along slowly with Haydn's late Nebras-
ka collections, and have nearly completed figures
and descriptions of all the species from the Upper
coal-measures of that region. Not many are new,
but many of them have not before been figured,
and as there is a dispute about the age of these rocks
I have thought it desirable to give scientific men every
means of judging for themselves. I also give the figures
for comparison, of the various European forms to which
taxonomy requires so many of them. They generally look from
side by side. In the late collections, I have found several
additional Carib. Sp. in the so-called syzyg, since writing my view.
You can examine the specimens I send, accepting
the little single valve washed (3) from Iowa. It happens
to be the only one of that species, showing the entire thick
rings of the shell, and the prismatic structure also.

Excuse me for troubling you about this matter. I should
not have done so, but for the fact that I supposed you would
take an interest in the investigation, and be able to give me some
suggestions.

Yours truly yours,

[Signature]

D. B. Mac
S. S. After writing the foregoing, I noticed that the thicker specimen of the little Atriculopereon from Iowa, only shows the prismatic structure as examined by transmitted light when looked upon from the inside. This led me to break it across to see if the fractured edge would show anything, when I found it composed of two layers, the inner one prismatic, and the outer (which gives the greater thickness to the shell) not. I am now nearly satisfied that this outer layer is not organic, but a mere encrustation or deposit of foreign matter, and that if removed, the prismatic layer would be found to be as thin as in the Nebraska - Ra specimen marked No. 2. The reason for thinking the outer layer not organic is that it shows no structure nor surface striæ, and has the costae even, less distinct than on the inner side.

I have put the two pieces of this valve in a cork. You can return it in a letter in the same way.

I also send for you Choronyta, glabra, Deinonyx, and Spinifer, fossils from you. Shumard. All are from the Upper Coal V.
Washington D.C. Dec. 16th 1867

Prof. Marsh

My dear Sir,

I have sent a little package of specimens to you in a package to the college library No. 17, which I would be obliged to you if you will look at as soon as it arrives. It will be sent today or tomorrow.

If my note on the shell structure of Avicentiprotus is not in type for the Journal will you please return it to me, so that I can modify it somewhat before it is printed? Yours very truly

F. B. Meek
Washington City Decr. 19th 1867

Prof. Dana,

I wrote Prof. Marsh a day or so since telling him that since sending on my note on the shell structure of Asiculepterus I had seen the same structure in Lima reticula of Sturmann, and this had rather shaken my confidence in what I had seen in the Asiculepterus, beyond doubt, an organic fossmatic structure, and suggested the inquiry whether it may not be a minutely crystalline structure? I sent an specimen to him, with the request that he might examine them and judge if the same, and to let me know whether or not you think what I have seen is crystalline structure. I at the same time requested him to send me the MSS of my note, if not already in type, so that I could modify it, somewhat.

This morning he sent me the proof of the note, and it is manifest had not received my package of specimens, when he wrote. I return the proof to you with a number of
Washington Dec. 25th, 1861.

Prof. Marsh:

My dear Sir—

I thank you for a copy of the 22nd Iris, with suggestions respecting the specimen of I. coquei. I am puzzled about the structure of some of these shells, yet in looking at them under the microscope I cannot get rid of the idea, that there is really a prismatic (organic) structure to be seen in some of them.

There is no doubt about the thick shell. Lord, being immersed, as you state. The portion of incrusting matter, removed from the edge of the anterior, leaving the smooth inner side exposed, I picked off with a point of a knife-jab before sending the specimen. These is, however, as I think, a thicker external inorganic encrustation. I hope you look at this specimen by transmitted light, looking upon the inner side. I mean under a 1/2 inch lens in a microscope. If you did, you must have observed a beautiful prismatic structure, rather dimly visible. This is seen through the thin inner encrustation, and not in it, for it is somewhat sharply defined in the smooth space from which I had removed it.
the interioration to which you allude, then elsewhere. What
makes me think there must be a thicker external
inorganic interioration is, that on looking at the
outer side, under the same power that shows the
prismatic structure from within, no such structure is
visible. From even by the aid of the strongest transmitted
light. From these facts, I think this shell, if entirely
demolished of the thick external interioration, and the thin
one within, would be little if any thicker than those I sent
you from Nebraska city, the indurated clay, and would
then show the same structure as the latter. If you
come on here we will look at these matters, and disy
them more thoroughly together.

I have not seen Rogers’ paper to which you allude,
and I am obliged to you for calling my attention
to it. I thank you also for your kind offer to
loan it to me. I believe, however, it is in the library
here. At any rate, I will not, just now, have time to
do anything in the way of examining shell structures,
as I am working very hard against time, in trying
to be ready to start to Illinois. I have written
to Wrighten that I hope to be ready to start about the
fiftieth of next month, but I may be delayed a few days
beyond that date.

My Report on the Upper Coal Measuring beds
of Nebraska, will include figures, and full descriptions
of all the fossils figured by Buxton, and many others,
including indeed most of the invertebrate remains of
the Western Upper Coal Meas. Mrs. St. John is
also preparing descriptions of the fishes. The Report will
include probably something over 100 species, all fully
illustrated. It happened to find an artist here, who has
died me very much. I have no doubt but Buxton
and Moscom, will reply to my criticism, with all
the force they can; but if I can get my Report of which
I speak, published, I am willing to leave the matter
for geologist to decide who is right, and who is wrong.

I am glad to learn you have decided to under-
take the work. I do not know exactly when I will
return, but I hope to be home by the last of
May.

The Commissioner of the General Land Office,
has recommended in his Report to Congress, that an
appropriation should be made for biological surveys
in several of the Western Territories, in which so much
public land remains comparatively unexplored, geologically,
He and others think there will be no difficulty in getting these appropriations, and I have had indirect intimations from the Land Office that some of these awards will be offered to me. All of my friends here are very anxious that I should accept if the offer is made, or rather that I should strongly, with all the backing I can bring as there will doubtless be several applicants, and political influences brought to bear upon the commissioner. Prof. Henry has voluntarily offered to give me any testimonials I may want, and to have an interview personally with the commissioners in my behalf. I scarcely know what to think of it. I fear, however, that I must do something that will give me more outdoor exercise, for as long as I am able to crawl about, I cannot keep from working and confining myself to the house when potatoes are brought in. One entire season’s field work in the mountains, and in crossing and recrossing the plains, would, I think, extend my lease of life.

Hoping to see you soon, remain,

Truly yours,

J. B. Meek